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THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Letters on the Use of Caustic for the Treatment of Strictures of the Urethra.* Edited by JAMES SYME, Esq., Professor of Clinical Surgery, University of Edinburgh.

MY friend, Mr Benjamin Bell, lately placed in my hands a packet of letters, on the treatment of stricture of the urethra, and more especially on the use of caustic for this purpose, which had been addressed to his grandfather, Mr Benjamin Bell, nearly fifty years ago, by various practitioners of the highest eminence at that time. It would appear that Mr Bell had intended to collect and publish a body of evidence which might determine beyond all question whether caustic was generally confided in by the profession, or was limited in its employment to the practice of a few individuals, who, by dint of position, connection, and unscrupulous statements, had succeeded in rendering it the fashion of the day. But although, as will be seen, the opinions which he received were no less decidedly expressed than entitled to confidence, from the respect due to their authors, the proposed publication did not take place, probably on account of the feeling which seems to have been entertained, that it would be attributed to hostility towards Sir Everard Home. This person having had the good fortune to become connected by marriage with John Hunter, entered upon practice under the auspices of that great man, succeeded him as surgeon of St George's Hospital, and inherited in the kind offices of influential friends the reverential regard which had been cherished towards his distinguished brother-in-law. When distrust began to be entertained as to the soundness of his practice, and even as to the accu-

racy of his statements, there was still felt an unwillingness to publish anything that admitted of being represented as an attack upon one so firmly entrenched within prejudices which are usually proof against the force of reason. Professional opinion, therefore, was allowed to pursue its course; and, as the truth gradually became diffused, the use of caustic for the treatment of strictures was banished from this country, if not completely, at least so nearly, as to render it comparatively harmless. But there is now no reason why justice should not be done to the respectable men who, it appears, resisted the delusion when its current was in full force, and, by their example, promoted the establishment of a more rational practice; while any delicacy towards the memory of Sir Everard Home would be quite thrown away, since Mr Clift's revelation of his having committed Mr Hunter's manuscripts to the flames, and the recent admission of his confidential assistant, Sir B. Brodie, that "in his latter years he certainly did commit the very great error of publishing some of Mr Hunter's observations, as if they had been his own,"[1] have placed him beyond the pale, not only of courtesy, but of pity.

I.—*Mr Benjamin Bell, of Edinburgh, to Mr John Pearson, of London.*

"6th November 1804."

In this letter, which is rather long, and somewhat diffusely written, and which, therefore, need not be printed entire, Mr Bell begins by stating, that having, some years before, published his opinion against the use of caustic for the treatment of strictures, and having done so in strong terms, from a desire to excite the attention of others to the hazardous nature of this practice, he still found that it continued to be the fashionable plan of management; although, in every instance of its alleged success, he believed that relief might have been obtained much more easily and safely by the use of "mild bougies." "I do not mean to say," he continues, "that bougies will cure every case of stricture; but I certainly think that caustic cannot be used *with safety to the urethra*, so as to remove any stricture that resists a proper application of mild bougies. Bougies, again, if they do not succeed, never render the patient worse; whereas the most dangerous consequences result from the use of caustic, such as excessive irritation, suppression of urine, fistulous openings, more permanent forms of stricture, and even death itself."

Practitioners, he says, seem to have been paralysed in expressing their opinion of this dangerous practice, partly from its having been patronised by Mr Hunter, whose memory they justly regarded, and partly from its being boldly supported by Mr Home through a constant reference to cases. These cases, he observes, had no weight

[1] Letter from Sir B. C. Brodie, "Lancet," 11th May 1850, p. 567.

with him; for, while cases given in confirmation of what is probable will gain belief in proportion to the credit that is due to those who relate them, no influence can arise from the greatest number of cases detailed in support of anything that is altogether opposed to the dictates of common sense. "When opposed by reason and experience," he adds, "cases can have no influence, and are read without interest, as mere delusions on the part of those who relate them, who frequently come to believe the most improbable things, merely from daily detailing them." After illustrating this position by reference to various popular delusions of the day, Mr Bell proceeds to express his persuasion that, under existing circumstances, nothing could prove more beneficial to the interests of society than a candid avowal of their opinions as to the use of caustic, by practitioners of reputation; and he, therefore, requests such a statement of his correspondents' sentiments upon this subject as may appear in his contemplated publication.

It is probable that a similar communication was addressed by Mr Bell to the other gentlemen, whose letters will appear below.

II.—*Mr John Pearson, of London, to Mr Benjamin Bell.*

"London, 28th November 1804.

"Dear Sir,—I do most sincerely regret that you were obliged to leave London before I could have the pleasure of seeing you. I was very desirous of conversing with you on the subject on which you have made so many judicious observations in the letter with which you have favoured me, and also on that of hernia, especially on the crural hernia, which has given rise to a good deal of discussion between my old master, Mr Hey, of Leeds, and several surgeons in London and in the country.

"I entirely accord with you in the opinion, that the method of treating strictures of the urethra by caustic, although useful in a very small proportion of cases, is a practice generally dangerous and unwarrantable, and ought by no means to be introduced into general use. When there is a point of irritable surface in the urethra, a transient touch of caustic may destroy the morbid sensibility, and it may be likewise employed in some cases of fistula in perineo with advantage. But I have always found, that by far the greater part of strictures may be cured with mild bougies; that when treated in this way they are not more liable to return than when treated with caustic; and, what is of very great importance, that the patients are not liable to suffer from a variety of unpleasant symptoms, which often continue during life where the caustic bougie has been freely applied. I have likewise found in my own practice, and have been informed by other surgeons, that where there is any considerable obstacle to overcome, the armed bougie is longer in producing the dilatation than the mild one; and, when the caustic has been repeatedly applied by Mr H. himself, I have known new strictures

produced, and an urethra partially obstructed has become absolutely impermeable during the use of the armed bougie. With a very small number of exceptions, I can manage every case of stricture by simple dilatation, without pain, without danger, and generally without difficulty. The use of caustic substances to overcome strictures of the urethra is, I believe, almost coeval with the invention of bougies themselves; indeed, I suspect that they were first employed either on account of some supposed virtue in their composition to act on the diseased part, or for the purpose of conveying medicaments of different qualities (often corrosive ones) to the seat of the obstruction. That they acted by pressure chiefly, was a discovery of modern date. The mischief they produced was a sufficient reason with our wiser ancestors to disallow their use; but in the present day it seems that injurious consequences and ill success are too feeble arguments to combat opinion and prejudice. The very open and polite manner in which your letter to me was conceived imposed it as a duty on me to be thus frank and explicit in my reply. I have always given my opinion on this point in my lectures without reserve. Although it would be flattering to me to appear in the same pages with you on this or any other surgical question, yet I am under the necessity of declining to write anything for the public at this juncture which would have the appearance of an attack on Mr Home. I am very glad to find my own opinions confirmed by the experience and judgment of so eminent a practitioner as yourself; and I shall wait with anxiety to see your intended publication.—I am, dear Sir, with great respect and regard, your most faithful obedient servant,

"JOHN PEARSON.

"Golden Square."

III.—*Mr Pearson to Mr Bell.*

"London, 27th December 1804.

"DEAR SIR,—Before I reply explicitly to the queries contained in your last obliging letter, I would premise, that no accident has ever occurred to me by the use of caustic to the urethra. I have indeed seldom employed it, and never with any remarkable advantage; hence it has been rarely resorted to in the course of my practice.

"The instances that have come to my knowledge, where it has done mischief, have been commonly related to me, either by the patient himself, or the surgeon under whose conduct the misadventure has happened. In this way I have been informed of several instances of retention of urine; but I know of none where it has been necessary to puncture the bladder, except the cases published by Mr Home himself. A profuse hemorrhage is the most common accident. I have known it so profuse, that the pulsation at the wrist has ceased, and during some hours it has been doubtful whether the patient could be recovered. The shiverings which succeed the ap-

plication of the caustic are often tremendous. They will continue during two or three hours, and seem to threaten the speedy dissolution of the patient. At other times the caustic brings on a state of irritation of the whole system, with symptoms resembling those of low fever. I received a very authentic account of the death of a gentleman from this cause, from the medical attendant who witnessed the whole process; and I know another case, where a gentleman was brought into a state of imminent danger from the same cause, and who probably owed his life to his family refusing to submit to any further application of the caustic. A very common consequence of caustic, is a permanent irritability and spasmodic state of the urethra, creating more trouble and uneasiness than the original disease. I have had several of these cases under my care, and have found them among the most troublesome diseases of the urethra. The patient is generally rendered uncomfortable during the remainder of his life. Abscesses in the perineum are not unfrequently produced by the application of caustic. The application of caustic will not uncommonly produce a complete obliteration of the urinary canal. I attended one melancholy instance of this kind a few years ago.

"I have never found any inconvenience from the use of mild bougies in the whole course of my practice; and where caustic had not been previously used, I have always employed them with success.

"I have now and then seen a hemorrhage to the amount of two or three ounces; and it is sometimes necessary to suspend the use of the bougie during a few days; but I have never met with any misadventure in public or private practice, and to the best of my knowledge, I have not failed more than two or three times of curing my patient effectually of his strictures. I always use the bougie myself, until the urethra will admit one of a large size with facility, and then I instruct my patient in the management of it for the future.

"I will now communicate an anecdote, which will prove how far some people's prejudices go in favour of caustic:—An army physician had the misfortune to rupture the tendo-Achillis partially. He became so lame, and was afflicted with such severe spasms, that he was confined to his bed during several months. Among other medical friends, Mr H. visited him, and suggested the probability of caustic applied to his urethra relieving the spasms. Although the good doctor had never been conscious of any obstruction in the urinary canal, he submitted to the operation more than once, without any effect, good or bad. His good sense, however, soon made him ashamed of his credulity, and he returned to the use of such remedies as were adapted to the nature of the local injury.—I am, dear Sir, with great respect and regard, your most faithful, obedient servant,

"JOHN PEARSON."

IV.—*Mr Hey, of Leeds, to Mr Bell.*

"Leeds, 15th November 1804.

"SIR,—My experience in the use of caustics for the cure of strictures in the urethra has not been extensive, but I have seen enough to satisfy me that the application of them is liable to some serious inconveniences. I have used both the armed bougie and the canula, but I approve of neither. I know that this method of treating stricture has been declared necessary, when the success afterwards attending the use of mild bougies has shown such prognostic to have been without foundation.

"I am truly sorry that the caustic bougie is so fashionable an instrument; but the mischiefs which follow from its application will, I am inclined to think, bring us back to the use of a safer remedy. Even the mildest bougies should be used with great caution for the removal of considerable strictures, especially when they are seated in the membranous part of the urethra. The weakness of its coats, and the curvature of its canal, in that part, may cause the bougie to form a false route, unless the point of the instrument is carried forward with great gentleness. We have cases upon record which demonstrate the necessity of this caution.

"When the urethra is extremely straitened, so as to give the sensation of a total obstruction upon the introduction of a small bougie, I have found it to be an advantageous method to suffer the point of the bougie to remain in contact with the stricture during an hour or two every day for some time, without attempting to push it through the straitened part. By gradually increasing the size of the bougie, the stricture may be lessened before the point of the instrument is made to pass through it. This cautious procedure causes the urethra to suffer less irritation, and exposes it to less danger of being wounded by the point of the bougie, when attempts are made afterwards to pass through the stricture.

"I think myself obliged to you for informing me of your intended publication; and I sincerely wish that your arguments in favour of mild bougies may be successful in restoring a method of practice which, I am persuaded, will in the end be found to be the most safe and effectual.—I am, Sir, your obedient servant,

"WILLIAM HEY.

"*P.S.*—If you think the preceding remarks can in any degree contribute to the success of your arguments, you are at liberty to insert them in your intended publication. The concurrent opinions of surgeons, who are totally unconnected, have undoubtedly some weight in the establishment of any mode of practice, otherwise I should esteem any other observations of mine to be quite unnecessary in aid of your reasoning on the subject."

V.—*Mr Ingham, of Newcastle, to Mr Bell.*

"Newcastle, 11th November 1804.

"DEAR SIR,—In my practice I have always used the common bougie in strictures of the urethra, with only two exceptions. In these I was induced to use the caustic bougie, from the wish of the patients to have the disease speedily removed, but after a few applications they were glad to have the caustic bougie discontinued, and their relief procured by resuming the use of the common bougie. In the one, the pain of the caustic was excessive; in the other, after each application, a painful stillicidium urinæ continued some hours. The use of the common bougie, if regularly and patiently persisted in, will, I am satisfied, even in very distressing cases, procure great relief. In one case, where the patient passed all his water by the rectum, the common bougie overcame two strictures, and enabled him to pass his water naturally. Another case, of an elderly gentleman who, for upwards of forty years, had made his water frequently, and with difficulty, in a very small stream, was also perfectly relieved, and he had the comfort of retaining his water the ordinary time of a person in health, and passing it with great ease, which he still continues to do, at the distance of two years since he was under my care. You will observe I employ the word *relief*, not *cure;* my reason for so doing is, I don't believe a perfect cure is to be obtained by any method, for I am of opinion, when a stricture has been *once* formed, there always remains a disposition in the urethra to reproduce the disease, if the patient, after he has found relief, is not attentive to pass and wear a bougie half an hour or so once a week, or oftener, according to circumstances; and this I find is also necessary to be attended to by those who have received relief from the caustic bougie. Some of my friends, after supposing themselves cured by the caustic bougie, under the care of other professional gentlemen, and not thinking it necessary to introduce a common bougie occasionally, have, at different periods from their supposed cure, been again attacked with stricture, and in a few instances the recurrence of stricture has been attended with suppression of urine. The common bougie has, with me in general, answered perfectly well; yet cases may arise where more active measures may be required; but from my own experience, I am inclined to believe that these will, in fact, be very few.—I am, dear Sir, yours faithfully,

"WILLIAM INGHAM."

VI.—*Mr Park, of Liverpool, to Mr Bell.*

"Liverpool, 7th December 1804.

"SIR,—That the letter with which you honoured me some weeks ago has remained so long unanswered, cannot perhaps be so justly attributed to pressure of professional engagements, as to a consciousness of inability to furnish you with any communication on the sub-

ject of it worthy of your acceptance. It is one on which I have had very little experience, having early conceived such doubts of the safety of using caustic bougies for strictures in the urethra, as have almost wholly restrained me from adopting them when I could possibly obtain a passage by more gentle means.

"My objections to them have been,—the difficulty of ascertaining with precision whether they were destroying more of the diseased than of the sound parts,—a conviction that few cases will ever occur in which a stricture can be so far radically cured by any means as to render it unnecessary to introduce a bougie occasionally during the remainder of the patient's life,—that the majority of patients will neglect to do this in spite of every warning that can be given them,—and that if, in consequence of such neglect, the stricture should return after a portion of the canal has been destroyed by caustic, it must be more callous and obstinate than it probably would have been if such means had never been employed. Of this I have seen one melancholy instance, in a gentleman's servant, who had a stricture destroyed by caustic by a surgeon who told him he was cured, and would have no further need of bougies. In consequence of this, it did return under more aggravated circumstances, occasioning suppression of urine, considerable abscesses, and complicated fistulous sinuses.

"In consequence of these doubts, I have hitherto only used caustic bougies in three cases. Two of these were strictures which I could not overcome by simple distension, in the time which I could prevail on my patients to persevere. In these the result was tolerably satisfactory; in one I obtained a passage for a middle-sized bougie in three or four applications; in the other, after eighteen or twenty applications, without any unpleasant symptoms, except two slight hemorrhages, not such as to occasion any alarm, and which seemed rather to produce beneficial effects.

"The third was a case in which I made three or four applications of caustic, with a view to relieve excessive irritability of a stricture, which had been dilated so as generally to admit a very large bougie, but remained frequently subject to strong spasmodic constrictions. Of the result of this I cannot yet inform you, having for some time lost sight of my patient. Whether further experience will increase or diminish these doubts, time must determine. If I have suffered them to make too strong an impression on my mind, I remain perfectly open to conviction.—I remain, Sir, yours very respectfully,

"H. PARK."

VII.—*From Mr Chilvers, of London, to Mr Bell.*

"New Burlington Street, 24th December 1804.

"DEAR SIR,—I have received your letter through Sir Walter Farquhar, and am happy to hear of your intention of again going into the press. It is much to be wished that you may have leisure

to give to the world the result of your observations and long experience in every branch of surgery. In regard to the object of your immediate attention, it is not in my power to give you any information that would be worth your acceptance. You must overrate the extent of my practice in the treatment of strictures by caustic.

"During the last twelve months I have scarcely used it at all, and I suspect that many other practitioners are become as timid and cautious in the employment of it as I am.

"I have seen it do a great deal of mischief; and the perusal of Mr Home's work has shown me, that even in the most expert hands, the armed bougie is not always used with impunity. I am not prepared, however, to say that no case of stricture can occur in which it might be employed with safety or advantage. The practice, as far as I am able to judge, is declining; and your work will, I doubt not, give the death-blow to it. At all events, in the collision of such talents, something must be struck out favourable to the cause of truth and science. I shall be extremely anxious for the appearance of your book, and anticipate the success of it with perfect confidence. —I have the honour to be, my dear sir, your obliged and faithful humble servant,

"S. CHILVERS."

VIII.—*Dr Hare, of Calder Hall, formerly of Calcutta, to Mr Bell.*

"Calder Hall, 23d December 1804.

"DEAR SIR,—I am this moment favoured with your letter, which certainly requires no apology; on the contrary, it will afford me the most sincere pleasure if any information I can give can be of use to you in treating a subject of very great importance. I will answer your queries in their order. 1. The gonorrhœa is a very common disorder among the natives, but they suffer very little pain, and often little inconvenience from it. 2. They never use injections when left to their native doctors, but use infusions of what they call *cold* medicines, and regulate the diet, confining themselves to mild fruits and mucilaginous plants, of which they generally make a secret. With these simple means the disease, in due time, disappears, which you and I might be apt to ascribe to nature rather than to medicines. 3. They are very seldom liable to stricture; indeed, I do not at present recollect a single case of a native having a real stricture who had never been under the care of a European surgeon for a gonorrhœa. And this observation often led me to suspect that injections imprudently used were the chief cause of the frequency of strictures. A young practitioner has not always resolution to resist the importunity of his patient to cure him speedily. And I have more than once been called in, when I thought it necessary to endeavour to bring back the gonorrhœa as the only means of removing more dan-

gerous symptoms. 4. I have never known a native surgeon use a bougie who had not learned the practice among us. In cases of suppression of urine, they sometimes try to stimulate the urethra by introducing different substances a little way into the passage, but none of them have the least idea of removing a natural obstruction by mechanical means or by caustic.

"I cannot express to you the satisfaction I feel, to find that your opinion on the subject of the treatment of strictures coincides so perfectly with what was my practice for many years. The complaint is so common in India, that many people thought that few who had been long in India were entirely free from it. Such, I know, is the language in London of some eminent surgeons, and this opinion, propagated in India, is often a source of much uneasiness and anxiety. You know well that there is a fashion in medicine as well as in dress, and caustic in this way became the invariable remedy in every case where there was the smallest difficulty of passing the urine. The consequences were often very dreadful, and even fatal; but still the caustic got credit, as it seemed to remove an obstruction which had been supposed to exist, but which I thought was merely spasmodic from an attack of fever. As I always avoided every kind of manual operation, though often obliged to be present, and assist with my advice, I can with great confidence declare that even in cases where real strictures existed, and where it was used with every precaution, yet I have seldom seen it useful, and often most prejudicial, and so much so that for several years I never gave my consent to its being used, and had but too frequently occasion to have my opinion confirmed by the fatal consequences that ensued. One of the sufferers was an intimate friend of my own, who, during my illness, was persuaded to try it, and a mortification was the consequence.

"In some measure to account for this seemingly frequent occurrence of strictures in India, I must remark that the bilious remittent fever, which is so common and so severe in that country, is very apt to recur from trifling causes, such as exposure to damp, or to the sun, or merely from an accumulation of bile. These attacks, even when so trifling as scarcely to deserve the name of fever, are very generally attended with a difficulty of making water, sometimes with a total suppression, especially in those who have before suffered from gonorrhœa. I have known many who have felt this so often, that they constantly carried a bougie about with them in case of feeling it coming on. In every case I discouraged this, as being attended with risk, and unnecessary; but the fashionable doctrine of stricture here once established, it became common to use caustic, although the facility and full stream with which the water was passed at other times ought to have convinced surgeons that no permanent stricture existed, and that the fever was the cause of the suppression, and not the consequence of it. Gentlemen coming home from India by a long sea voyage get rid of this habitual fever, and with it of the frequent attacks of dysuria, and are taught to ascribe

this to the caustic which the London surgeons, particularly Mr Home, never fail to apply, with the assurance that it will cure the fever which had harrassed them so long.—I am, dear sir, your obedient servant,

"JAMES HARE."

The letters which have now been printed are so creditable to the good sense and feeling of their respective authors, that I trust the reader will not think Mr Bell or I have done wrong in making them public. It is, fortunately, not necessary at present to take the field in force against the use of caustic. But as it is impossible to tell what follies may turn up in the course of time, and the revolution of professional opinion, it may not be without advantage to record the sentiments upon this subject, which, when the practice was in the highest degree fashionable, were entertained towards it by some of the wisest and ablest practitioners Great Britain ever produced.

Since the introduction of metallic instruments instead of the elastic gum and plaster bougies, formerly in use, the treatment of stricture by simple dilatation has been rendered much more efficient; but still, as Mr Bell says, and his correspondents seem to have felt, cannot be regarded as equal to the cure of every case. It was for the relief of those obstinate and contractile strictures which resist the bougie, that I some time ago recommended external incision upon a grooved director, conveyed through the seat of contraction,—on the ground of its being absolutely free from danger to the patient's life, certain to afford complete relief from all the symptoms of the disease, and probably sufficient in general, if not always, to protect him from future inconvenience. Knowing the serious, and too frequently fatal, consequences of contending against such strictures by vain attempts to dilate them, and believing that the operation thus proposed was calculated to lessen a large amount of human suffering, I was little prepared for the storm of abuse and misrepresentation which burst forth against me in the atmosphere of London surgery. I knew that in that city there were some shoulders well fitted to wear the tattered mantle of Sir Everard, and I was not so sanguine as to look for any speedy improvement in the practice of men whose sole object is to squeeze money out of their patients; but I certainly did not anticipate the extraordinary effort that threatened to overwhelm my well-meant proposal with disgrace.

This was neither more nor less than publishing the deaths of various patients, who were alleged to have fallen victims to it in London; and when statements to this effect were not only printed in the medical journals, but discussed in medical societies of the metropolis, without a single pen or voice being raised to question their accuracy, there seemed every prospect of matters continuing to go on in the old way. But at length the astounding fact was divulged that in all of these fatal cases the operation performed had not been

the procedure I recommended, but one entirely different, against which I had protested in the strongest terms, as pregnant with the most dangerous consequences. I shall leave the parties concerned in this transaction to meditate on the very peculiar position in which they have placed themselves. On looking back into the records of surgery, they will, so far as I know, find only one precedent for their conduct, which, though not precisely parallel, inasmuch as it was not so palpably unfair, may be quoted for their instruction. When Cheselden performed his lateral operation with a simple knife and staff, the success he met with gave great offence to some ingenious gentlemen, who had been labouring all their lives in the contrivances of complicated apparatus for the purpose. Of these, M. Le Cat, being especially offended, determined to extinguish the presumptuous innovation by an argument which should admit of no dispute, and which may be related in the words of Mr John Bell:—"There wanted but one argument more to complete the proofs of Le Cat's doctrine,—viz., that he should be able, after so many bad arguments, to adduce some unlucky operations done with the knife. This was perfectly easy: he had but to make the experiment himself, and with his own knife; and even this he condescended to do in compliment to his favourite theory. It will be recollected that his kind master, Morand, had, after his return from England, put the knife of Cheselden into his hand; but Le Cat refused his instructions. He did, indeed, perform the operation of Cheselden—unwillingly and awkwardly, no doubt—on nine patients in the Hôtel-Dieu. Of these nine patients, six died; and this infatuated man ventured to proclaim publicly the ill success of his own mal-adroit operations, as a proof of the danger of wounding the neck of the bladder! There was no contending with one who could bring such damning proofs."[1]

ARTICLE II.—*Turning Successful in a Case of Fibrous Uterine Tumour occupying the centre Cavity of the Pelvis, and subsequent Expulsion of the Tumour.* By D. R. RANKIN, Esq., Surgeon, Carluke.

THE attention of accoucheurs has recently been directed by the leaders of the profession to turning, in certain cases of small pelvis, to avert the awful expedient of destroying the child by craniotomy, on the ground that the base of the cone-shaped cranium offers less width than the arch of it. Reasoning from a rule in nature, one would be disposed to question the accuracy of the doctrine, that the head of the child, with the vertex presenting, offers greater resist-

[1] John Bell's "Principles of Surgery," vol. ii., p. 76.

ance, or is less easily moulded to the form of the pelvis by the efforts of nature, than the base of the cranium. Does experience teach that the base of the head offers less resistance than the vertex in passing through the outlet of the pelvis?

Mrs G. is twenty-seven years of age, and the mother of five children. The first two births were natural; but in the third (24th August 1846) some difficulty, I learn, arose in the early stage, to the probable cause of which her subsequent history points.

At the fourth birth (31st March 1848), the difficulty became too apparent. After watching the efforts of nature for thirty-six hours, and finding that though the head had entered the brim of the pelvis, an insuperable obstacle hindered its further progress, Dr Selkirk, whose case it was, requested my advice and assistance.

A large tumour was found to occupy the right hypogastric region, producing displacement of the uterus; and, on examination, the vagina was found to be depressed, as well as compressed, and thrown upon the left side of the pelvis by the tumour, which thus occupied the greater part of that cavity. By the ordinary mode of examination, the uterus could not be reached; but, on introducing the hand, the head was found free of the uterus, although completely retarded from farther progress by the counteraction of the impending mass.

As there seemed no alternative, perforation was resolved upon, which, from the little mobility and incompressibility of the tumour, was not easily accomplished; but the ultimate extraction of the child was effected, after tedious, difficult, and careful manipulation with the crotchet.

When a subsequent pregnancy came to the knowledge of the medical attendant of the family, he naturally felt anxious. Encouraged in the attempt, the induction of premature labour was tried by him in the seventh month; but it could not be accomplished, the uterus being displaced, and beyond reach.

On the 1st of February, and again after an interval of some days, I examined Mrs G., with the aim of determining how her case was to be met when the urgent hour arrived. The prospect was most unpromising. The recollection of former difficulties, with the certain knowledge that the obstructing agent was more formidable than before, made me look forward to the crisis with a feeling of awe. To lessen the burden of responsibility, and in order that the patient might have the advantage of the best advice, Dr Wilson (of Glasgow) was consulted on the 9th, and through him was secured the advice of Drs Watson and Macfarlane.

All concurred in viewing the case as one of great difficulty, involving the probability that the Cæsarean operation might, of necessity, be resorted to, should turning fail. Efforts were made to secure the consent of the patient to remain in Glasgow, under Dr Wilson's care; but she declined, and reluctantly I had to undertake the case.

On 25th February, labour pains came on. Dr Selkirk had been in attendance about twelve hours, during which time the pains were not urgent. In a note I had from him that night, at nine o'clock, after intimating the onset of true pains, he stated,—"The tumour feels as hard and unyielding as ever. The passage of the child will prove an impossibility." On examination, two hours afterwards, the tumour was found to occupy the entire cavity of the pelvis, and was acted upon and pressed down by every effort of the womb. The presenting parts could not be reached by the ordinary mode; but on introducing the hand, to ascertain how far the tumour could be displaced upward, it was found obedient to pressure to some extent; and, by passing the hand onward, on the left side of the pelvic cavity, against which the vagina was forced, a small bag of the protruding membranes was felt; but, unwilling

to rupture them at this stage, and as it was otherwise impossible to reach the womb, I resolved, in absence of any urgent symptom, to wait till the distended membranes exercised somewhat longer their relaxing influence. At half-past one o'clock on the following morning I introduced my hand, moulded the passage by acting on the tumour through the walls of the vagina, and passed the hand up carefully till I became sensible of the presenting parts, through the well-dilated orifice, which was situated above the edge of the left ilium. The liquor amnii was now evacuated ; the hand passed into the womb, and what proved to be the left foot was secured. Very considerable force was necessary to bring the child into the pelvis. During the operation, the tumour was gradually forced up from the cavity of the pelvis into the cavity of the abdomen, and a sensation of tearing was communicated to the hand, and rendered audible, which led me to conclude that some fearful disaster was impending. This was soon found to be referable to the parts of the mother ; for the child was delivered uninjured, though in a state of asphyxia, from which it rallied within ten minutes.

The placenta was thrown off within fifteen minutes, the uterus contracting regularly and satisfactorily above and partially behind the tumour.

A train of symptoms, by no means promising, followed. There was acute pain in the uterine region for several days, much sleeplessness, and occasional delirium. A fetid discharge followed, and a smart attack of erysipelas of the face and head came on. On examination on the seventh day after delivery, the maternal canals over the most prominent part of the tumour were found to be in a sloughy condition. A few days after, a small projecting mass was found in the seat of the slough, and fetid parts constantly escaped, the nature of which was not ascertained. On 3d April—the thirty-eighth day after delivery—the patient informed me that something protruded from the vagina ; and with little difficulty, the parts being all but free, I extracted a partially putrid mass, of flattish form, ten inches long, five broad, and weighing fourteen ounces,—consisting of a dense fibrous substance, fringed here and there with small cells, emptied of their contents.

The diagram (see Plate) is a fair representation of the position and relative proportion of parts; and it may well be conceived that in bringing a child of ordinary size into the cavity of the pelvis, by forcible means, against such resistance, the tumour would be acted on with very considerable power. The rupture of its adhesions or parts was heard and felt; and, during the passage of the child, the tumour was dislodged from the pelvis, and forced into the abdomen. In all probability the blood-vessels were, amid so much violence, torn, and hence the death and ultimate expulsion of the tumour in a flaccid and semi-putrid state. Small as it appeared comparatively, it is easy to form an idea of what the tumour would be while rigid, with its circulation and cells entire.

Nothing could have warranted the practice pursued in this case but the direct necessity, and the dread of attempting the Cæsarean section.

The treatment has not been alluded to ; careful nursing comprehended all.

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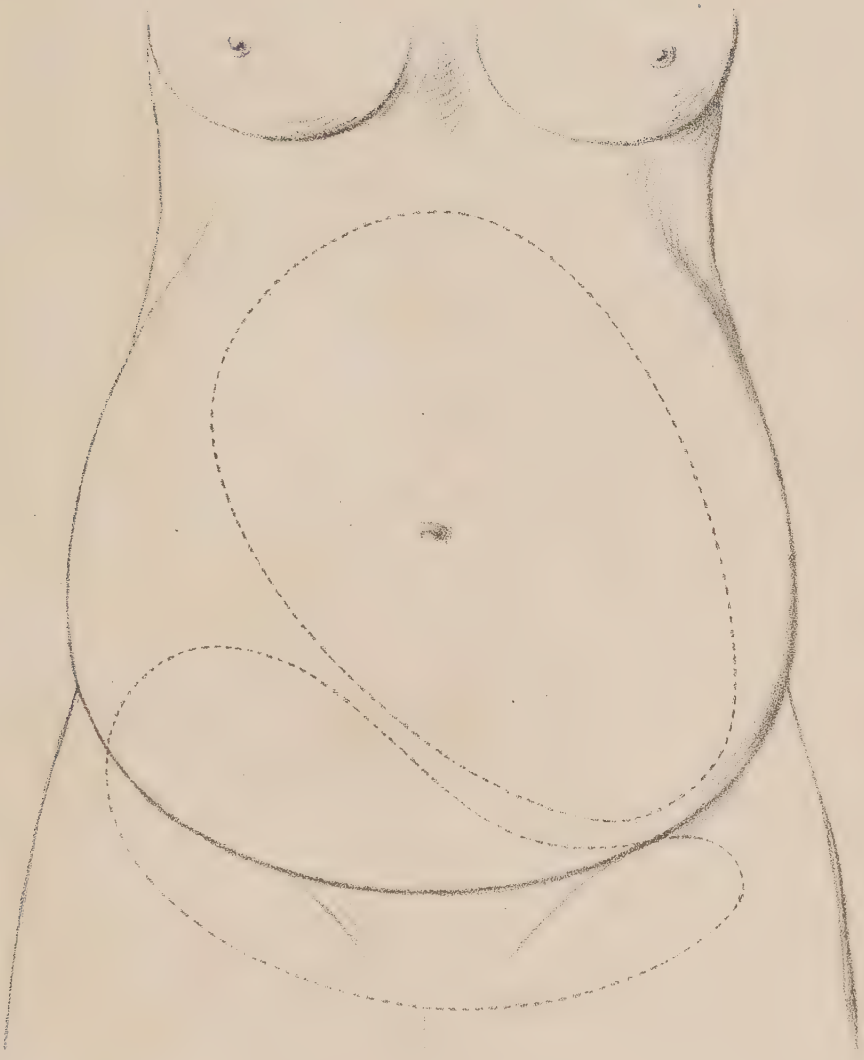

Schenck & Mc Farlane, Edin.r

Mr D. R. Rankin's Case

ARTICLE III.—*On Disease of the Larynx.* By JOHN SCOTT, M.D.

(*Read before the Medico-Chirurgical Society, 15th June* 1850.)

MY object in the following observations is to call the attention of the Society to a subject of great practical importance, which does not appear to me to have obtained that notice from the profession which it well deserves. I allude to the cauterisation of the larynx and pharyngeal cavity in cases of follicular disease. An excellent work on this subject was published in 1846, by Dr Horace Green, of New York, in which he describes, in a plain and lucid manner, the application of the topical remedy, and the diseases to which it is applicable. Physicians have for many years been in the habit of applying caustic to the throat, tonsils, and uvula, in the solid and liquid form, in cynanche, scarlatina anginosa, tonsillaris, and other acute diseases. The first person, however, to apply the caustic solution to the larynx itself, was our very able countryman, Sir Charles Bell. In his "Surgical Observations," he relates an interesting case, where the solution was applied in an ulcerated state of the glottis, when the patient was threatened with suffocation, and with a successful result. Another case, similarly treated by Sir Charles, is referred to by Dr Watson. A full account of these cases will be found in the last volume of "Forbes' British and Foreign Review;" and also a description of Dr Vance's practice, in what he calls *swabbing* the glottis, by means of the finger of a glove dipped in a caustic solution.

In 1836 Trousseau and Belloc described the cauterisation of the larynx in laryngeal phthisis, but they seem to have used it with hesitation and reluctance, and not to have appreciated the full advantages to be derived from it, preferring, in most cases, the injection by means of an ingenious syringe. It is to Dr Green, therefore, that we owe the proper application of this treatment to a common and very troublesome form of disease, which he was the first to describe and properly discriminate. The disease alluded to is denominated by Dr Green follicular disease of the air-passages, as it consists of a diseased condition of the glandular follicles of the mucous membrane of the throat, larynx, and trachea. The structural changes are—inflammation, which may terminate in ulceration, hypertrophy, induration, or a deposition of tuberculous matter in the follicles themselves. This, from its frequency among the clergy in America, as well as in this country, has been denominated the clergyman's sore throat. It is a form of disease which we have all occasionally met with, and experienced great difficulty in treating. I have always been in the habit of employing the nitrate of silver, either in a solid or liquid state, or the sulphate of copper, but by overlooking the diseased state of the epiglottis and larynx, the cure was rendered more tedious and unsatisfactory, and in many cases the affection was considered as incurable, and mistaken for some more serious disease.

Dr Green's plan of treatment consists in cauterising freely the whole diseased surfaces by a strong solution of crystallised nitrate of silver, generally from forty to sixty grains to the ounce of distilled water. The tongue is depressed, and the epiglottis brought into view by means of a jointed spatula, of which the most convenient forms are those now exhibited.[1]

The solution is applied by means of a curved rod of whalebone, to which a small piece of sponge is firmly attached. In many cases it is sufficient to cauterise the throat, and to press the sponge against the orifice of the glottis, without attempting to pass it into the windpipe. In other cases, it is necessary to pass it fairly into the superior aperture, and down to the chordæ vocales. This is in general accomplished with little difficulty. I am aware that the possibility of doing this has been denied, but I can only say, that I have done it repeatedly, and found it to cause far less irritation than might have been expected. To accomplish this object, the sponge is carried steadily, without touching the pharynx, behind the erect epiglottis, drawn slightly forward, and pressed steadily downwards, when it readily slips into the glottis, which is known both by the feeling of grasping, and the spasm immediately induced in the patient. The sensation is peculiar, though not painful in general. By causing the patient to inspire fully, the operation may be facilitated. The spasmodic action squeezes the fluid from the sponge, and it is thus freely applied to the diseased surface, causing an immediate and profuse discharge of mucus. It is always advisable, before passing it into the glottis, unless in very urgent cases, to apply the sponge two or three times to the throat and fauces, to educate the parts, as Dr Green expresses it. Sometimes a difficulty arises from the swelling of the epiglottis or aperture; a few applications will remove this, and then the sponge can be passed without difficulty.

It occasionally happens that the spasm is severe at the moment, but this soon passes off, and I have never seen it followed by any disagreeable symptoms. The disease to which this mode of treatment is applicable, is so fully described by Dr Green, that it is superfluous for me to enter into much detail.

For more particular information I must refer to Dr Green's book, or to the very able analysis of the work contained in the last volume of Dr Forbes' "Review," or to that in the last number of the "Monthly Journal." I may remark that, when this paper was written, the only public notice of the practice was contained in the first-named journal, the discontinuance of which, and the retirement of its able editor, I consider as the greatest loss the medical profession has sustained for many years. I shall not refer to the application of this mode of treatment to croup, as no case of this kind has fallen under my own observation. A work on this subject has been pub-

[1] These can be procured from Mr Young, surgeon's instrument maker, Princes Street.

lished by Dr Green, where the success has been such as to warrant the operation in this very formidable disease. The same remark applies to hooping-cough, in which this treatment has also been said to be successful. The state of the tonsils and uvula ought always to be attended to, and for these, when seriously affected, excision is the only effectual remedy. There are many cases of acute disease to which this treatment may be applied, and, as already mentioned, English physicians have long been in the habit of applying the caustic to the throat in such.

In diphtherite, by the free application of caustic in solution over the whole surface, the inflammatory crust is destroyed, and a more healthy surface remains. The repetition of the application two or three times a-day is required. In scarlatina anginosa I have used the sponge with good effect.

The following case, furnished by my friend Dr Brown, may illustrate its application to acute diseases :—

Case I.—J. S. S., æt. 43, strong, active, and healthy, had a severe attack of erysipelatous sore throat, which required vigorous treatment, local and general. He was recovering, when I was sent for, the message being that Mr S. was dying. Meeting fortunately with Dr Scott, he accompanied me. The patient had all the appearances of imminent death; his face expressive of intense terror and anguish; the extremities cold; the pulse hardly to be counted, from its rapidity and weakness; the breathing all but impossible, apparently from some affection at the top of the wind-pipe; the voice was gone. On looking deeply into the throat, the pharynx and top of larynx were seen of a deep red. The patient being too weak for blood-letting, and too ill for any slower means of relief, Dr Scott applied the solution of the nitrate of silver, which he happened to have with him. He got the sponge completely into the larynx. Mr S. almost instantly expressed by signs his relief. In the evening he could speak a little, and was able to lie down in bed, and was, in all respects, better. He was blistered, and had calomel and opium. Next morning he was much better. The sponge was again applied without any difficulty. He recovered rapidly, and has been for more than two years in perfect health, attributing, without any hesitation, the saving of his life to the sponge and the caustic.

The issue of this case was fortunate, yet I fear we cannot be always so certain of success, and must combine the practice with general treatment. Leeches, antimony, calomel and opium, or bleeding, are frequently required. In the early stages of *diphtherite*, where the exudation is commencing on the tonsils and posterior fauces, I have seen the solid caustic, or the solution, arrest the disease. In the very fatal epidemic diphtherite, where sudden depression of the vital powers and death occur in a very short period, I have not had an opportunity of trying its effect. The disease, however, for which the treatment is most usually required, is follicular disease of the throat and larynx, which is a very common affection, and so frequent, that it is surprising how little attention it has attracted. It is met with in all classes, more perhaps among public speakers than others, and more frequently in males than in females. It is singular that it occurs less frequently among barristers than any other class

of public speakers; this may be owing to two causes: their not expending so much emotion and nervous energy, and their not being obliged to vociferate so much as clergymen. From my own experience, I should have considered that body of men as exempted, had I not seen instances in the last few days.

The disease may continue for months or years without exciting much attention. In some cases the patient's notice is first called to it by hoarseness or partial loss of voice.

CASE II.—Mrs F., æt. 32, came under my charge from the country about a year since, with almost complete aphonia. The general health was also considerably affected. There was general constitutional disturbance, with quick pulse, and furred tongue. Means were taken to remedy this state, and as the throat was found granular, the caustic was freely applied. In three weeks there was great amendment, but as the aphonic state continued, the sponge was passed into the glottis with good effect. In this case the amendment was slower than usual, but progressive, and when she left town the voice was almost restored.

There are certain cases of *aphonia* in which, even when it may have originally arisen from follicular disease, or have been connected with it, this treatment is not successful. The loss of voice may depend on various causes, on some nervous affection, or on relaxation of the *chordæ vocales*, and in such cases the *aphonia* still remains when the original disease is removed. It is usually cured by the employment of various tonics and antispasmodics, but is very apt to return. Some epidemics of influenza are remarkable from the frequency of cases in which the voice is affected; and during the past spring I have had frequent opportunities of confirming this observation. Indeed, Dr Green considers that the disease became prevalent in America after the influenza.

In the usual cases it may be observed that there is very little cough. The throat is sometimes red, swollen, and granulated; at other times, pale, relaxed, and covered with indurated tubercles. The first symptoms noticed are usually hawking and constant spitting, some alteration in the voice, hoarseness, and loss of tone, increased by speaking or exertion. There is also some uneasy sensation in the throat, and a constant inclination to swallow, and sometimes there is difficulty in swallowing; this state may continue a long time. On inspection, the throat presents a granulated appearance, the mucous follicles enlarged, and covering the upper part of the pharynx; in the advanced stage these become indurated, and extend into the larynx. "As the disease advances," says Dr Green, "and the follicles situated at the root of the epiglottis, and in front of the arytenoid cartilage, and the still more numerous glandulæ of the laryngeal mucous membrane, become involved in the morbid action, all the above symptoms appear greatly aggravated; the hoarseness is much increased, and is constant; speaking or reading aloud is attended with great difficulty, and when continued for any period, is followed by pain and increased soreness in the region of the larynx, and by a sensation of extreme languor, not only about the vocal

organs, but throughout the whole system. In some cases, where the disease affects the glands situated in the ventricles of the larynx and near the vocal chords, the voice becomes completely extinguished; and if, by great effort, the patient essays to speak aloud, the vocal resonance is uneven, harsh, and discordant." By the application of the solution of crystallised lunar caustic, cases of the first class speedily recover. When the larynx is affected, we find that after the granular state of the posterior pharynx is removed, there still remains a feeling of soreness in the throat, hoarseness, and difficulty of swallowing. It is then necessary to have recourse to the cauterisation of the larynx in the manner already described. In my own practice I have often found some constitutional signs accompanying follicular disease, and have been led, in many instances, to consider it as an indication of the state of the mucous membrane of the stomach or bronchial tubes. In other cases it is purely local, and requires no particular treatment. In the early stages of phthisis, it is of great consequence to attend to the condition of the throat.

Follicular disease is often accompanied with cough and slight expectoration,—sometimes mucous, sometimes purulent. In some cases phthisis is suspected, and gives rise to great and groundless alarm. In such a state of matters we must be guided by the stethoscope and percussion. When the chest is sound and the larynx diseased, the symptoms are referable to the latter organ, and disappear under proper treatment. The following is one of these supposed cases of phthisis:—

Case III.—Mrs. C., æt. 23, had suffered from influenza and bronchitis, from which she slowly and imperfectly recovered. There was occasionally a slight appearance of blood in the morning, cough, slight emaciation, febrile accession towards night, and great debility. The tongue was furred, the pulse quick, and the appetite defective. The chest was found healthy on examination, but the throat was swollen, red, tuberculated, and the epiglottis swollen. General treatment was resorted to, and the throat and epiglottis touched every second day. In a fortnight the symptoms were almost gone, and the patient left Edinburgh quite recovered.

I have also met with another form of the affection, of which the following is an example:—

Case IV.—Mrs S. had been for some years affected with hoarseness, loss of voice, and great difficulty of breathing in damp weather, so much as to prevent her lying down. The cough came on in severe fits. At times, in warm dry weather, she was tolerably well, but liable to relapse in spring and winter. In this case there was tubercle of the throat, and slight ulceration of the epiglottis, apparently extending into the larynx. There was some difficulty in managing the passing of the sponge, and the treatment required it to be applied eighteen times. She left for a milder climate.

Dr Green, in mentioning the causes of follicular disease, considers dyspepsia as a consequence, rather than as a cause. This is contrary to my experience, as in many instances there is dyspepsia and general derangement of the chylopoietic viscera. It may frequently,

also, be traced to mental anxiety, over-study, or neglect of hygienic remedies. In all such cases the general health must be restored by sarsaparilla, iodine, quinine, and other remedial means, —change of scene and relaxation from business are often essential. I have generally found it necessary to give a mild mercurial alterative, with some alkaline and saline medicine, for a week or ten days, at the time the local treatment was going on. In other cases, however, the general health is quite unaffected.

CASE V.—Mr C. had held a situation, which required much public speaking. About two years since the voice became hoarse and harsh. There was no cough or difficulty of breathing; a slight expectoration of mucus; pain above the sternum and under the clavicle ; general health good. On inspection, small ulcers were discovered in the throat, and lower down, the fauces were denuded of epithelium, white, and covered with a thin muco-purulent matter. The epiglottis was also thickened. On the first application of the caustic—which, however, did not touch the epiglottis—the patient felt relieved. On the second occasion, next day, I passed the sponge fairly into the glottis, without causing much irritation.

CASE VI.—One of the most interesting cases of the kind I have seen, occurred quite lately. The patient was brought by my friend, Mr Carmichael, who stated that, for more than three years, he had been liable to paroxysms of coughing, so severe as occasionally to suspend respiration, the lips becoming blue, and the patient falling down insensible, and not recovering for some time. From the violence of these fits he had been obliged to give up his business as a merchant. Every means his medical advisers could suggest were tried, without effect ; and no progress had been made even in alleviation. The throat, on inspection, was found slightly tuberculated in the upper part ; lower down it was seen to be in a state of ulceration, while the epiglottis was also found swollen and ulcerated. On the first application the caustic was passed into the larynx, and a fearful paroxysm of croupy breathing came on, followed by profuse expectoration, and subsequent relief. I have now applied the sponge five or six times, a little more cautiously than at first, but still into the larynx. After the third application, he informed me that he could swallow soup, which he had not done for some time. Sickness and vomiting generally followed the taking of food, but he can now, after three weeks' treatment, take any kind of food, and has gained four or five pounds in weight,—the best indication of improved health ; and I trust his future progress will be equally satisfactory.

One remarkable effect of the follicular disease is the extreme mental depression which accompanies it, and a great feeling of debility and weakness. As the amendment in the throat progresses, the mind resumes equanimity, and the strength is restored.

I may likewise allude to a very interesting case, in which I had recourse to this practice with great temporary relief :—

CASE VII.—A late celebrated artist had been, for a long series of years, affected with hoarseness, occasional loss of voice (at the time I first saw him he could not speak), difficulty of breathing on change of weather, and a feeling on inspiration, as if there was an obstacle preventing the air from passing. There was also at times a difficulty of deglutition. The general health was not good ; and he had suffered much from a variety of morbid affections. The throat was red and swollen, the epiglottis thickened and rigid. On applying the caustic solution, which was done cautiously at first, and, from the long-standing of the disease, without much hope, considerable relief was obtained.

About the fourth application the sponge was passed into the larynx. Violent spasm ensued, with a strong feeling of suffocation. This, however, soon passed off, and was followed by profuse expectoration, and great relief. After this the patient could lie down and breathe without difficulty. The spasmodic effect was so violent that I really had some misgivings about applying the caustic a second time. The patient, however, a man of great resolution, had none. It was applied again and again, and, though the spasmodic action always returned, it gradually became less, the voice improved, and considerable ground had been gained, when unfortunately, during the intensely cold weather of January, the patient was seized with acute sore throat and bronchitis, and all local treatment was suspended. At times there appeared some amendment. When I saw him he could only speak in a whisper. The larynx, however, was not much affected, except on one occasion, when there was so much difficulty of breathing that I contemplated laryngotomy. Next morning, however, this was gone, and the inspiration and the expiration quite free. Mr Syme saw him along with me, and agreed that the larynx was not the organ affected. The disease was evidently bronchitis, with disease of the lung; and he sunk exhausted from that cause in April.

I could describe numerous cases of a similar kind, but have already occupied enough of the Society's time, and have only to make a few concluding remarks. The nostrils are sometimes affected by the follicular disease extending upwards and behind the uvula and velum. The discharge is acrimonious, and seems to keep up the disease in the throat. Most commonly only one nostril is affected, and generally the same side of the throat. This is remedied by passing a thin flat sponge, dipped in the caustic solution, behind the velum, and upwards into the posterior nares.

The frequency of ulceration in the larynx in cases of phthisis is well known; but, as far as my own experience goes, the ulcerated state follows phthisis, and does not produce it. Still, in the earlier stages, we often meet with the follicular disease, which yields to the usual treatment; and in the more advanced stages, where ulceration is present, we can give great relief, though with no hope of an ultimate cure. In venereal cases I have not had an opportunity of using the caustic plan of treatment. The cases connected with phthisis I shall take an opportunity of laying before the Society at a future period.

ARTICLE IV.—*Successful Case of Extraction of the Child by the Cæsarean Section, after the Death of the Mother.* By GEORGE HARLEY, Esq., House-Surgeon in the Edinburgh Maternity Hospital.

(*Read by Dr Simpson, on 5th June, at the Meeting of the Edinburgh Medico-Chirurgical Society.*)

CATHERINE DAVIDSON, æt. 39, unmarried, was admitted into the Royal Maternity Hospital, Edinburgh, in the beginning of March 1850, being in the seventh month of her second pregnancy. She stated that she had been very delicate during her pregnancy, and complained of shortness of breathing in ascending stairs, and of

symptoms of dyspepsia. Had menstruated last on 12th August 1849, and expected to be delivered about the middle of May.

Being much troubled with swelled legs, and her face having a puffy usually pale appearance, her urine was tested ten days before her death, and showed no appearance of albumen on the application of heat or nitric acid, separately and combined.

Thursday, 25th April 1850.—About 25 minutes to 11 A.M., I was summoned to the kitchen by the matron, as one of the patients was in danger of suffocation. My first inquiry was, "Had she lately taken any food?" The answer was in the negative. On reaching the kitchen, I found Catherine Davidson sitting on a chair, in the washing-house, close to the kitchen door, supported by two of the other patients, with eyes prominent, lips becoming blue, respiration very hurried, with great effort and heaving of the chest; her countenance altogether denoting the most intense anxiety. All that she could say was, "I am gone! O, doctor, save me!" Hearing a distinct half crowing, half gurgling sound in her throat, I immediately ordered her dress to be loosened, and her stays torn off. She was now spitting up a brownish, glairy, semi-fluid matter.

We supported her, and got her up stairs to Ward No. 4, having to stand still on the way several times, to allow her to recover her breath. As soon as she sat down in bed, I ran and prepared an emetic of 3ss. sulphate of zinc, which she willingly swallowed. Her symptoms gradually, though quickly, becoming more urgent, I proposed performing laryngotomy; she laid herself upon her back, and the operation was quickly completed. On bringing her head forwards over the bed, white frothy mucus ran out of her mouth, and some from the wound. There not being a tracheotomy tube in the house, I cut about three and a-half inches off a full-sized flexible male catheter, and inserted it into the wound; but finding the mucus did not come readily through it, it was immediately withdrawn. I then repeated the emetic, as the former dose seemed greatly to have assisted expectoration, and in less than five minutes above eight ounces of white frothy mucus were collected in a vessel. The patient now became quite livid in the face, her eyes seemed starting from their sockets; and, after tossing about her arms, and giving one or two gasping inspirations, she fell backwards insensible. The symptoms still simulating those of spasmodic closure of the glottis, or some other obstruction at the larynx, and thinking that the artificial opening was not large enough to admit sufficient air to carry on the respiration, or allow her to expectorate freely, we pulled the bed to the nearest window, and I immediately proceeded to perform tracheotomy; but, before the operation was much more than half-finished, all attempts at respiration ceased.

The matron, a most intelligent person, having had her hand on the pulse, watching its decline, told me that it had ceased to be perceptible for some minutes. On touching the wrist, I found she was perfectly correct; and, on putting my ear to the chest, I heard no

pulsation at the heart. A single glance at the patient's face showed evidently that she was dead; the frothy mucus was running out from the mouth and nostrils, the eyes were fixed, and the pupils dilated.

All hope of saving the woman being lost, my next thought was to save the life of the child; so I again snatched up the bistoury, ripped down the patient's dress, and instantly made an incision in the mesial line, through the parietes of the abdomen, commencing a little above the symphysis pubes, keeping close to the right side of the umbilicus, and terminating a little above it. The uterus then appeared, and I proceeded to make careful incisions in it, to avoid wounding the child. In making these incisions none of the intestines came in the way of the knife, and there was very little or no bleeding from the wounds.

When the cavity of the uterus was reached, the liquor amnii escaped. I put the two first fingers of my left hand into it, laid the back of the knife against them, and cut downwards and outwards. One side of the nates now appeared. I then put the right hand into the uterus, caught hold of the first thing that came in the way, which happened to be a leg, and withdrew the child without any difficulty, the uterus not contracting round the neck.

The child, on extraction, looked beautiful and clean, as if it had been carefully washed; it was to all appearance quite dead, no pulsation being felt either at heart or cord. I dashed cold water on its chest, gave it a rub, and then put my mouth to its mouth, depressed and pushed back the larynx, held the nostrils with the one hand, and pressed on the chest with the other, after each time that I filled the child's lungs with air.

After a few minutes I stopped to take breath, and during that time I applied friction and aqua ammoniæ to the breast; and on using artificial respiration for some minutes more, the child's heart began to beat, and the pulsations in the cord became distinctly visible; a ligature having been put upon it, the child was cut away. It was a male, weighing 6 lbs. 12 ozs., and measuring 18½ inches. Shortly afterwards I put my hand into the uterus, and peeled the placenta from the back part and right side of the organ, as it would not come away by pulling at the cord; it weighed 1 lb. 4 ozs., and the cord measured 20 inches. The wound was stitched up, and the body removed into the delivery room, where the post-mortem examination took place.

Not more than twenty minutes elapsed from the time the patient was seized in the kitchen till all was over.

Autopsy, 24 hours after death.

Present,—Dr Thompson, Mr Rolston, surgeon, and Mr Harley, house-surgeon.

External Surface.—Countenance livid, lips bluish-purple, eyes prominent, pupils dilated; an incision 8½ inches long, extending from a

little above the symphysis pubis, keeping to the right side of the umbilicus, and terminating about an inch above it.

Pulmonary Organs.—Lungs pale and white at the margins, which were emphysematous, touching each other opposite the 2d and 3d ribs; six ounces of serum in the left pleura, as much, if not more, in the right, which was not measured, as some blood from the incisions made in opening the chest had mingled with it.

On incisions being made into the lungs, they were seen to be quite full of mucus, which exuded on pressure; the crepitation was very slight, in consequence of the œdema; a few hard substances like tubercles were found throughout their substance.

Cardiac Organs.—On opening the pericardium, 4 ounces of serum were found in it; it was otherwise healthy. Heart rather pale, weighing 14 ounces. Ventricles contracted, without any blood in their cavities; left auricle dilated; mitral valves cartilaginous, and scarcely admitting the point of the fore finger. The corpora aurantii on the aortic valves were very much hypertrophied, preventing complete closure of the aortic orifice, and consequently permitting regurgitation. Right side of heart natural.

Larynx and *Trachea* both quite healthy; no vestige of any lesion, except the laryngotomy wound; epiglottis and vocal chords healthy.

Abdomen.—*Kidneys* full size and quite healthy. *Spleen* healthy, and of usual size. *Liver* not examined. *Intestinal canal* quite normal. *Uterus* not contracted, the wound in its anterior surface quite distinct. Some urine being drawn off by the catheter, was found to be slightly coagulable by nitric acid, but not by heat, till nitric acid was also added. It was not changed by caustic potash and heat.

Examination of Head, 29 hours after death.

Present,—Dr Keiller and Mr Harley.

Skull thick, with dura mater adherent to it; no extravasation external to dura mater. Superior longitudinal sinus empty. On raising the dura mater, a quantity of serous effusion was found beneath the arachnoid membrane, and a large quantity flowed also from the spinal canal. *Brain* quite healthy, rather anemic than otherwise; arteries empty, and choroid plexuses very anemic; ventricles containing rather more than the usual amount of serum. Nothing could be detected in the brain to account for death.

From information subsequently received from the relations, it was found that, when 14 or 15 years of age, she had been confined to bed for a fortnight or three weeks with an attack of acute rheumatism. Four years ago she had had a severe cold, and had always complained more or less of colds since, together with shortness of breath on going up stairs, and occasional slight palpitation; during the past winter her cough had been more troublesome than usual.

ARTICLE V.—*Case of Laryngo-Tracheitis terminating Favourably after Tracheotomy.* By JAMES D. GILLESPIE, M.D., F.R.C.S.E.

(*Read before the Medico-Chirurgical Society, 19th June* 1850.)

MRS DONALD, æt. 30, residing in Greenside Lane, wife of a painter in Mr Croall's factory, about seven months pregnant, on the evening of the 15th of January 1850, after exposure to cold and wet, was seized with the symptoms of an ordinary catarrh, to which she paid little or no attention.

On the 20th, pain in the larynx, with dyspnœa, was superadded. The difficulty in breathing gradually became more intense, but as the patient had been occasionally subject to asthmatic paroxysms, the increasing danger of the symptoms was disregarded. On the morning of the 29th, however, her husband became alarmed, and applied to Mr Hugh Balfour for medical advice.

He found the patient propped up in bed, unable to occupy the recumbent posture, and suffering from repeated paroxysms of severe dyspnœa. Mr Balfour ordered leeches to be applied to the larynx, and an emetic to be immediately administered. The leeches were not obtained, but the emetic was taken, and caused violent retching, without, however, any abatement of the symptoms.

The same evening I was requested by Mr Balfour to see the patient, as he thought it probable tracheotomy would be necessary. I accordingly saw her about midnight, when the following was her condition:—The pulse was about 100 in frequency, weak, but jerking, and every two or three minutes a paroxysm of intense dyspnœa, threatening asphyxia, occurred. On examining the fauces, the uvula and soft palate were considerably injected and swollen, and by pressing down the tongue, I was enabled for a moment to catch a glimpse of the epiglottis, projecting straight up, much thickened and enlarged. This examination caused a violent fit of coughing and dyspnœa.

As the dyspnœa was occurring in well-marked paroxysms, differing in intensity, I deemed it advisable to try the effect of antispasmodic remedies, before resorting to the extreme measure of making an artificial opening in the trachea; so a combination of sulphuric ether, with the muriate of morphia, was prescribed to be taken every half hour, till relief was experienced. Directions were given to summon me should the patient become worse.

Before daylight I was roused by an urgent message—"That Mrs Donald was dying." On hastening to her house, I found her sitting up, tossing about in bed, gasping for breath, the pulse almost imperceptible, the countenance much flushed, the eyes protruding, and a cold profuse perspiration breaking on the forehead. Soon afterwards Mr Balfour arrived, and we at once decided that tracheotomy was the patient's only chance of safety from impending asphyxia. Owing to the convulsive movements of the muscles attached to the larynx, and the strong contraction of the sternal portion of the mastoid muscle, which rendered the trachea deeper, careful dissection proved almost impossible. The difficulty was also augmented by the shortness and narrowness of the patient's neck, and the less than usual rigidity of the tracheal rings. During the operation, the flushing of the countenance became changed, first into purple, then a bluish lividity; the pulse entirely imperceptible; the respirations very much prolonged, not above four or five in the minute; so it was imperative to open the trachea without delay. This was accordingly done, and an opening made sufficient for the introduction of a middle-sized tube. Its insertion caused violent fits of forced expiration; and by partly closing the orifice, a considerable quantity of venous blood, which had entered the trachea, was expelled. Still, however, the breathing was not so free as might have been expected, and the patient having communicated by signs the desire to say something, wrote upon a slate, that she felt there were some "pieces of skin" to come away. The friends were ordered to watch the patient vigilantly, and clear out the tube repeatedly with a feather. About noon a most violent paroxysm of coughing occurred; and her husband at last succeeded in extracting from the inner ori-

fice of the tube a large piece of firm membranous lymph, having a semi-tubular form. Soon afterwards she was seen by me, the breathing being comparatively easy; but the patient repeated her assertion, there was much more *skin* to come away. Pills, containing two grains of calomel with a little hyoscyamus, were ordered to be taken every four hours.

January 31st.—A large-sized tube has been substituted. Several smaller pieces of membrane have been coughed up during the night. The patient now first experiences pain in the chest; and examination shows, over the whole front, the existence of innumerable bronchitic râles. There is much difficulty of expectoration, resulting in the discharge of very thin viscid sputa, which speedily concrete on the sides of the tube. The pulse is 100, excited, but having little strength in it. Sixteen leeches to be applied to the upper part of the chest, the calomel pills continued, and an expectorant mixture, containing ipecacuan wine, and sweet spirits of nitre, occasionally given.

Evening.—The leeches have been applied, and the bleeding orifices encouraged to bleed freely. Pulse 120, weak. Breathing considerably relieved, but large gushes of a thin gummy fluid come from the tube. A blister to be applied over the upper part of the chest.

February 1st—*Morning.*—Easier to-day; the blister having shifted during the night, has greatly extended the amount of vesicated surface; pulse 90, soft; bronchitic râles not so loud or numerous; breathing less hurried. During the night there passed through the tube a dense piece of membranous lymph. Another portion has been flapping against the end of the tube, but is not detached.

Afternoon.—Dr Graham Weir saw her with me, and during our visit, I succeeded in detaching, with a probe bent into the form of a hook, a piece of greyish buff coloured membrane, about two inches long and an inch broad, not possessing a perfect tubular character.

February 2d—*Morning.*—Breathing continues easy. No more membrane has come away, and there are now no symptoms of any more being present. Râles in the chest more dry, and as if the ramifications of the bronchial tubes were clogged with more consistent mucus.

Afternoon.—Dr Weir again saw her with me. During our visit she had several well-marked labour pains. On examination by Dr Weir, the os uteri was found slightly dilated, admitting the point of the fore finger. She got some castor oil in the forenoon, which has acted once freely. To have an injection, consisting of a drachm of laudanum in solution of starch.

Evening.—There has been no return of labour pains, but the uterus is strongly contracted. The skin is hot and dry, and the countenance flushed. To take some Mindererus' spirit, with a little sweet spirits of nitre, every three hours.

February 3d—*Morning.*—During the night labour pains recurred every five minutes, but were checked by another starch and opium injection, and they have not returned. Pulse 120, soft and small. On examination of the chest, there was marked improvement in the bronchitic sounds heard on the right side; but on the left side, between the lower half of the scapula and the spine, there is slight dulness, with small crepitation, occasionally but obscurely heard during inspiration. Owing to the profuse discharge still coming from the blistered surface, no examination can be made of the anterior part of the chest. A large mustard blister to be applied to the back on the left side. The gums are now affected by the calomel, a red margin and mercurial fetor being distinct,—so the pills are to be discontinued.

February 4th.—The crepitation heard yesterday is more extended, but does not appear quite so minute. She is now under the full influence of mercury. Pulse 110; countenance flushed; breathing easy, except when collections of mucus require to be expectorated. The sputa are very tenacious, some of a rusty colour, others streaked with blood. To take a pill, containing half a grain of tartar-emetic and a third of a grain of opium every three hours.

Evening.—Labour pains recurred since visit, but were checked by the injection as formerly. The two first pills caused retching, so they are to be halved, and taken every two hours.

February 5th.—The patient feels distinctly the movements of the child, but the pains have not again appeared. Pulse 120. All œdema of the fauces and uvula having disappeared, and the epiglottis being no longer visible, there is reason to believe that the larynx is now free ; and as it is likely to keep up the disease in the chest, the tube has been removed, and the patient breathes quite easily without it. The blistered surface on the chest is still suppurating freely.

Examination of the back discloses, on the right side, puerile respiration, with a few cooing and sonorous râles. On the left side we still have an extension of the pneumonic symptoms, the upper part beneath the scapula being dull, and having little vesicular respiration, while all below is slightly dull, with crepitation slightly intermixed with cooing or sonorous râles. Sputa not so abundant, but having the same character. She has been regularly taking the halved pills, without their producing sickness,—so a fourth of a grain more of antimony is to be added to each.

February 7th.—Since last report, the patient has been progressing favourably. The puerile respiration on the right side is now nearly absent ; the crepitation on the left side not so abundant, and coming from larger air-vesicles. Pulse 100. Sputa now of a thick greenish mucus, neither stained nor streaked with blood.

To have a mixture containing ipecacuan wine, sweet spirits of nitre, and tincture of hyoscyamus.

February 8th.—Has passed a restless night, owing to the difficulty of expectorating the tough mucus. Complains of pain in the left shoulder, passing through to the back. Cannot lie on the right side. Pulse 98. Countenance slightly flushed ; skin moist. Wound in the neck contracting. Most of the sputa coming from the natural passage.

Examination of the back discloses on the left side not nearly so much bronchial râle, and in the upper part the vesicular murmur apparently returning. On the right side respiration still slightly puerile, mixed up with dry and moist râles.

February 10th.—Considerable improvement. Wound of neck closing fast. This morning she coughed up from the throat a piece of membrane, about an inch long.

February 12th.—Wound of neck healing rapidly ; no air passes through it, except during a fit of coughing. Breathing quite easy. Voice almost natural.

February 15th.—Wound of trachea quite healed. Bronchial râles on both sides of the chest ; dry on the right side. To take a pill, containing a grain of the sulphate of bibeerine, half a grain of ipecacuan, and extract of hyoscyamus, three times a day. To have a more generous diet.

February 16th.—Complains of pain in the left side, and swelling, with tenderness, round the thyroid cartilage. Voice slightly hoarse. To have a mustard blister applied to the side.

February 17th.—The hoarseness continues ; but the pain is removed.

February 19th.—As some tenderness around the thyroid cartilage still exists, six leeches are to be applied, and a smart purge taken.

February 21st.—Pain in the neck removed, and hoarseness almost entirely gone.

February 23rd.—Discharged cured.

I need only further add that, on the 1st of April, Mrs Donald was brought to bed, at the full time, of a remarkably fine boy ; and at the present date (April 25th) she is in the enjoyment of perfect health.

I have deemed this case worthy of record, as it appears to me to possess some features peculiar to itself, which I can scarcely find recognised in any systematic work on diseases of the trachea and larynx, nor have I been able to find cases precisely similar detailed in any of the medical journals. All the cases recorded by Louis as

examples of croup in the adult, may be distinguished from what we are now considering by the primary disease commencing in the fauces and pharynx, and gradually extending downwards till it affected the larynx and trachea,—in fact, closely resembling, if not identical with, the epidemic disease known under the term of diphtherite, which has been so well described by Bretonneau. Moreover, in some of Louis's cases, the laryngitis was a secondary affection, occurring during the progress, or after the termination, of a debilitating primary disease.

Dr Stokes has clearly pointed out the difference between the so-called primary and secondary croup; and though he does not deny the possibility of the primary affection occurring in the adult, in his work on "Diseases of the Chest," he has been unable to furnish any example of the disease.

Dr Cheyne, who had most advantageous opportunities for observation, makes the following remark:—"In no part of Britain, I imagine, is croup more prevalent than in Leith and its immediate vicinity; yet, in the course of nearly fifty years of extensive practice, my father has not seen one instance of croup occurring after puberty, while he has attended many cases between the tenth and fourteenth year, both in delicate and robust children."

There is only one case which I can find in the journals closely resembling that which I have detailed above. It is narrated in the volume of the "Lancet" for 1838, and is entitled, "A Case of Primary Laryngo-Tracheitis."

The patient was a female, in the eighth month of pregnancy; the symptoms of impending suffocation were imminent; the child died in utero; but tracheotomy was not performed, owing to the woman having been reported to have coughed up a piece of false membrane. The autopsy showed "the epiglottis erect, hard, and wonderfully thickened, representing the fourth of a sphere, instead of a plane." "The trachea was lined with a buff-coloured membrane, nearly as thick as the rings of the trachea, extending to the bifurcation; it formed a perfect cylinder, except at one point posteriorly, and at the lower part of the trachea, where there was a longitudinal deficiency. The cylinder of false membrane was free inferiorly; it was attached above." "The folds of the membrane forming the edges of the glottis were hard and swollen; the ventricles of the larynx were hardly visible, being nearly obliterated by the inflammatory thickening and exudation."

It is a subject for consideration in this instance, whether tracheotomy should not have been resorted to, as a chance of saving the patient's life. When we reflect on the obstructed condition of the glottis in acute laryngeal affections, we must at once see the extreme difficulty, if not utter impossibility, of any considerable amount of membranous exudation being expectorated through the greatly narrowed or closed orifice; and it is surely better to give the patient a chance of relief, by performing tracheotomy, than to leave him to

certain and immediate suffocation. The difficulty of ascertaining whether any membrane has been formed cannot be urged as an excuse for the non-performance of tracheotomy; for if, as is now almost universally admitted, we are authorised to open the trachea, in the uncomplicated acute laryngitis, whenever the danger of suffocation becomes imminent, that operation is surely more strongly indicated, when we have reason to suspect that the obstruction of a false membrane in the larynx or trachea is superadded to the constricted state of the glottis.

These remarks will not apply to the treatment of croup in children, for many circumstances tend to render the propriety of operation in their cases much more doubtful. The lungs are so frequently seriously affected in the infantile disease, and the bronchi plugged up with adventitious membrane, that careful examination of all the features in each particular case must be made, ere we can resort to tracheotomy, even as a forlorn hope; for we should always keep in view the fact, of the truth of which every practical man must be well aware, that, when a case terminates unfavourably, after surgical interference, the fatal event is certain to be ascribed to the operation; and thus discredit may be brought on what, when employed with judgment, might prove a valuable acquisition in surgery.

That Mrs Donald's case, though it has some features in common, cannot be strictly ranked as an illustration of primary croup, is, I imagine, sufficiently evident. I am inclined to consider it as an extension of the common acute inflammation of the larynx to the trachea, where, owing to some idiosyncrasy on the part of the patient, it assumed the form of lymphatic exudation.

On inquiry, I find that Mrs Donald, when an infant, had a most severe attack of croup, from which she recovered, under the treatment of the late Dr Abercrombie.

It is worthy of remark, that the intense inflammation of the bronchial tubes and left lung, which became developed after tracheotomy was performed, did not prove fatal. This appears to me to corroborate the opinion, that inflammation, induced by direct injury, though it almost invariably proves of a most acute character, is not actually so much to be dreaded as when it occurs idiopathically.

I have never seen, and never wish to see, in my own practice, bronchitis of a more intense nature affecting so universally both sides of the chest.

That the infant in utero retained its vitality is certainly most surprising, when we take into account the imperfect oxygenation of the maternal blood, which existed for a considerable period; but it, at the same time, evinces the power which nature bestows on the offspring of accommodating itself to the influences affecting the vital economy of the parent.

With regard to the diagnosis of the disease detailed above, little can be said. Mrs Donald was not seen by me soon enough to admit of much time for deliberation respecting the precise extent of her disease.

So far as I can ascertain, I find that she had no cough till the day before I saw her; and, when it did appear, it had not the ringing metallic character heard in the croup of children. The stridulous breathing was certainly present, but that may be referred to the œdematous state of the glottis. I believe, therefore, that unless some portion of membrane has been expectorated, or some appearance of exudation has been found on the fauces, no suspicion of false membrane in the larynx and trachea can be excited. It is fortunate that the indications for treatment in acute laryngeal and tracheal affections of the adult are alike, and that we are called upon to provide an artificial means of respiration, whenever the natural passage becomes inefficient for the purpose.

It must be clearly understood, that I speak only of acute primary idiopathic inflammation of the larynx and trachea; for, in the diphtheritic kind of inflammation, the propriety of operation is much more doubtful. In it there is an unhealthy state of the system to be combated, which is probably more dangerous than the local affection; and it is the confounding these two distinct diseases—laryngitis and diphtheritis—which has caused so many apparently contradictory statements to be made by authors.

Part Second.

REVIEWS.

The Theory and Practice of Midwifery. By FLEETWOOD CHURCHILL, M.D. Second Edition. Pp. 496. London, 1850.

No living author has contributed more industriously, or more successfully, than Dr Churchill, to the modern obstetric literature of England. His various excellent works on the "Diseases of Females," on the "Diseases of Pregnancy," on the "Diseases of Children," and on "Operative Midwifery," afford ample evidence of the truth of this remark. The volume, however, before us is, we believe, the best of the series which Dr Churchill has written; and, in our opinion, it forms one of the best if not the very best text-book and epitome of obstetric science which we at present possess in the English language.

The first edition of Dr Churchill's work on midwifery appeared in 1842. It has, we have heard, already gone through more than one edition in America. The present is the second English edition, and contains numerous corrections and improvements. At the same time, we cannot help expressing a wish that it had been brought up to the present date in some important points in which the author has left it in the state in which it originally

appeared in the first edition. For example, much new information has been of late years accumulating regarding the morbid anatomy of the pelvis, the structure of the mucous membrane of the uterus, the varying forms of the uterus and its cavity, the nature of the decidua, the extent and relations of the allantois, &c.; but Dr Churchill does not carry down the knowledge of his readers in these matters beyond 1842. In consequence, also, of his adhering to his original description of some matters, these descriptions, though now repeated, are really at the present day obsolete. For instance, the 178th paragraph, giving an account of Dr Lee's ideas regarding the structure of the placenta, stands in this predicament. In 1832, Dr Lee published some views regarding the placenta entirely opposed to the views of the Hunters, and maintaining the absence of maternal vessels in the placenta. In 1833, Dr Lee made some dissections, with Mr Lawrence, that convinced him that the Hunters were, after all, right; in 1842, Dr Lee, at last, publicly withdrew the anatomical errors into which he had himself inadvertently fallen, and into which he had drawn others; and yet, in 1850, Dr Churchill persists in describing Dr Lee as still holding those erroneous views on this matter which he renounced several years ago. These, however, are very small blemishes in a work which contains so much that is really good.

In the more practical part of his work, Dr Churchill exhibits greater care in laying before his readers information of the latest date. The longest new dissertations which he has added in this edition are,—on the treatment of unavoidable hemorrhage, by detachment of the placenta; on turning, as a substitute for craniotomy; and on the use of anæsthetics in midwifery. We propose to look briefly at each of these new additions.

If there is any truth in the doctrine of the maternal circulation of the blood in the placenta, as laid down by the Hunters (and all modern physiologists admit the soundness and truth of that doctrine), blood *must* escape from the free uterine surface of the placenta when the placenta becomes partially detached from the uterine surface, as in cases of unavoidable hemorrhage; and, consequently, the flooding becomes arrested when the placenta is *entirely detached*,—the plan of treatment which has of late years been proposed to be followed artificially, in instances in which the older modes of treatment were either ineffectual, or specially difficult and dangerous of application, as in dangerous unavoidable hemorrhage, with the os uteri still too rigid to allow of turning; when the mother is too exhausted to permit of turning; when the child is dead or non-viable, &c. Dr Churchill considers that the hemorrhage in placental presentations does not come from the placenta, but that it escapes from the uterine sinuses, laid bare by the detachment of the placenta. In order to produce the hemorrhage in this way, nature must, at the time of its production, turn back

and invert the course of the circulation in these sinuses. But this point we have not time to dwell upon.

Dr Churchill reasons, and, as we conceive, again inaccurately, on the probable number of cases of placental presentations in which some other operation would be required *besides* the detachment of the placenta, provided this plan were adopted in the treatment. In numerous old cases detailed by authors, and in which the placenta was spontaneously expelled, and the hemorrhage ceased, the child notwithstanding was afterwards removed by operative interference, as by turning and craniotomy. A great number of these secondary operations were had recourse to merely because the attendant was terrified to risk the non-delivery of his patient, after the placenta was wholly separated. But Dr Churchill argues, that if we separate the placenta artificially, we will require to adopt these methods of forced delivery, just as frequently as was done formerly under fear of hemorrhage. Certainly not. In placenta prævia, mal-presentations of the child occur more frequently than in natural labours; but transverse mal-presentations, requiring delivery by turning, do not occur oftener than once in twenty cases. And we could point out to Dr Churchill published cases of placental presentation, where turning, when it was required from mal-presentations, could not be effected till the placenta was removed. Dr Churchill considers the detachment of the placenta improper in cases where the hemorrhage is very great, but the os uteri still not much dilated. These are certainly, however, the cases best adapted for the practice; and turning in them is frightfully fatal, from the tearing of the cervix uteri (the most vascular part of the whole uterine parietes in placenta-prævia), and the consequent liability to subsequent fatal hemorrhage or phlebitis. Dr Churchill objects, that where the os uteri is still imperfectly developed, he does not see how the placenta can be easily or safely detached. And yet, in his chapter on the "Induction of Premature Labour," he does not see any difficulty in the practice of "separating the membranes for two or three inches around the os uteri," although, at the time of doing so, the os uteri is not in the very least degree opened. If the membranes can be thus separated before the os uteri is at all dilated, the placenta may certainly be equally, or indeed more easily, separated when the os uteri is already partly opened. And in relation to the practice, we are never to forget the true observation of William Hunter, that the placenta is as readily separated from the uterus as the orange is from its rind. After all, the value of the new treatment of unavoidable hemorrhage by complete separation of the placenta, will never be decided by mere theoretical reasoning and objections. Its value, like the value of other new practices, can be decided by the results of practice only. The time has probably not yet arrived when such a decision can be pronounced, because the practice is as yet too recent, and the cases in which it has been followed are yet too few, to permit us

to judge definitely of its utility. But the cases and their results, as far as we know them, are in favour of the new practice. We have seen three cases in which the placenta was artificially separated, in accordance with the principles of the new practice; all the three mothers made good recoveries. Dr Churchill alludes to seventeen cases collected by Dr West; sixteen out of the seventeen mothers were saved. Dr Waller, lecturer on midwifery at St Thomas' Hospital, London, and a gentleman who has had extensive experience in this and other morbid complications, two years ago published a long series of interesting cases of placenta-prævia. In a late addition to his former communications in the "Medical Times," he observes, "My confidence in Professor Simpson's plan in these cases, remains unshaken. It will be seen, in reference to my cases, that in nine instances the placenta was separated, either naturally or artificially, before the birth of the child. The same effect was produced in all, namely, *the cessation of the hemorrhage;* and eight of these women recovered. Surely this result could not have been accidental, as some would have us imagine. I am not (Dr Waller adds) aware that any cases have been recorded wherein hemorrhage has *continued* after complete separation." In these 29 cases of the new practice we have two maternal deaths,—or 1 in 14 died. Out of 182 cases of the same complication, treated by the old plan, and given by Dr Churchill at p. 388, there occurred 51 maternal deaths,—or nearly 1 in $3\frac{1}{2}$ died.

Among our original articles in the present number will be found an interesting case of obstructed labour, in which turning was successfully adopted, and the mother and child saved, instead of the labour being terminated by craniotomy. Dr Churchill discusses the propriety of this innovation, and altogether objects to the saving of infantile life by this means. He first gives a wrong history of the origin of the practice. He observes, "Dr Simpson has renewed the proposal of Velpeau to substitute turning in certain cases of distortion of the pelvis for craniotomy." Now the proposal of the practice was made, and the principle of it suggested and published, by an old master of the Dublin Hospital, who died half a century before Velpeau wrote. Nay, Dr Churchill forgets that, only a couple of pages before, he had himself, in his own work, stated that, "Denman and some other writers recommend turning when the pelvis is slightly too narrow for the child's head." And Dr Churchill knows medical biography too well not to be aware that Denman lived before Velpeau. Besides, Velpeau, in truth, has nowhere made such a proposal.

The observations which Dr Churchill subsequently makes on turning instead of craniotomy, fully prove that he entirely misunderstands the principles of the practice. He fears we might turn the child and yet find the pelvis too small to let it pass without

craniotomy. Among Dr Collins' 16,000 cases in the Dublin Hospital, there was no pelvis too small, for there was none below $2\frac{1}{2}$ inches in its conjugate diameter, and only one so small even as this; and we have seen a full-sized child pass through as small a pelvis by turning, its head becoming compressed during its passage. Dr Churchill objects that, for the success of turning in contracted pelvis, we must be able to ascertain the "exact diameters" of the pelvis. He might as reasonably argue that, for the success of craniotomy in contracted pelvis, we must be able to ascertain the "exact diameters" of the pelvis. The general case is this,—the head becomes arrested at or above the brim, in consequence of the head being slightly too large, or the brim too small; and the question is, how is delivery to be effected? Take a steel perforator (says Dr Churchill), introduce it through the cranium of the child, allow some of the brain to escape, so as to diminish the head, and thus allow of its passage. We say, no; if the child is alive, and there is no contra-indication, extract the infant by turning, and in its extraction subject the flexible skull of the child to transient, and not necessarily fatal, compression, rather than to fatal perforation. He fears that, even if the child were turned, we might sometimes still require to reduce its head by perforation after all. But if so the operation is more simple than if the arch of the cranium presented, in so far that no crotchet is then required. He objects further that the operation would be of "very little benefit to the child." Certainly we admit that *all* the benefit it receives is giving it life, or a good chance of life, instead of being submitted to certain death, by having its head perforated. Numerous children's lives have been already saved by the practice. And we feel fully assured of one fact, that in proper cases the operation of turning gives the mother a far better chance of escape; turning, according to Dr Churchill himself, in other parts of his work, being an operation fatal in about one in every sixteen cases to the mother, whilst, by his own showing, craniotomy is fatal in about one in every five cases to the mother, and, of course, in every case fatal to the child.

Dr Churchill discusses, at very considerable length, the use of anæsthetics in labour. He has committed one error at all events, in his treatment of the subject. He places it as a portion of the chapter on the Induction of Premature Labour. We unhesitatingly submit that this is a true malposition; and we are sure that on reflection Dr Churchill will come to the same conclusion. After describing the history, effects, &c., of chloroform, he observes,—

"It has been now used extensively in Great Britain, in America, and on the Continent, and we have an account of at least 3000 cases in which it has been employed. From this it appears,

"1. That in midwifery practice, no death has occurred which can be fairly and directly attributed to the chloroform. In the cases brought forward by Mr Gream, there is no evidence to prove that the deaths did not result from

the circumstances of the labour, and abundant proof of a disposition to attribute every accident to this new agent.

"2. That some unpleasant symptoms have occurred in hysterical and nervous women during the stage of excitement, but no instance of the alarming or even fatal collapse which has taken place in cases unconnected with pregnancy or parturition. These symptoms disappear in a few moments if the chloroform be discontinued, or, as is said, if the dose be increased.

"3. In a small proportion of cases, the uterine contractions are weakened, rendered less frequent, or even suspended, so long as inhalation is continued, but they return if the use of chloroform be discontinued [or diminished].

"4. In the great majority of cases, it does not interfere with the labour pains, except by suspending all voluntary exertions if the insensibility be complete. Where the dose given is milder, although great relief be afforded, the patient will not become insensible, and will be able to exert considerable force.

"5. That chloroform, in full doses, is capable of entirely removing the pain of obstetrical operations, and thereby increasing the facility of their performance. Moreover, that the dose can be so graduated as to afford degrees of relief, so that, in natural labour, a certain amount of labour may be spared without producing insensibility or incurring the risk, whatever that be, of a full dose.

"6. It neither prevents or weakens the subsequent contractions of the uterus, and consequently does not render the patient more liable to flooding.

"7. That certain women seem more obnoxious to its injurious effects than others, and in some these effects are said to continue some time. Giving full force to these [suppositious] cases, they appear to form a small part of a large number whose recovery was not retarded [but greatly expedited], and whose subsequent health was uninjured."—P. 251.

Dr Churchill subsequently states, that he believes that chloroform should be given in most obstetric operations, except when they are complicated by the presence of convulsions or hemorrhage. We have no doubt that in Dr Churchill's next edition these exceptions will disappear,—as both states are now found to be strong indications for its use, not counter-indications against it. He has more scruple about giving it in natural labour, and does not seem to think that the abrogation of the mere pain and suffering of the mother in natural parturition is a matter upon which the physician should insist. He would on principle give or not, as the patient herself prescribed it or not. We are sure of one thing, that Dr Churchill does not allow his patients such a liberty with any other one medicine in the pharmacopœia. But we admire Dr Churchill for stating the whole question, even so boldly as he has done; for we know well the prejudices against all changes and innovations which surround midwifery in the school of Dublin. An esteemed townsman of Dr Churchill, and an amiable and distinguished physician, when discoursing lately on the subject, in the "Dublin Journal," in an article strangely odd and outrageous, argued that it was a glaring abuse of chloroform to administer it to a woman in natural labour, with (to quote his own words) "no other object than that of rendering her insensible to a part of the ordinary degree of pain which the Almighty, in His wisdom, has connected with the process of parturition." We know that this argument was considered at the time, in Dublin and elsewhere, as very strong and conclusive. But

in our humble opinion, it is simply ridiculous, and almost irreverent. Dr Montgomery might, with equal truth, argue that it was a glaring abuse in himself, or any other physician, employing a horse or a carriage, with "no other object than that of rendering him insensible to a part of the ordinary degree of fatigue which the Almighty, in His wisdom, has connected with the process of walking."

We have just now adverted to the slowness with which new doctrines and practices are usually admitted in the obstetric school of Dublin. Perhaps this species of conservatism in individual medical minds and communities is useful in the long-run for the full and ultimate elucidation of truth in medical science and practice. Indeed, we do not doubt that it may, in the medical world at large, serve the best of purposes, and that it should not be viewed with the impatience sometimes manifested against its more immediate obstructive qualities. At the same time, when, at last, a doctrine is acknowledged under such circumstances, and after long years of doubt and delay, we may assume that it is incontrovertibly true. One or two changes of doctrine in this new edition of Dr Churchill's work have suggested these cursory remarks, and we will give, as an illustration, the first alteration from the old edition, which we happen to notice.

When the head presents, it is now well known that it almost always lies with its long axis parallel to the right sacro-iliac diameter of the brim; and that out of every one hundred such presentations the face looks to the right sacro-iliac sychondrosis in seventy cases, and the occiput is directed towards the same point in about thirty. In other words, cranial cases, in which the face originally looks forward, or to the left groin, occur in about one in every three or four natural labours. The frequency of this position of the head, at the commencement of labour, was stated long ago by Solayres de Renhac and others; but it was first distinctly enunciated and proved by Professor Naegele, of Heidelberg, about thirty-two years ago. There is now no accoucheur, we believe, of any note, on the continent of Europe, who does not subscribe to the truth of Naegele's observations on this subject; and since 1828, when Dr Rigby first translated Naegele's Essay, it has gradually gained more and more converts in England and Scotland. But Dr Collins and others in Dublin, long found results different from what were observed elsewhere. Dr Collins enters the occipito-posterior position of the head as occurring in only about 1 in every 1300 labours in the Dublin Hospital. In other hospitals they occur as often as 1 in every 3 or 4 labours. In his first edition, Dr Churchill doubted their frequency; in his present edition, he declares the correctness of Naegele's observations. In the article in the "Dublin Journal," from which we have already quoted a paragraph, Dr Montgomery tells us, that the Dublin accoucheurs had been told, "with somewhat less than courtesy," that "facts require ten years to arrive in Dublin." We do not remember to have ever seen or read such a remark in print;

but whoever made it, proceeded, we opine, upon an undue want of knowledge of the length of time that some facts at least require for transmission in the direction stated. Here, for example, we have a simple, but most important fact—the frequent position of the face forwards,—a fact, of which any tyro may obtain complete evidence, if he only uses his sense of touch with a little care, and without preconceptions or prejudices,—and this fact is not acknowledged in the principal Dublin text-book on midwifery, till upwards of thirty years after it is announced in Germany, and upwards of twenty years after it is announced in England. In writing this statement, we cannot but entertain a confident hope, that many other doctrines and practices, opposed and objected to by the Dublin school, are yet destined to share the same fate.

But we must draw these observations, already too extended, to a conclusion, and we do so with a sincere wish for the success of Dr Churchill's excellent work. We have omitted to state, that it is illustrated by numerous well executed wood-cuts. Many of these are copies of the accurate figures of the positions and presentations published by Professor Moreau. A few of the wood-cuts, however, greatly require alteration. For example, fig. 76 represents an appplication of the long forceps, that would, we believe, be found simply impossible, even upon the dry bones, and with a large pelvis to boot, instead of a small one. Fig. 89 represents the application of the crotchet upon the forehead instead of the occiput, or parietal region of the head,—an application totally at variance with all sound knowledge relative to the mechanism of labour.

The Principles of Surgery. By JOHN A. ORR, A.B., F.R.C.S.I., one of the Surgeons of the City of Dublin Hospital. Dublin, 1850.

IT is so long since any contribution to the science or practice of surgery has emanated from the Dublin school, that in the work of which the title is prefixed we expected to find an accumulation of new facts and ideas bearing upon a department of the profession which was formerly supposed to be cultivated with considerable success in the Irish capital. The extent of our disappointment may, therefore, be imagined, when we found this volume to contain nothing more than such a concoction of pre-existing materials as merely to constitute another "manual," or compendious body of surgical information, in addition to the numerous productions of a similar kind which of late years have issued so copiously from the press upon this side of the channel.

We cannot help regarding the appetite for these publications as an unfavourable symptom of the spirit which actuates the readers, and as a rather alarming prognostic for the good of their patients, as

well as their own professional success, excepting in so far as the making of money may be concerned. For we hold it as nearly, if not altogether, impossible to acquire from second-hand sources of knowledge the fresh and vigorous character of mind which is essential to effecting any advance beyond the limits of routine practice; and we are accustomed to judge, with no small confidence, from the contents of a student's shelves, what chance he has of rising beyond the common level. It would have been well for many of Mr Liston's admirers, who endeavoured to imitate him by acquiring manual dexterity alone, if they had known, as we do, how diligently in his early days he perused the original writings of surgical authors, and how eagerly, up to the end of his life, he availed himself of the information obtained from such sources. But as for the "*crambe recocta*," of which the "manuals" are composed, we believe that nothing could overcome the disgust excited by such unsavoury food, except the terrors of the "Hall," or the wants experienced in practice from defective education.

We object, further, to these publications, as being injurious also to the physical well-being of their readers, by the straining of eyes which is requisite for deciphering the microscopic characters employed in accordance with the general principle of compression. It is hardly necessary to say that, having discovered the nature of Mr Orr's work, we did not dip deep into its pages; and we, consequently, shall not venture to express any opinion as to their merits, in so far as the information which they contain is in question. We may, however, quote a sentence, to give some idea of the spectacles which our mature readers will require, if they should devote any part of their winter evenings to the study of this book:—

"The whole quantity of the ointment should be divided into four portions, each of which should be rubbed in perfectly and successively; and the patient should wear the same drawers night and day, as thus a portion of the ointment will be constantly applied to the skin. After each thigh has been rubbed three or four times, the remains of the ointment should be washed off with soap and water the night before the next friction. If the patient is unable to perform the inunction himself, the hand of the person who does it should be protected with a pig's bladder, softened in warm water, and well rubbed with oil. If the thigh is the seat of any eruption, the ointment may be applied to the axilla. The effects of the mercury on the constitution and sore will be the same as if that medicine were given by the mouth."

From this passage (on which we have accidentally stumbled), together with some unpleasant recollections of infirm noses, which have appealed for our protection, we are led to fear that the mercurial delusion is still permitted to exert its baneful influence in Ireland. We lately felt constrained to protest against the "mercurial fume" of Birmingham, and do not feel more favourably disposed towards the pig's bladder and medicated drawers of Dublin. We hope and trust that the day is not distant when the light of truth will be so diffused as to dispel all such mischievous follies.

Eighth Annual Report of the Secretaries and Treasurer of the Birmingham and Midland Counties Pathological Society. Birmingham. 8vo. Pp. 44.

This Report embraces a brief account of the morbid specimens exhibited at the monthly meetings of the Society during the year 1849, and of the leading symptoms observed in the cases of the different patients from whom they were obtained. More detailed reports are published in the "Provincial Medical and Surgical Journal;" and to these, as well as to the Report Book of the Society, the pamphlet before us may serve as an index. As specimens of the succinct mode in which observations are here recorded, we extract the two following cases:—

"22. R[1] . . . . . Mr Alf. Baker. P. 337.

"Varicose aneurism of the right arteria profunda femoris, following upon deligation of the femoral, for aneurism in Hunter's canal. The profunda, for its first two inches, dilated into a sac, larger than a walnut, quite closed below from the rest of the artery, by adhesions; the femoral artery and vein communicated by an opening an inch in length, by which, also, the dilated profunda took its origin from the femoral, the opening being thus common to the three vessels; at its rugose margin, the lining membrane of the three vessels continuous. The femoral artery quite obliterated (by the deligation), for half an inch, commencing a quarter of an inch below the large opening. The original aneurism, in Hunter's canal, the size of a walnut, filled with fibrinous layers, the artery pervious up to it, above and below; the vessels otherwise healthy; the profunda vein opened just below the large opening. Superficial veins of the limb dilated. Aorta, nearly to the diaphragm, loaded with atheroma, with some calcareous plates, and also at its bifurcation; the first part of the arch dilated. Heart greatly dilated and proportionally hypertrophied; valves healthy; the usual effects of recent acute pericarditis externally. Left lung, second stage of pneumonia at its base, with lymph on the pleura, and effused fluid.—A man, aged 41, a porter. The femoral artery tied five years before his death; ligature came away on the nineteenth day; ten days after, secondary arterial hemorrhage occurred, easily restrained by pressure *above*, and soon checked; twenty-one days after, sudden return of hemorrhage, the wound not having quite healed, *diminished* by pressure *above*, but *stopped* by pressure *below* the wound; in three days again, the wound oozed blood, but was soon checked, and he recovered entirely. Four years and nine months after, first came under Mr B. with large ulcers of the leg, and great dilatation of the superficial veins of the thigh and leg; a pulsating tumour over the profunda, size of a small orange, its contents fluid; with each pulsation a rushing thrill, distinct in the deep and superficial veins nearly to the knee, and a loud raspbruit; pressure above stopped pulsation, and quite emptied the tumour. A loud double saw-sound over the heart, louder up to the top of the sternum; a saw-sound also in the back, along the spine. The ulcers healed by treatment, but he died of acute pericarditis and pleuro-pneumonia, consequent on a drinking bout."—Pp. 22, 23.

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[1] "The initial letters placed before each communication have the following signification:—L. Living Specimen; R. Recent; P. Prepared; D. Drawing. The Pages refer to the Report Book of the Society. Vol. II."

"25. R . . . . . Dr FLETCHER. P. 238.

"ULCERATED LARYNX; the epiglottis destroyed; submucous tissue around the rima condensed; ventricles of the larynx nearly obliterated; ulceration of the true cords at their posterior extremity; general mucous membrane vascular and thickened. Lungs filled with miliary tubercles, small cavities in their apices; other viscera healthy.—A man, aged 34, suffered for some months from laryngeal phthisis, with cough, purulent expectoration, and hæmoptysis; his voice whispering. Much temporary relief from the strong solution of nitrate of silver to the larynx, his voice being restored."—P. 24.

It appears that the Society is in a very flourishing condition, and that there is no lack of interesting business at its meetings, eighty-one morbid specimens and drawings having been presented during the past year alone. This augurs well for the future; and we wish the members of the Society all the success which their efforts for the promotion of medical science deserve, and many imitators in other large provincial towns.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

FAVUS.

CASE I.—Isabella Fergusson, æt. 22, a somewhat stout servant girl, with fair skin, and scrofulous aspect, was admitted into the clinical ward of the Royal Infirmary, May 6, 1849. She states that there has been an eruption on her head for the last twelve years. Four months ago the catamenia ceased, since which time she has been subject to occasional headach, constipation, and slight dyspepsia. Nearly the whole of the scalp is covered with a thick yellow friable crust, of uneven surface, and irregular margin, emitting a highly offensive odour, like cat's urine, and causing great itching and irritation. Up to the middle of July she was treated with various internal remedies, which subdued the constipation and dyspepsia, and caused return of the catamenia. The crusts on the scalp were removed by poultices, and an ointment, composed of *ammon. mur.* ʒj.; and *ung. sulphuris*, ʒj., applied locally. I first took charge of the case on the 14th of June. The head was then again covered with favus crusts, some isolated, others compressed together, and forming an elevated scab. A small portion, examined under the microscope, presented the branches and sporules of the cryptogamic plant, so characteristic of the disease. *The crusts were again removed by poultices of linseed meal, the head shaved, and cod-liver oil ordered to be applied to the scalp morning and evening,—the whole to be covered with an oil-silk cap.* This treatment was continued for six weeks, but on suspending it the favus crusts returned. During the months of August and September iodine and pitch ointments were applied; portions of the scalp were even blistered, but without effect.

At the commencement of October, the scalp being at the time perfectly clean and closely shaved, all local treatment was suspended, and the reappearance of

the disease carefully watched. In three days the entire surface presented a scaly eruption, the epidermis being raised, cracked, and broken up over the whole scalp, which was exceedingly dry and harsh. The furfuraceous condition of the scalp continued, becoming more and more dense, until the fourteenth day, when there were first perceived minute bright sulphur-coloured spots in it. These, on being examined microscopically, were seen to be composed of fine molecular matter mingled with epidermic scales, from which delicate branched tubes were apparently growing. The crusts were now once more removed by repeated poulticing, and cod-liver oil applied as formerly.

The scalp continued free from eruption until the 20th of November, when she was seized with febrile symptoms, which ushered in a very severe attack of typhus, that ran its usual course. She was not considered fully convalescent until the 8th of December. During this period, no local application was made to the scalp, with the exception of the cold douche to alleviate the head symptoms, delirium and coma having been severe. The surface latterly once more became covered with furfuraceous scales; and on the 11th December the bright yellow minute spots again made their appearance. As her strength improved, the favus crusts increased in size and number, and the progress of this very singular disease was again carefully watched. Each individual crust, at first the size of a small pin's head, gradually flattened out, and became circular. Its centre was cupped and umbilicated, and many, which were more isolated than the rest, grew until they measured a quarter of an inch in diameter. More generally, however, they came in contact with others, and groups of twos, or threes, and sometimes a dozen, became compressed together, and presented the hexagonal form of the honey-comb. Gradually the concavity disappeared. Each crust presented an external dark ring, and an internal lighter centre, which became considerably elevated. The various groups became aggregated together, and she complained of great itching and irritation, and it was evident that, if allowed to proceed further, the condition she presented on admission would be soon produced. The crusts were, therefore, again removed by poultices, cod-liver oil once more applied, and the scalp remained clean and free from irritation until 17th January, when the cure appearing to be hopeless she was dismissed. She was enjoined to continue the use of the oil, which, whilst applied, and covered with the oil-silk cap, had the power of preventing the formation of fresh crusts on the scalp.

Case II.—Margaret Bryer, æt. 12, of scrofulous and cachectic appearance, was admitted June 19, 1849, with favus crusts on the scalp. The crusts are most numerous and dense on the crown of the head; but others, isolated, or in small groups, are scattered over the temples, forehead, and occiput. The scalp is bald, here and there, in patches, varying in diameter from half an inch to an inch. On examining the crusts microscopically, they were seen to contain the cryptogamic branches and sporules pathognomonic of favus. The disease is of three years' standing, and is attributed to the use of a comb, belonging to another girl who had a sore head. The crusts have been several times removed by means of pitch plasters and a variety of ointments, but have always returned. At first, the crusts were removed, and the scalp kept moist by means of an alkaline lotion, which succeeded in removing the irritation. Early in July she was ordered ℥ss. *of cod-liver oil, three times a-day. The oil was also directed to be applied to the shaved scalp twice daily, which was to be kept constantly covered with an oil-silk cap.* This treatment was persevered in until August 18, when she was dismissed cured.

This girl was readmitted September 5, and remained in the Infirmary five days, under observation. Up to this time the disease had not reappeared, so that, when dismissed on the 10th, a permanent cure was undoubtedly produced.

Case III.—Margaret Cameron, æt. 5, an ill-nourished, cachectic-looking child, admitted July 23, 1849, on account of an eruption on the scalp. In some

places the hair was matted together by a recent pustular eruption; groups of impetiginous pustules and eczematous vesicles being scattered here and there. In others, where the disease was more chronic, hard, nodulated, elevated masses, and friable crusts, existed. The disease was eczema impetiginodes. No favus was present, as was proved by a careful examination, and microscopic demonstrations of the scabs. *Poultices were ordered to the scalp, to remove the crusts; and afterwards an alkaline wash, with cod-liver oil internally.* My colleagues taking charge of the ward during the months of August and September, I lost sight of this patient; but, on resuming duty in the beginning of October, I was surprised to find the child's head covered with favus crusts, with the branches and sporules fully developed, as proved by the microscope. It appeared that the girl was a great favourite with Isabella Fergusson (Case I.), and frequently slept in her bed. It was, therefore, supposed that she had caught favus from her. The child's general health, however, had greatly improved; and *the crusts were ordered to be removed by poultices, the head shaved, and cod-liver oil applied locally twice daily, and an oil-silk cap to be worn constantly.* This treatment was continued for seven weeks. At the end of that time all treatment was suspended, and the scalp watched daily. In fifteen days the head was covered with a slight furfuraceous desquamation; but the hair was abundant. Another week elapsed without any return of favus; and, her health being now good, she was discharged, December 6.

CASE IV.—Margaret Adie, æt. 12, thin, and of weak appearance, admitted July 24, 1849, says that as long as she can remember she has had an eruption on the scalp, which has been treated in a variety of ways, without effect. All her brothers and sisters have been similarly affected. On admission, the entire hairy scalp is covered with prominent crusts of favus, forming an entire mass, emitting an offensive mousey odour. Here and there small portions have scaled off, leaving a bright red surface, presenting here and there impetiginous pustules. From below some of the scales a purulent fluid exudes, and she complains of occasional great itching and irritation. The parasitic nature of the crust was established by microscopic examination. *The same practice as in the last case was adopted.* She was treated by my colleagues during the months of August and September, and then discharged, to make room for a more urgent case.

CASE V.—Jane Kirkwood, æt. 6, admitted October 11, 1849. The scalp is scattered all over with bright yellow, circular, cup-shaped crusts. On the sides of the head and forehead these are more or less aggregated together, of hexagonal form, presenting in mass the honey-combed appearance. On the summit of the head they are more isolated, the intervening scalp being bald. Posteriorly there is a dense, elevated crust, in which the hairs are imbedded, and where the original form of the eruption is lost. The whole exhales the peculiar odour of the disease, and produces considerable itching and irritation. She is of a cachectic, scrofulous appearance, seems ill-nourished, and says she caught the disease three months ago, from her brother. The digestive and other functions are normal. The crusts on examination presented, under the microscope, the vegetable branches and sporules characteristic of favus. *The crusts were ordered to be removed with successive poultices, the head then to be shaved, and cod-liver oil smeared over the scalp twice a-day; an oil-skin cap to be constantly worn; a tea-spoonful of cod-liver oil to be taken three times a-day; full diet.* This treatment was continued up to November 24, when she was dismissed, and has not returned.

CASE VI.—Jessie Somerville, æt. 14, admitted December 27, 1849. She has been affected with an eruption on the scalp for two years, which has caused continual itching. Otherwise she has enjoyed tolerable health, until a fortnight since, when she caught scabies from another girl with whom she slept. On admission the scalp presents the crusts of favus in their advanced stage, forming a dense, irregular scab, below which, here and there, purulent matter escapes. Impetiginous pustules are also observable here and there. There is a papular and vesicular eruption of scabies on the hands, arms, and chest—in

the former situation, mingled with pustules of ecthyma. *The scalp was treated in the same manner as the last case, and simple lard was ordered to be rubbed over the parts affected with scabies.* The ward book says she was dismissed cured, February 5. All traces of scabies had then for some time disappeared, and the scalp remained clean. Whether the favus was eradicated is very doubtful.

Case VII.—James Scott, æt. 15, a painter, applied for advice, January 27, 1850. He states that, a week ago, without any known cause, he observed a small spot, about the size of a pin's head, over the external angle of the left malar bone. On examination a circular reddened spot, about the size of a shilling, is seen over the external angle of the left malar bone, in the centre of which were several favus crusts, aggregated together. These examined under the microscope presented the branches and sporules pathognomonic of the disease. *The whole was then cauterised with nitrate of silver*, and he never returned.

Commentary.—The seven cases of Favus above detailed, all of which have entered the clinical wards within a comparatively short time, must convince you that this is a very common disease among the lower orders of this city. They have furnished us with excellent opportunities of studying it in all its different stages, watching its mode of development, its treatment, and, indeed, most of the facts in connection with it. The disease is one which commonly lasts a very long time, even many years, is never easily eradicated when it has existed for any considerable period, and sometimes seems to be incurable. On this account it is rare that you are able to watch favus through its entire progress,—first, because it may be doubted how far a public charity of this kind ought to be made to support individuals so affected for months or years; and, secondly, because it always happens that, when urgent cases demand admission, and beds are required, these are just the individuals who are discharged to make room for them. This will account for the unsatisfactory terminations of some of the cases above reported. At the same time, having studied the appearances presented in the seven instances referred to—some of which were recent, others of some standing, and others exceedingly chronic,—you will have had little difficulty in recognising the various aspects it presents.

Favus is a disease, with the true nature of which we have been made acquainted by the modern cultivation of histology. I propose, therefore, directing your attention to it in an especial manner. But, in the first instance, I propose saying a few words with regard to eruptions of the scalp in general, because they are exceedingly common, and because great confusion has been thrown over the subject by systematic writers.

There was a period in the history of skin diseases when they were arranged in two great divisions, viz., those affecting the scalp, and those affecting the rest of the cutaneous surface. All the disorders comprehended in the first of these divisions received the name of Porrigo, a word, said by some to be derived from *porrum*, on account of the scales or concretions of the scalp, resembling the layers of an onion; by others, it is derived from *porrigere*, to spread. Willan described six kinds of Porrigo, viz., P. larvalis, P. furfurans, P. scutulata, P. favosa, P. lupinosa, and P. decalvans. It is now ascertained that none of these diseases are necessarily peculiar to the scalp,—and that, although they are more or less modified by growing among, and matting together, the hairs of that region, they may also occur on other parts of the skin. There can be little doubt, however, that the employment of the term Porrigo, as well as the corresponding word *Teigne* in France, has thrown great confusion over the subject of eruptions on the scalp. It will be well, however, as this term is still in pretty general use, to explain to you what diseases these different kinds of Porrigo really are.

Porrigo larvalis (*larva*, a mask) is really Impetigo, or Eczema impetiginodes of the scalp. The former is recognised by crusts more or less prominent or

nodulated; the latter, by the circumstance that, in addition to these nodules, there is between them a laminated or brittle crust, spread more or less equally over the surface. They are both very common in infants and children; and, as the disease sometimes extends over the face, concealing the features, hence the term *larvalis*. A very characteristic representation of impetigo capitis, which I now show you, is given in *Willan* and *Bateman*, Plate xli., erroneously called Porrigo Favosa.—See also the disease on the face. *Ibid.*, Plate xxxvii. *Alibert*, Planches 13 and 15.

Porrigo furfurans (*furfur*, bran) is really Pityriasis of the scalp, although Psoriasis of that region has also received the same appellation. There is also a peculiar form of Eczema, or Eczema impetiginodes, in which the crust is friable, and breaks up, or crumbles into minute fragments, to which the term furfurans has been erroneously applied. The true Porrigo furfurans (Pityriasis) is well represented.—*Willan* and *Bateman*, Plate xxxviii. *Alibert*, Planches 14 and 15.

Porrigo scutulata (*scutulum*, a small shield).—The nature of this disease has been much disputed. By some, it is said to be Favus (Erasmus Wilson), by others a form of Herpes (Cazenave, Neligan, Burgess). The disease is described by Willan and Bateman, and more recently by Burgess, to consist of oval or rounded, slightly elevated patches, covered with furfur, and having stunted or filamentous hair projecting from the surface. It is a form of skin eruption exceedingly rare in Edinburgh. It seems to be represented, *Willan* and *Bateman*, Plate xxxix. *Willis*, (Trichosis Scutulata.)

Porrigo favosa (*favus*, a honeycomb) is a disease, the true nature of which has been only lately determined. It consists essentially in an exudation on the skin, in which fungi or phytaceous plants grow. Round, isolated, bright yellow crusts are formed, which, when compressed together, assume an hexagonal shape. Hence the term *favosa*. It is well represented, *Willis*, (Trichosis lupinosa). *Erasmus Wilson*, Fasiculus I. *Alibert*, Planche 17.

Porrigo lupinosa (*lupinum*, the lupine).—This is the same disease as the last. The round or oval crusts when isolated, and at an early stage, present a concavity and form, resembling that of the lupine seed—hence its name.

Porrigo decalvans (*calvus*, bald).—Baldness is so common among the aged, that it can scarcely be called a disease; but when it occurs in young persons, and is circumscribed, it constitutes the Porrigo decalvans of Willan. It is said by Gruby to depend on a vegetable parasite growing in the hair. It is well represented, *Willan* and *Bateman*, Plate xl. *Willis* (Trichosis Decalvans).[1]

From this analysis of the different kinds of the so-called Porrigo, you observe that there is nothing peculiar with regard to them. With the exception of baldness, none essentially belong to the hairy scalp. True favus is far more common on the head than elsewhere; but I have frequently seen it on various parts of the cutaneous surface, and occasionally on the cheeks or shoulders, without being on the scalp at all. (Case VII.) I propose, then, abolishing this term Porrigo altogether, and, in future, making use of the term Eczema Impetigo, Pityriasis, Psoriasis, or Favus of the scalp, as the case may be. It is this latter disease that I now propose to describe to you more particularly.

History of Favus as a Vegetable Parasite.

The demonstration by Bassi[2] of the vegetable nature of the disease named muscardine in silk worms, which causes so great a mortality amongst those animals, opened up to pathologists a new field for observation, and led to the discovery, that certain disorders in the higher animals, and even in man him-

[1] It is not every plate that is published that can communicate instruction to medical students about to study skin diseases. Those I have referred to are characteristic, and may be readily examined in the reading-room of the University.

[2] Del Mal. del Segno Calcinaccio o Muscardino. Milano, 1837.

self, were connected with the growth of parasitic plants of a low type. Schönlein,[1] of Berlin, was the first to detect them in favus crusts,—an observation confirmed by Remak,[2] Fuchs, and Langenbeck.[3] Gruby[4] gave a very perfect description of these vegetations, in 1841, and made numerous researches as to their seat, origin, and mode of propagation. These were repeated by myself, and further extended in 1842.[5] In 1845 I succeeded in inoculating the disease in the human subject. Since then they have been made the subject of further investigation, by Lebert,[6] Remak,[7] Robin,[8] and numerous other inquirers, to whose observations I shall have occasion to allude subsequently.

Mode of Development and Symptoms of Favus.

By most writers, amongst whom may be cited Willan, Bateman, Biett, and Rayer, favus is described as commencing in a pustule, which breaks and forms the peculiar scab. Others, such as Baudelocque, Mahon, Alibert, and Gibert, deny its pustular nature, and state that it commences in a crust. With a view of determining the mode of its commencement, I made the following observations.

Observation 1.—All the crusts were removed from the head of a boy, labouring under the disease, by the application of poultices. In a few days the scalp was quite clean, presenting here and there, anteriorly, patches about the size of half-a-crown, deprived of hair. In these bald portions of the scalp, the skin looked somewhat injected and glossy on the surface ; but there was no pain on pressure, no abrasion on the skin, or other symptom of inflammation or local lesion. The disease was now allowed to take its natural course, and I watched its development daily. In two days, minute pustules were observed to be thinly scattered over the surface, the contents of which, when examined under the microscope, were found to consist of normal pus. In two days more, the number of pustules had considerably increased, and those formerly observed had become larger. I surrounded several of the latter with a ring of ink, in order that there might be no difficulty in following the changes they underwent, and distinguishing them from others. In another day two of the pustules under observation broke, and the matter exuded formed a scab, which, under the microscope, was found to be composed of epidermic scales and irregular amorphous masses, without any trace of vegetable structure. In the interstices of these scabs, the scalp was covered with a furfuraceous desquamation, consisting only, as shown by the microscope, of epidermic scales. On the sixth day the scabs were of a dirty yellow colour, but not of the peculiar tint or form of the porrigo crust. Only a few pustules remained, and the injected appearance of the skin was gone. On the tenth day, the head was covered with irregular agglomerated scabs, similar to those produced from impetigo. The furfuraceous desquamations also continued. On the twelfth day, I detected, for the first time, at the posterior part of the scalp, where the hair was most abundant, small bright yellow spots, the size of a pin's head, somewhat depressed below the surface. On removing one of these spots with the point of a lancet, and examining it by means of a biconvex lens of an inch focus, I found a smooth, cup-shaped, bright yellow capsule, the diameter of which was about $\frac{1}{20}$ of an inch. Its margin was continuous with several epidermic scales, which it was necessary to cut or tear through before the capsule could be removed.

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[1] Müller's Archives. 1839.
[2] Medicinische Zeitung. 1840.
[3] Comptes Rendus de la Polyclinique de Göttingen.
[4] Comptes Rendus. Tom. xiii., p. 72 and p. 309.
[5] On Parasitic Vegetable Structures found Growing in Living Animals. Edinburgh Philosophical Transactions. Vol. xv., p. 277. Monthly Journal, June 1842.
[6] Physiologie Pathologique. Tom. ii. 1845.
[7] Diagnostische und Pathogenische Untersuchungen. 1845.
[8] Des Végétaux qui croissent sur l'Homme, &c. 1847.

Having done this, it was readily separated from the parts below, except where the hair, which usually perforates these crusts, connected it inferiorly with the dermis. On pulling this out, or cutting it through, the capsule could be removed entire, leaving behind it a reddened inflamed concave depression, corresponding to the convexity of its inferior surface. On placing this capsule in a drop of water, pressing it between two slips of glass, and examining it with a magnifying power of 300 diameters, it was found to be composed of a finely granular mass, in which were numerous long-jointed filamentous tubes. These were seen coming from the edge of the capsule, as M. Gruby has described. At this time the round and oval sporules were few in number, and did not appear in groups or chains until three days later.

Observation 2.—In a boy of well-marked scrofulous habit, who laboured under favus in its most characteristic form, the crusts over the two anterior thirds, and bald portion of the scalp, were numerous, round, and isolated, but matted together posteriorly, where the hair was still abundant. When these were examined microscopically, the cryptogamic vegetations were immediately detected, as in the last case. All the crusts were removed by the application of poultices, and the head rendered perfectly smooth and clean. In three days a furfuraceous desquamation of the cuticle appeared, which became more and more abundant until the eighth day, when the small bright yellow spots of the porrigo made their appearance, not having been preceded by the formation of any pustules. The crusts were removed several times in succession, and the disease again allowed to appear ; but in this case the presence of the peculiar favus crusts was never preceded by that of pustules.

In the other cases which have come under my observation, I have satisfied myself that the formation of pustules is not essential to the disease, although they are often present. (Case I.) Hence the mistake of those pathologists who classified favus amongst the pustulæ. M. Gruby says that they are *never* present, which is equally erroneous, although they appear to be a secondary result, attributable to the irritation the disease produces in some individuals. On the other hand, I have never seen this affection produced, without having been preceded by desquamation of the cuticle, an observation which appears to me of some importance, in explaining the origin of the disease, as we shall subsequently see.

It would appear, then, that the first morbid change is increased vascularity of the skin, accompanied with a desquamation of the cuticle ; and that in a period varying from twelve to fourteen days, small spots of a bright yellow colour, like that of sulphur, may be detected. These gradually augment in size, but even at the earliest period, may be observed, with a lens, to have a central depression, through which a hair may generally be observed to pass. The crust or capsule may enlarge to about the size of a shilling, and if it be isolated, still retain its rounded form. Usually, however, its edges come in contact with other capsules, and then it loses its rounded shape, and assumes the hexagonal and honey-combed appearances described by authors. I consider, then, that the so-called porrigo lupinosa, and porrigo favosa, constituting distinct forms or varieties of some writers, are merely different stages of the same disease, and dependent upon the greater or less aggregation of the crusts. On the first appearance of the capsule, its edges are somewhat depressed below the surface of the cuticle ; but as it increases in size, they become more and more elevated, and at length are very prominent. At first, also, the whole capsule appears of a homogeneous bright yellow, but when further developed, its centre assumes a whiter colour. This arises from the aggregation of the sporules of the plant, which are more abundant in this situation. As the development proceeds, this central yellow whitish mass assumes a mealy, powdery consistence, and encroaches upon the edges of the capsule, which gradually disappear, whilst its upper concave form becomes convex, as Gruby pointed out. In general, an inflammatory ring is seen round the crust, which, as the capsule becomes elevated above the skin, enlarges, and assumes a deeper colour, indi-

cative of the increased local irritation. At length the whole cracks or splits up; all regular form is lost; a dense thick crust covers the scalp; an odour, like the urine of cats or mice, is evolved; and in chronic cases, vermin deposit their eggs in the interstices, and crawl in large numbers over the surface.

Fig. 1.

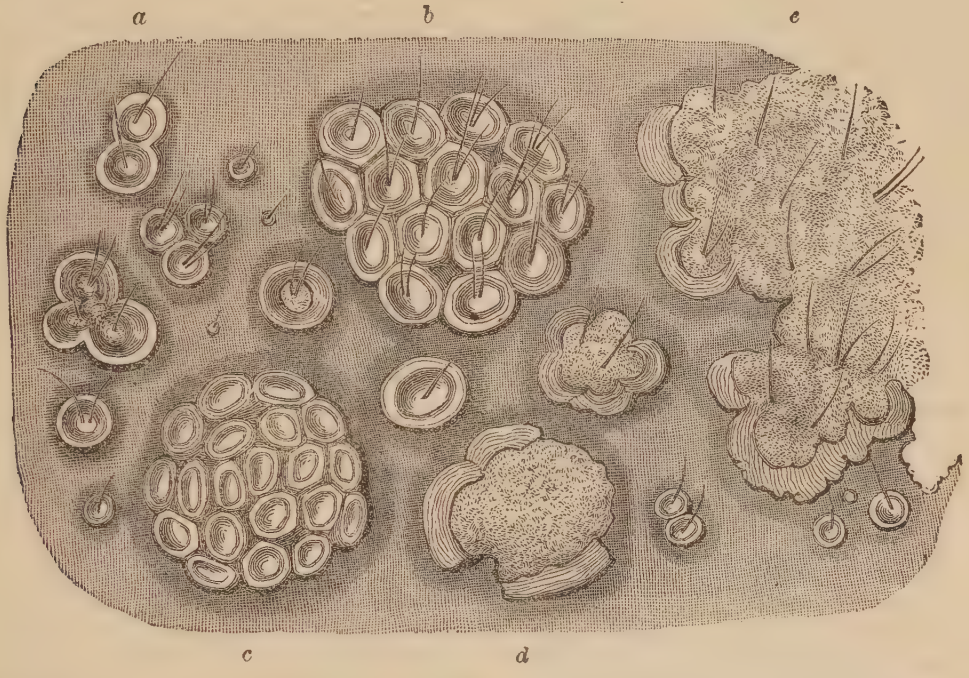


Fig. 1.—*a*, Isolated crusts of Favus, presenting the lupine seed-like depression in different stages of growth (so-called *Porrigo lupinosa*); some are arranged in groups of two and threes. *b*, A larger group of these crusts, somewhat compressed at the sides, like a honeycomb (*Porrigo favosa*). *c*, Another group, which occurred on the shoulder of a young girl. No hairs passed through the centre of these crusts. *d*, Large isolated crusts in an advanced stage of growth, the external ring is cracked, and the friable centre is enlarged and elevated. *e*, Numerous crusts aggregated together, so as to form an irregular elevated mass. Traces of the original form may be observed in the cracked rings round the margin. (Natural size.)

The other local symptoms are merely those which result from the greater or less degree of irritation produced in different persons by the changes above referred to. At first, scarcely any uneasiness is felt; perhaps occasional slight itching of the part. As the disease progresses, however, the itching becomes more intolerable, and induces the patient to rub and scratch the scalp. By these means, several of the crusts are forcibly torn from their attachments, and considerable effusion of serous fluid and blood is produced. Sometimes inflammation is thus occasioned. Impetiginous pustules are frequently formed, or suppuration produced, terminating in ulceration, and the discharge of an ichorous fluid from beneath the crusts. At an advanced stage of the disease, the peculiarly offensive odour exhaled is insupportable to those who surround the individual, and the ichorous discharge, vermin, and crusts, which cover the affected parts, present a most disgusting appearance.

Although the disease most commonly attacks the hairy scalp, it may occur on the forehead, temples, cheeks, nose, chin, ears, shoulders, arms, abdomen, lumbar region, sacrum, knees, and legs. Alibert gives a plate in which it is figured in all these situations. I have myself seen it on the cheek, shoulders, back, arms, and inferior extremities, and in some of these situations I could detect no hairs perforating the capsules.—(Fig. 1.)

The constitutional symptoms are of the utmost importance, but, generally speaking, receive little attention from practitioners. In most of the individuals affected, who have come under my notice, the general health has been greatly deranged, and a scrofulous or cachectic constitution more or less evident. In some the *facies scrofulosa* of authors has been well marked; in others

there were engorgements of the lymphatic glands of the neck ; and in the only fatal case which has come under my observation, there were found tubercular depositions in the lungs, mesenteric glands, and other textures. Indeed, the generality of individuals who die labouring under favus, perish from phthisis, or other forms of tubercular disease. The beautiful plates published by Alibert, are in this respect far from being true to nature ; for whilst the capsules and crusts are accurately drawn, the individuals affected seem to me ideal personages, enjoying the most robust health, and possessing even the utmost beauty of form and feature. In the generality of cases, on the contrary, the patient is thin, the countenance is of a dirty yellow colour, and the whole aspect betrays depression of the vital powers. The appetite is often impaired, the alvine evacuations irregular, and the functions of digestion and nutrition are impeded. Numerous writers have observed the physical and mental development of the individual to be retarded ; and Alibert gives instances where the epoch of puberty was considerably delayed.

By those not well accustomed to the diagnosis of skin diseases, favus has often been confounded with other eruptions of the scalp, more especially eczema and impetigo, or the combination of these diseases known as the eczema impetiginodes. In none of these eruptions, however, do the yellow crusts or scales present traces of vegetations when examined microscopically. This, therefore, furnishes the real diagnostic and pathognomonic character of the disease.[1]

Causes.

Alibert considered the disease hereditary, and gives cases confirmatory of this view. As regards age, it is by far most common in children between the ages of three and twelve years. In infancy, and after puberty, it is more rare, although sometimes present ; and in a few instances it has been observed in persons advanced in years. In almost all the cases which have come under my notice, the individuals have been exposed to causes which depress the vital powers, and are well known excitants of tuberculous disease. Close questioning will usually elicit that they are of a scrofulous family ; have been exposed for some time to infected or corrupted air ; inhabited small rooms, or confined streets, or dwellings situated in unhealthy situations ; that the aliment has not been very nutritive, &c. &c. Hence why the disease is common in workhouses and jails, and most prevalent amongst the poorer classes of the population, and individuals who obtain a precarious existence.

Almost every writer on the disease considers it to be contagious. Bateman, Guersent, and others, speak of its spreading amongst school-boys, from the employment of the same towels, combs, caps, &c. Gibert has seen it propagated in the wards of St Louis from the same cause. It has been observed, he says, two or three times to be communicated by young people kissing each other, when it has appeared in the chin or neighbourhood of the mouth. Mahon even pretends to have contracted favus incrustations on his fingers, from having neglected to wash them after dressing the heads of those affected. Alibert, in his early writings, also thought it to be contagious. In his later works, however, he evidently doubts it, says that much exaggeration has been made use of on this subject, and states that the *amour propre* of parents usually induces them to ascribe the origin of so disgusting a disease to external communication. He further observes, "Mes élèves ont souvent tenté d'inoculer en notre présence, le produit de l'incrustation faveuse, sous plusieurs formes, et en variant les procedés. Le plus souvent il n'est rien resulté, dans d'autres cas est survenue une inflammation passagère, qui s'est bientôt évanouie—parfois une suppuration semblable qui pourrait s'établir par tout irritant mechanique, ou par l'in-

[1] I am not aware that this peculiar disease has ever been observed in any of the lower animals. I may, therefore, mention that I have seen it on the face of a common house mouse, in which animal the same cryptogamic vegetations were to be detected as in man.

sertion d'une substance étrangère dans le tegument."[1] Gruby also, on discovering its vegetable nature, inoculated thirty phanerogamous plants, twenty-four silk-worms, six reptiles, four birds, and eight mammifera, but only produced the disease once, and then in a plant. The human arm was inoculated five times, but, independent of a slight inflammation and suppuration, no effect was produced.

Remembering the ill success of these experiments, I thought it right to try whether the disease could be propagated in another part of the individual already affected; because if not susceptible of extending in a person already predisposed, it was not likely to be caught by one in perfect health. I accordingly made a small oblique puncture in the neck of a boy labouring under the disease, about an inch below the occipital protuberance, and an inch and a-half from the large masses of crusts connected with the scalp. I introduced through this puncture, under and into the cuticle, some of the broken-down yellowish-white friable matter found in the centre of the capsules, which consists principally of the sporules of the plant. The wound healed up in a few days without presenting anything anormal. I also inoculated my own arm in the same manner, but without any result. I repeated these inoculations twice on the boy and on myself with the matter of the pustules instead of that of the crusts, but in every case without success.

It then occurred to me, that, as the disease usually appeared in the hairy scalp, it might be more readily produced in that part of the integuments. I therefore had my own scalp inoculated in two places with the pus taken from one of the pustules. It excited inflammation, suppuration, and ulceration. The matter discharged formed hard scabs, which, however, in no way resembled those of favus, or exhibited vegetations when examined with the microscope. After continuing three weeks, during which period one of the sores extended to the size of a shilling, and both ulcerations still spreading, they were destroyed by the frequent application of caustic. I subsequently had my head inoculated with the sporules of the parasite, but the wound healed up completely without producing any appreciable result.

I afterwards rubbed a mass of the white friable matter, constituted of the sporules, upon the arm, so as to separate several of the epidermic scabs, and induce erythematous redness. Slight superficial abrasions were produced, which healed in a few days, without presenting any evidence of the vegetations having germinated. I also sprinkled the sporules over an extensive accidental abrasion on the leg, which, however, healed up in the usual manner.

Thus, in none of these experiments, performed in various ways, and frequently repeated so as to avoid fallacy, could I succeed in causing the plant to germinate on parts different from those on which it was originally produced. In other words, I could not communicate the disease to other individuals, or from one part of the same individual to another.

At the time I did not consider these experiments (performed in 1841-2) as decisive of the question, although they show that it is with great difficulty inoculation succeeds. Shortly after, Dr Remak, of Berlin, communicated the disease to his own arm in the following way:—He fastened portions of the crust upon the unbroken skin, by means of plaster. In fourteen days, a red spot, covered with epidermis, appeared, and in a few days more a dry yellow favus scab formed itself upon the spot, which, examined microscopically, presented the mycodermatous vegetations characteristic of favus.[2] Mentioning this fact to my poly-clinical class, at the Royal Dispensary, in the summer of 1845, one of the gentlemen in attendance volunteered to permit his arm to be inoculated. A boy, called John Baugh, æt. 8, labouring under the disease, was at the time the subject of lecture, and a portion of the crust, taken directly from this boy's

[1] Traité des Maladies de la Peau, fol., p. 443.
[2] Medicinische Zeitung, August 3, 1842.

head, was rubbed upon Mr M.'s arm, so as to produce erythematous redness, and to raise the epidermis. Portions of the crust were then fastened on the part by strips of adhesive plaster. The results were regularly examined at the meetings of the class every Tuesday and Friday. The friction produced considerable soreness, and, in a few places, superficial suppuration. Three weeks, however, elapsed, and there was no appearance of favus. At this time, there still remained on the arm a superficial open sore about the size of a pea, and Mr M. suggested that a portion of the crust should be fastened directly on the sore. This was done, and the whole covered by a circular piece of adhesive plaster about the size of a crown piece. In a few days, the skin surrounding the inoculated part appeared red, indurated, and covered with epidermic scales. In ten days, there were first perceived upon it minute bright yellow-coloured spots, which, on examination with a lens, were at once recognised to be spots of favus. On examination with the microscope, they were found to be composed of a minute granular matter, in which a few of the cryptogamic jointed tubes could be perceived. In three days more, the yellow spots assumed a distinct cupped shape, perforated by a hair; and in addition to tubes, numerous sporules could be detected. The arm was shown to Dr Alison; and all who witnessed the experiment being satisfied of its success, I advised Mr M. to destroy each favus spot with nitrate of silver. With a view of making some further observations, however, he retained them for some time. The capsules were then squeezed out, and have not since returned. Mr M. had light hair, blue eyes, a white and very delicate skin. There is every reason to believe that the strips of plaster employed in the first attempt shifted their position, and that the crust was only properly retained by the circular piece of plaster employed in the second experiment.

That the disease, therefore, is inoculable, and capable of being communicated by contagion, there can be no doubt, a result which accords with the observations of most practitioners, and with numerous recorded facts. (Case III.) It must also be evident that it does not readily spread to healthy persons, and that there must be either a predisposition to its existence, or that the peculiar matter of favus must be kept a long time in contact with the skin.

Pathology.

I have shown, when describing the symptoms and mode of development of the disease, that it is not essentially pustular, and that the pustules occasionally present are accidental. On the other hand, it has been shown that the peculiar favus crust is composed of a capsule of epidermic scales, lined by a finely granular mass; that from this mass millions of cryptogamic plants spring up and fructify; and that the presence of these vegetations constitutes the pathognomonic character of the disease. In order to examine the development of these vegetations microscopically, it is necessary to make a thin section of the capsule, completely through, embracing the outer layer of epidermis, amorphous mass, and light friable matter found in the centre. It will thus be found, on pressing this slightly between glasses, and examining it with a magnifying power of 300 diameters, that the cylindrical tubes spring from the sides of the capsule, proceed inwards, give off branches dichotomously, which in turn terminate in round or oval globules. These tubes are from the $\frac{1}{400}$ to $\frac{1}{600}$ of a millimetre in thickness, jointed at regular intervals, and often contain molecules, varying from $\frac{1}{10,000}$ to $\frac{1}{1000}$ of a millimetre in diameter. The longitudinal diameter of the sporules is generally from $\frac{1}{300}$ to $\frac{1}{100}$, and the transverse from $\frac{1}{300}$ to $\frac{1}{150}$ of a millimetre in diameter (Gruby). I have seen some of these, oval and round, twice the size of the others. The long diameter of the former measured $\frac{1}{75}$ of a millimetre. The sporules agglomerated in masses are always more abundant and highly developed in the centre of the crust. The cylindrical tubes, on the other hand, are more readily found near the external layer. There may frequently be seen swellings on the sides of the jointed tubes, which are apparently commencing ramifications.

Fig 2.

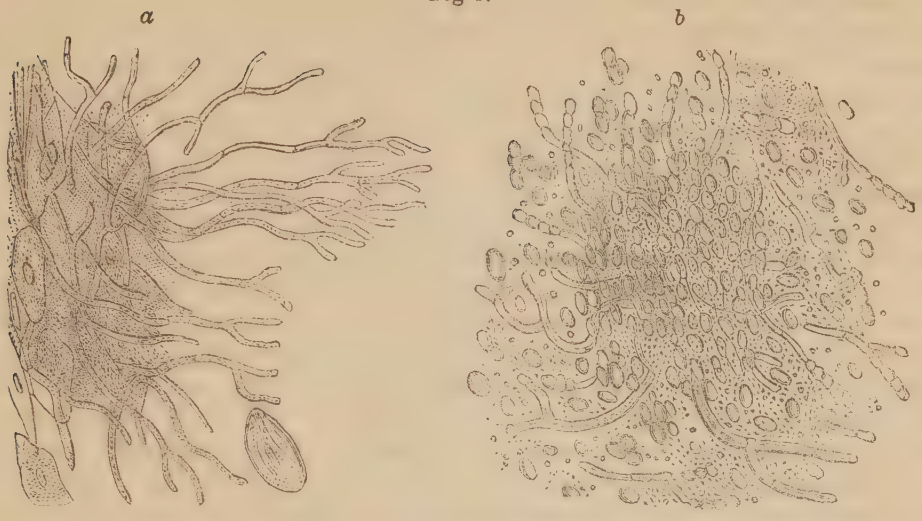


Fig. 2.—*a*, Branches of the *Achorion Schoenleini*, in an early stage of development, growing from a molecular matter, and mingled with epidermic scales, from a very minute Favus crust. *b*, Fragments of the branches more highly developed, with numerous sporules and molecular matter, from the centre of an advanced Favus crust. (Magnified 300 diameters linear.)

On examining the hairs which pass through the favus crusts, it will often be found that they present their healthy structure. At other times, however, they evidently contain long, jointed branches, similar to those in the crust, running in the long axis of the hair, which is exceedingly brittle. I have generally found these abundant in very chronic cases ; and on adding water, the fluid may be seen running into these tubes by imbibition, leaving here and there bubbles of air, more or less long. There can be very little doubt, that the tubes and sporules after a time completely fill up the hair follicle, and from thence enter the hair, causing atrophy of its bulb, and the baldness which follows the disease. The various steps of this process, however, I have been unable to follow, never having had an opportunity of observing favus in the dead scalp, and of making proper sections of the skin.

Fig. 3.

Fig 3.—*a*, A light hair, containing branches of the *Achorion Schoenleini*—(magnified 300 diameters linear). *b*, A darker coloured hair, containing branches of the plant—(magnified 800 diameters linear). The wood-cutter has made the branches too beaded.

Several writers on favus have treated its vegetable nature as a mere hypothesis. At first it was considered, as by Mr Erichsen,[1] to be "founded merely

[1] Medical Gazette, December 1841, p. 415.

upon the outward appearance, sufficiently strong certainly, which the cup-shaped crust of favus offers to lichens, or vegetations of a similar description." Subsequently favus was supposed to consist of a mass of cells; and it was argued by Dr Carpenter[1] that the vesicular organization is common to animals as well as plants; and hence "to speak of *porrigo favosa*, or any similar disease, as produced by the growth of a vegetable within the animal body, appears to the author a very arbitrary assumption." Mr Erasmus Wilson, in his work on "Diseases of the Skin" (p. 430), as well as in a special "Treatise on Ringworm," is also opposed to the idea of favus owing its essential characters to a vegetable growth. He considers that the peculiar branches and oval bodies previously described, are mere modifications of epidermic cells, which, in some cases, he is of opinion may be transformed into pus cells—in others, into those observed in favus. The branches of the plant he calls "cellated stems," and the sporules, secondary cells; and argues, that mere resemblance to a vegetable formation is not sufficient to constitute a plant. He says, "The statement of the origin of the vegetable formations by roots implanted in the cortex of the crust is unfounded; the secondary cells bear no analogy to sporules or seeds; and it is somewhat unreasonable to assign to an organism so simple as a cell the production of seeds, and reproduction thereby, when each cell is endowed with a separate life, and separate power of reproduction."[2] Lastly, M. Cazenave, in a work just published,[3] although he acknowledges himself to be no histologist, says, he has sought for the sporules many times, and believes himself authorised to conclude, that their detection is not always so easy as is supposed (p. 225). Finally, he denies that favus is a vegetable parasite, and maintains it to be a peculiar secretion, originating in the sebaceous glands (p. 236).

With the exception of Mr Wilson, who seems carefully to have examined the favus crust, the opposition to the vegetable nature of this production, seems to have originated in very imperfect notions as to its intimate structure on the one hand, and that of certain cryptogamic plants on the other. For if long hollow filaments, with partitions at intervals, containing molecules within their cells, springing from an unorganized granular mass, and giving off towards their extremities round oval bodies, or sporules, arranged in bead-like rows, be not vegetables, what are they? The animal tissues present nothing similar, while numerous plants, long known to botanists, present the same identical structure. But not only must they be referred to the vegetable kingdom, but to a considerably elevated position among the cryptogamic plants. The *protococcus nivalis* and *torula cerevisiæ*, universally considered as plants, together with the *sarcina ventriculi*, described by Goodsir, are immeasurably beneath them in complexity of structure; and many of the *mucores* or moulds growing in damp places, are, as I have satisfied myself by repeated examination, much more simple in their organism. Any one who looks over the cryptogamia of Greville, will at once detect the strong analogy between the structures found in favus, and the *penicilium glaucum* of Link, the *aspergillus penicillatus*, *acrosporium monilioides*, *sporotorium minutum*, *nostoc cœruleum*, and other plants therein figured. Indeed, it seems to me surprising, how the vegetable nature of these structures can, for a moment, be doubted by any one who has personally examined them, especially under powers of from six to eight hundred diameters linear.

In considering whether the structures described, and now by every one acknowledged to exist in the favus crusts, really belong to the vegetable kingdom, we should remember that they are not the only formations of this kind which have been found to grow parasitically in living animals. In my original paper, I described others growing in phthisical cavities, in the sordes on the gums and teeth of typhus patients; and pointed out that they had been

[1] Principles of Physiology, p. 453.
[2] On Ringworm, 1847, p. 23.
[3] Traité des Maladies du Cuir Chevelu, 1850.

Fig. 4.

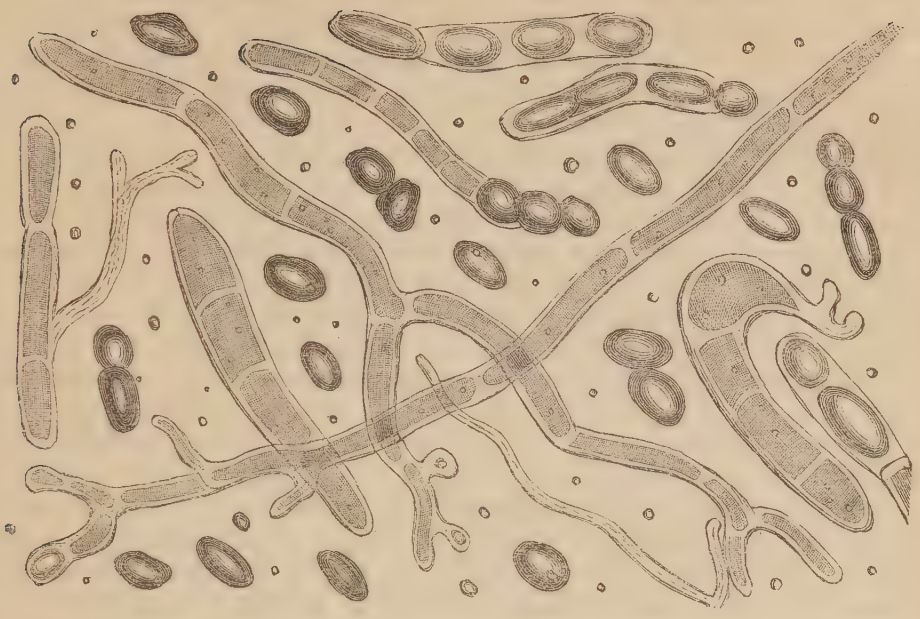


Fig. 4.—Thalli, mycelia, and sporidia, of the *Achorion Schoenleini*, showing the mode of reproduction. (Magnified 800 diameters linear.)

observed in the living tissues of mollusca, insects, reptiles, fishes, birds, and mammiferous animals. These observations have subsequently been confirmed by numerous pathologists and naturalists. Lastly, we cannot overlook the opinion of botanists themselves concerning this question. The most eminent mycologists, so far as I am aware, have no doubt of the vegetable nature of favus. Dr Greville, to whom I exhibited them, was quite satisfied of the fact. Brogniart, according to Gruby, and Messrs Link and Klotzsch, to whom they were shown by Remak, expressed a similar opinion. Brogniart considers them to belong to the genus Mycoderma of Persoon. J. Müller places them among the genus Oidium; but both Link and Klotzsch consider that they ought to constitute a distinct genus. The former, in consequence, has given it the generic name of Achorion (from achor, the old term for favus), and added to it the designation of the discoverer, Schönlein. The following is his description of the plant:—

"*Achorion Schoenleini nobis, orbiculare, flavum, coriaceum, cuti humanae praesertim capitis insidens; rhizopodion molle, pellucidum, floccosum, floccis tenuissimis, vix articulatis, ramosissimis, anastomoticis (?);*[1] *mycelium floccis crassioribus, subramosis, distincte articulatis, articulis inaequalibus irregularibus in sporidia abeuntibus; sporidia rotunda, ovalia vel irregularia, in uno vel pluribus lateribus germinantia.*"

With regard to the idea of Mr Erasmus Wilson—that they are modifications of epidermic cells—it must be observed that this touches upon a great question in pathology—namely, whether particular cells have certain definite characters and modes of development, or whether one kind of cell may be modified into others. I have frequently asked myself whether accidental circumstances might not transform an epithelial or hepatic cell into that of cancer, or whether various healthy cells might not become morbid ones. Without entering into this subject at length, I need only state my opinion that, in the present state of science, every fact leads to the conclusion, that cells, once formed from a blastema,

[1] I have never seen any anastomosis.

always advance according to laws originally impressed upon them. Consequently, epidermic cells, though they may be transformed into hair, horn, or other epidermic structures, cannot be changed into pus cells on the one hand, or into those resembling vegetable growths on the other. If this view be correct, it follows that we must regard the branches and sporules in the favus crust, as consisting of true vegetable parasites, and as cryptogamic plants, growing in the animal textures.

The seat of favus has been much disputed by authors. By some it has been located in the piliferous bulbs or follicles (Duncan, Baudelocque, Rayer), by others in the sebaceous glands (Sauvages, Underwood, Murray, Mahon, and lately by Cazenave), and a third party in the reticular tissue of the skin (Bateman, Gallot, Thompson). According to Gruby, the plants grow in the cells of the epidermis, the true skin is compressed, not destroyed, and the bulbs and roots of the hairs and sebaceous follicles are only secondarily affected.

I have made observations to determine the correctness of this statement, and found that the whole inferior surface of the capsule is formed of epidermic scales, thickly matted together. These are lined by finely molecular matter, from which the plants appear to spring, and which unites the branches and sporules together in a mass. Superiorly, however, the epidermic scales are not so dense; and I have always found them more or less broken up, and not continuous. This observation is valuable, as indicating the probable mode in which these plants, or the sporules producing them, are deposited on the scalp. It will be seen, that the appearance of the peculiar porrigo capsule, was invariably preceded by a desquamation of the cuticle, that is, a separation or splitting up of the numerous external epidermic scales which constitute its outermost layer. Hence it is more probable, that the sporules or matters from which the vegetations are developed, insinuate themselves between the crevices, and under the portion of epidermis thus partially separated, than that they spring up originally below, or in the thickness of the cuticle.

M. Cazenave, in his recent work, supports the opinion, that the sebaceous glands are the primary seat of favus. He states that the disease *always* forms round a hair, and is never produced if the follicle be obliterated, or on a cicatrized surface devoid of hair. He considers the matter of favus to be a peculiar secretion, or a modification of the normal matter secreted by the sebaceous glands, analogous to what exists in *acne sebacea* (p. 234.) It is true that the favus crust most frequently, although not invariably, forms first round the external orifice of the hair follicle, for it can easily be removed from this situation, and the hair is then seen to perforate it, whilst its bulb is unaffected. As previously stated, it is later that the hair and its bulb contain jointed tubes. It cannot be denied, therefore, that M. Cazenave may be correct in his conjecture, although it is much to be regretted that, in a matter of this kind, he has not dissected the skin, made sections of it in proper directions, and *demonstrated* the fact, if such be really the case.

The real constitution of the matter originally exuded, M. Cazenave supposes to be of a fatty nature, but it appears to me to be more probably albuminous, and allied to the molecular character of all broken down or disintegrated organic material in which fungi grow. We have seen (Obs. I. and II.) that, previous to the return of favus crusts, the head is always covered with broken-up epidermis, more or less disintegrated. Experiments have shown that the plants will not grow on the healthy skin, and that inoculation succeeds only in places where pustules have previously been formed. It is also exceedingly probable that, when favus is communicated from one person to another, the part affected (generally the scalp) has been the seat of some other eruption (Case III.), or is not particularly clean.

Mr Erichsen considered, "That the matter of favus is a modification of tubercle—that it is a tubercular disease of the skin. By tubercular I do not mean a disease like lupus, characterised by small firm tumours, but a disease, the nature of which consists in the deposition of that *heterologous* formation

called tubercle." This view of the nature of favus I have long held; and it was distinctly stated by me, when treating of the pathology of scrofula, in a work published in 1841.[1] The favus crust however, is not constituted alone of tubercular matter. This peculiar exudation only constitutes the soil from which the mycodermatous vegetations spring, as I shall now endeavour to show.

Gruby describes the mycodermata of favus as springing from an amorphous mass, of which the periphery of the capsule is composed. This mass undoubtedly exists, and, according to my observations, is composed of a finely molecular matter, identical in structure to certain forms of tubercle, or recently coagulated exudation. The cheesy matter, for instance, so frequently found on the secreting surface of serous membranes, and in tubercular cavities and other structures in chronic cases of tuberculosis, or general tendency to tubercular deposition, presents this character. Every pathologist who has minutely examined tubercle, recognises a granular form, in which there is no trace of nucleus or cell, and which, therefore, we are warranted in considering as unorganised. I have myself repeatedly examined this tubercular matter, and been unable to detect any difference between it and the mass in which the vegetations of favus appear to grow. Chemical analysis of this form of tubercle demonstrates it to be composed principally of albumen, with a minute proportion of earthy salts; sometimes there is combined with it a small quantity of fibrine or gelatine. If this general result be compared with the analysis, by Thenard, of favus matter, the identity between it and tubercle must appear highly probable. He found in 100 parts—coagulated albumen, 70; gelatine, 17; phosphate of lime, 5; water and loss, 8 parts. Thus the evidence furnished by morphology and chemistry, agree in determining the molecular matter found in the crusts of favus and of tubercle to be analogous.

All the facts which recent researches have brought to light, only tend to confirm the conclusion which I arrived at on this subject in 1842, as expressed in the following passage :—

"In man all the vegetations yet discovered have been found connected with the matter effused into the textures in scrofulous constitutions. The fungi found by myself, for instance, growing in the tuberculous cavities of the lungs, and those discovered by Schönlein, and described by Gruby, constituting scrofulous eruptions on the skin, grew on a finely granular amorphous mass, which presented no evidence of organisation. Chemical researches have shown, that this form of tubercular matter is principally composed of albumen,—which explains the large proportion of this animal principle present in the crust of the porrigo or tinea favosa, according to the analysis given by Alibert. The fungi found by M. Eudes Deslongchamps growing on the membranous lining of the air-passages in an eider-duck, sprung from an 'albuminous layer,' 'forming the soil on which they grew.' The mould, or mucor, discovered by Owen growing in the lungs of the flamingo, occupied the same situation as those observed by myself in the lungs of man,—viz., the lining membrane of tubercular cavities. The fungi found by MM. Rousseau and Serrurier in the parroquet grew on a species of false membrane. What the nature of this membrane was is not stated, but it is distinctly mentioned that the animal died of laryngeal and pulmonary phthisis. In pigeons, also, the same authors describe it as commonly induced by exposure to cold and moisture, circumstances well known to be the most common cause of scrofula and of tubercular depositions. According to the observations of Valentin, the parasitic confervæ found growing on fish are connected with a diseased state of the animal, and are induced by keeping them in narrow vessels and foul water. The gold fish was evidently unhealthy which furnished the vegetations which I have myself described; and I have shown that these were connected with a granular, inor-

[1] Treatise on the Oleum Jecoris Aselli, p. 94.

ganic, albuminous matter, identical with that found in the lungs of phthisical individuals, and in the crusts of porrigo favosa. The salamanders and frogs in which confervæ grow, as described by Hannover and Stilling, were decidedly in an unhealthy state, induced by circumstances which must necessarily impair the vigour of animal life, and induce cachexia. Vegetations attach themselves to, or grow on, insects generally when in a chrysalis state—that is, when the powers of life are sluggish or dormant. When seen in these animals during the most perfect period of their existence, they evidently laboured under disease, and soon perished. This was distinctly observed in the vegetable wasp of Gaudaloupe by Madianna, and in the dytiscus marginalis by Busk. Lastly, the discovery of M. Bassi has demonstrated that vegetations occur in silk-worms affected with the muscardine,—a disease which causes a great mortality among these animals; and the researches of M. Ardouin have shown that these vegetations are formed at the expense of the adipose tissue. Whether tubercular matter was present in the worms is not stated; but we know that the disappearance of fat is one of the constant symptoms attendant upon imperfect nutrition."

I believe, therefore, that the pathology of favus is best understood by considering it essentially to be a form of anormal nutrition, with exudation of a matter analogous to, if not identical with, that of tubercle, which constitutes a soil for the germination of cryptogamic plants, the presence of which is pathognomonic of the disease. This matter may be found primarily or secondarily. That is, there may be want of vital power from the first, as occurs in scrofulous cases, or there may have been production of cell forms, such as those of pus or epidermis, which, when disintegrated and reduced to a like molecular and granular material secondarily, constitute the necessary ground from which the parasite derives its nourishment, and in which it grows.

Treatment.

Almost every species of treatment has been had recourse to, in order to remove this disagreeable and intractable disease; and there can be no doubt, that cases have been cured by all or any of the methods recommended. In some instances, favus wears itself out, or rather, as the development of the frame proceeds, and the constitutional strength improves, the conditions necessary for its production and maintenance are removed, and it consequently disappears. In every case, however, it must be our object to get rid of the disease permanently as soon as possible, and this is only to be done by removing the pathological conditions on which it depends.

For the most part, practitioners have trusted to local remedies, and every medical man had his favourite application in such cases. Citrine, sulphuric, tar, and iodine ointments, are those most commonly used in this country,—all of which, it is stated, are highly successful. No exact results, however, so far as I am aware, have been arrived at, nor has any particular treatment been tried in Britain on such an extensive scale as to induce any great confidence in its success.

The notion that it originates in the bulbs of the hair caused an attempt to remove the disease by eradicating the structures with which it was supposed to be connected. Hence the barbarous and cruel treatment by means of the *Calotte*. A modification of this plan was extensively practised in Berlin in 1841. In Paris this practice has generally been put aside, for the milder empiric treatment of the frères Mahon. Between the years 1807 and 1813, 439 girls, affected with favus, were cured by them at the Bureau Central des Hôpitaux, and the mean duration of the treatment was 56 applications. During the same lapse of time, 469 boys were cured, and the mean duration of the treatment was 56 applications. These applications are generally made every other day, so that the average length of treatment by this much boasted and successful method is three months and a-half.

I have endeavoured to show, however, that in many cases it is a constitu-

tional disease, and dependent upon the causes which induce scrofulous diseases in general. The treatment, therefore, in such ought to be constitutional, and directed to remove the tendency to tubercular exudation, on which the malady depends. No doubt, however, a local treatment in this, as in all disorders which are at the same time general and local, is of the utmost service.

I consider, then, that the chief indications of treatment are, 1st, To remove the constitutional derangement; and, 2dly, To employ such topical applications as tend to prevent the development of vegetable life. This line of practice may be thought similar to that recommended long ago by Lorry, who advises, 1st, A modification of the fluids and solids of the economy by a general treatment; 2dly, A vigorous attack upon the local disease by topical applications, capable of removing the crusts, causing the skin to suppurate deeply, and substituting a solid cicatrix for the morbid ulceration of the hairy scalp. For the most part, however, the general treatment of physicians has been confined to diluent drinks, blood-letting, purging, and remedies which depress the vital powers, whereas it must be evident, that if the views of its pathology I have brought forward be correct, and it is in its nature allied to tubercular affections, a treatment exactly opposite ought to be pursued. The development of vegetable life may also be prevented by the application of much milder remedies than the escharotics, or irritating ointments usually employed.

The treatment of scrofula will be fully entered upon by me in a subsequent lecture. Suffice it to say at present, that I shall endeavour to show how this peculiar cachexia is caused and kept up by some fault in the digestive process; that the blood is secondarily affected, and its albuminous constituents proportionally increased; that the albumen at length becomes effused into the different structures of the economy, causing the various forms of tubercular disease; and, lastly, as the albumen in the blood becomes excessive, and its effusion into the textures increases, the fatty constituents of the frame diminish. It will be shown, by an appeal to numerous facts, that under such circumstances the internal and external exhibition of cod-liver oil has been attended with the most marked advantage, and often been made the means of cure when all other remedies have failed. The action of the oil appears to be the same in favus as in other forms of scrofulous disease, and its use should be combined with appropriate diet and exercise, and with reference to the same indications and contra-indications. As it is my intention to describe these minutely on some other occasion, it is unnecessary for me to allude to them at present.

The local treatment I have employed for several years, is directed, in conformity with the pathological views previously detailed, to the exclusion of atmospheric air, so as to prevent vegetable growth. For this purpose, I direct, in the first instance, that the affected scalp should be poulticed for several days, until the favus crusts are thoroughly softened, and fall off. Then the head is to be carefully shaved, after which it will be found to present a shining clear surface. Lastly, cod-liver oil should be applied with a soft brush, or dossil of lint, over the affected surface morning and night, and the head covered with an oil-silk cap to prevent evaporation, and further exclude the atmospheric air. Every now and then, as the oil accumulates and becomes inspissated, it should be removed by gently washing it with soft soap and water. It is very possible common lard would do as well as cod-liver oil; and I observe that Dr Girot has lately recommended olive oil as a local application.[1]

I have found the average duration of this treatment to be six weeks, which contrasts very favourably with the results of M. M. Mahon's practice at the Hôpital St Louis. Some cases seem to be incurable, and these are most frequent among adults, but even in them, so long as the scalp is kept moist with oil, and

[1] Gazette des Hôpitaux, May 18, 1850.

excluded from the air, the eruption will not return (Case I.). In young subjects, in whom general as well as local treatment is admissible, and in whom a scrofulous disposition is manifest, the prognosis is more favourable, and the disease may be permanently eradicated. (Cases II. and III.) Whenever favus is recent and of limited extent, it may at once be destroyed by cauterization with nitrate of silver. (Case VII.)

POMPHOLYX.

Mary Little, æt. 46, married, admitted June 20, 1849. Says that, two and a-half years ago, and before the birth of her last child, an eruption appeared on her neck, shoulders, and sides. This disappeared spontaneously, and was followed by an eruption on the soles of the feet, similar in character to the former one. She states that the affected parts were painful and swollen for three months; they then became red, and vesicles, about the size of a finger nail, containing clear fluid, rose from the surface; these burst and formed a laminated scab on the surface. The eruption of the vesicles was always attended with considerable pain, which diminished on their breaking. She has been subject to stomach complaints for three years, and these preceded the local disease. On admission, she complains of burning heat in the soles of her feet. On examination, the sole of the left foot is swollen, and of a purplish red over the whole surface, with groups of vesicles, varying in size from a pin's head to that of a large pea, and now distended with pus. The two anterior thirds of the left sole are similarly affected, but the skin covering the heel is unaffected. The tongue is unusually clean and large, and covered with a network of deep fissures. She has an unpleasant taste in her mouth, with some acid eructations. The appetite is good, no thirst, bowels regular. Occasionally there is pain in the epigastrium, and severe headach. Menstruation irregular. Other functions healthy.

It would be tedious to give the daily reports of this case during the time she remained in the house, a period of nearly seven months. The dyspeptic symptoms were treated in a variety of ways. Alkalies, with vegetable bitters and tonics; mineral acids and arsenic. At the end of November she was attacked with typhus fever, which was severe, and from which she recovered slowly. During this period she underwent, first, a febrifuge treatment; and, subsequently, was put on vegetable and mineral tonics, chalybeates, with nutritive diet, and wine. She also went through a course of the iodide of potassium and of mercury. The dyspepsia, when she left the hospital, had nearly disappeared; but the tongue remained fissured, and unusually clean.

Locally, the soles of the feet were kept continually moist with an alkaline wash; subsequently lotions of diluted nitric acid, acetate of lead, and nitrate of silver were employed. During the attack of fever all local treatment was suspended, and it was observed that no eruption appeared on the feet. As she became convalescent, however, the pompholyx returned, and the solid nitrate of silver was rubbed over the parts so as to produce a thick eschar. Subsequently, as a crop of the vesicles appeared, these were punctured, and each freely cauterized with the nitrate of silver. All these remedies, however, proved of no avail, and she was discharged January 26, 1850.

Commentary.—Pompholyx is, on the whole, a very rare disease. It consists of successive crops of vesicles on a reddened and swollen surface, which, in a few days, become filled with pus, break, and form a laminated scab, that in the course of twelve or fifteen days drops off, leaving the skin red, but perfectly smooth. In a period, which varies considerably, another crop makes its appearance, and runs a similar course; and so the disease continues indefinitely. In the "Clinique" of Professor Barry, of Berlin, I once saw a case very similar to the one above detailed. The seat of the eruption was also in the soles of the feet, and it had continued twenty years. It is evidently a constitutional affection: no local treatment seems to be of any service. It is usually associated with

derangement of the stomach and dyspepsia, but often continues, as in the present case, when these are alleviated or removed. I was in hopes that the typhus fever would have so operated on the constitution as to have cured the pompholyx; but although it was interesting to observe that, during the course of the fever, the succession of the crop of vesicles was interrupted, still no permanent impression was made on the disease. Everything that could be thought of was tried. The dyspepsia was much relieved; and having given a fair trial to almost every known tonic and alterative, she was discharged as incurable.

Most dermatologists have recorded the utter failure of all local remedies in this disease. Dr Graves, however, describes a case of five years' standing, which yielded to the application of nitrate of silver to the punctured blebs. This practice was fully tried in the woman under our observation without success.

ECZEMA.

Case I.—Donald Menzies, æt. 40, a painter, admitted October 8, 1849. States that he has had an eruption on his hands for twenty years. He attributes its origin to mixing paint with them. He says the disease began with redness, followed by cracking of the skin: the hands being often stiff and swollen. He has undergone many kinds of treatment. On admission, the backs of both hands are red and swollen: the skin is indurated, and rough to the feel, and covered here and there with patches of laminated crusts. Between the fingers, and over several of the joints, are deep cracks with indurated margins. There is intense itching in the affected parts, which often prevents sleep. Functions generally natural. ℞ *sodæ subcarb.* ʒij.; *aquæ font.* ℥xx., *solve, fiat lotio.* Lint, saturated with the wash, was ordered to be applied to both hands morning and night, and the whole to be covered with oiled silk to prevent evaporation. *October* 22*d.* Hands are now smoother and softer; the itching disappeared from the commencement of the treatment. Cracks evidently healing. He was dismissed from the house, November 9th. This man continued the treatment for about four weeks after leaving the Infirmary; at the expiration of which time the cracks had disappeared, the skin was soft, and he might be considered perfectly cured. Up to this time, June 12, 1850, he has remained free from the disease.

Case II.—Anne Murphy, æt. 26, widow, admitted December 19, 1849. States that she has enjoyed good health up to five months ago, when she fell down and struck her elbow a severe blow. A month afterwards, there appeared on the arm minute pimples, which spread over the whole extremity from the shoulder to the wrist. Thin scabs were formed, which, on separating, were succeeded by others. On admission, her general health is good. The right arm is covered with laminated scabs, below and between which the skin presents a dark red colour. There is considerable smarting, and often burning pain in the affected skin. In many places, groups of minute vesicles can be observed. The same lotion as in the last case was ordered; lint, saturated with which, was to be applied to the affected parts, night and morning, and the whole covered with oiled silk to prevent evaporation. From this time all irritation ceased, the eruption gradually disappeared, and she was discharged cured, January 24th.

Commentary.—Eczema essentially consists of diffuse crops of minute vesicles, seated on an inflamed red surface, which, on breaking, yield a thin serous looking fluid that coagulates on the surface, so as to form a flat laminated scab. There is almost always intense itching, or a feeling of burning or smarting, often very painful; symptoms most intense in acute, but also present in chronic, cases. In these last, the skin often assumes an unusual degree of induration, a more or less deep red colour, and presents cracks and fissures, giving rise to great uneasiness. The first case above detailed is one of this kind, and presented all the characters of extreme obstinacy. The second case was

more acute. Both were treated with local applications only, and with the best results.

The application I employ is a solution of ʒij. of the subcarbonate of soda, in a pint and a-half of water. But an alkaline lotion in itself, applied from time to time, as is most commonly done, will not prove of any great service. I have found it necessary to place lint, saturated with the solution, over the affected parts, and to cover the whole with oil-silk, in order to prevent evaporation. Keeping the surface moist seems to me a necessary part of the treatment. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean, and prevent the accumulation of those scabs and crusts which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their fiery red colour, merge into that of the healthy skin, and finally the whole surface assumes its normal character.

I am quite satisfied that this local treatment is far superior to the stimulating ointments and lotions, which are so frequently used one after the other, in the hope of overcoming these obstinate forms of eruption. Many of them, indeed, only seem to augment the disorder. In these cases there is an increased exudation from the skin, not only of sebaceous but of purulent matters. Alkalies, we know, have the property of dissolving these, and acting as a calmative and emollient to the irritated part. Their constant action on the skin, therefore, in the manner I have described, may easily be imagined to be productive of benefit. But if, as is usually done, alkaline lotions and baths are only employed from time to time, they remove the sebaceous matter, leave the skin dry and harsh, and thus these applications often irritate. Keeping the surface covered with moist lint, on the other hand, while it serves to protect, removes the results of exudation, and prevents incrustation.

I do not wish it to be understood that this is the *only* local application which can be useful in these cases, but that I have found it more extensively applicable, and more uniformly serviceable, than any other.

PSORIASIS.

Case I.—Thomas Brown, æt. 36, baker, admitted July 20, 1849. He has a scaly eruption over the inferior extremities, and various parts of the trunk. On the thighs and legs it forms large diffuse patches; and here, as well as on the trunk, there are also smaller spots, varying in size from that of a pea to that of a five shilling piece. The tongue is furred, the appetite capricious; otherwise the digestive system, and the other functions, are normal—℞ *Liquor. Arsenicalis gtt.*, xv.; *Aquæ font.*, ʒiij. *M. ft. Mist.* An ounce to be taken three times a-day. To rub simple lard on the affected parts morning and night. *August 8th.*—Eruption, freed of scabs, and the red colour not so deep. Pitch ointment ordered to be applied to the affected parts. *August 24th.*—It was found that the pitch ointment caused so much irritation that it had to be discontinued, the simple ointment, therefore, resumed, and the arsenical solution continued. *September 13th.*—All traces of the eruption had now disappeared, and he was dismissed.

Case II.—Agnes Watt, æt. 36, a washerwoman, admitted August 15, 1849. About eight months ago, on recovering from an attack of cholera, red spots appeared on various parts of the body, which subsequently became covered with whitish scales. These have increased in size and number until now. On admission, the body and limbs are covered with patches of a scaly eruption, seated on a red surface. A diffuse patch, the size of the hand, is seated over the right ilium. The inferior and superior extremities, as well as the trunk, are scattered over with patches varying in size from a fourpenny piece to that of the palm of the hand—the larger present an annular form. All the characters described under the names of Lepra vulgaris, Psoriasis diffusa, and P. guttata, are observable in this individual. The eruption has occasionally caused

itching and irritation, sensations at present originating in the hip, knees, and elbows. Digestive system in good order; urine loaded with lithates; other functions normal.—℞ *Liq. Arsenic*, ʒj.; *Tr. Lyttæ*, ʒj.; *Aquæ* ʒv. *M. ft. Mist.* A tablespoonful to be taken morning and evening. *Ung. Picis* to be rubbed over the surface night and morning. On the 24th of August, the arsenical mixture was suspended from its causing constitutional symptoms, and the ointment at first induced some local irritation. Gradually, however, the skin became soft, the redness of the eruption faded, and she was dismissed cured, October 24, 1849.

Case III.—Catherine Balderston, æt. 15, a servant, admitted December 4, 1849. Eight months ago a scaly eruption appeared, first, on the inferior, and then on the superior extremities. She entered the clinical wards last August, and, after five weeks' treatment with pitch ointment, went out, the eruption having nearly disappeared. Fourteen days after dismissal, however, it was as intense as before, and has continued until now. On admission, the superior and inferior extremities are covered with the rings of lepra vulgaris, mingled with patches and round spots of psoriasis. The former are largest about the knees and shoulders. They occasion itching, but no pain. The digestive and other functions are normal. Pitch ointment was ordered to be applied to the affected surface twice daily. The scabs rapidly disappeared, the reddened spots faded, the skin became soft, and on February 2d she was dismissed without a trace of the disease.

Commentary.—So-called lepra vulgaris, psoriasis diffusa, psoriasis gyrata, psoriasis guttata, and psoriasis inveterata, are one disease, more or less chronic. In the works of systematic writers, the varieties of this affection are multiplied indefinitely, even to giving minute separate descriptions of it, as observed on the eyelids, lips, prepuce, palms of the hands, nails, &c. &c. &c. The sooner you disembarrass yourself of all such useless distinctions the better.

The real nature of psoriasis is altogether unknown. I am not aware that any researches have been made as to the alterations which the structure of the skin undergoes in the disease. All that can be said is, that, as we see a considerable redness, there can be little doubt of there being a congested state of the vessels, combined with increased amount and desquamation of the cuticle. There is no exudation, properly so called, but an increased growth, or an hypertrophy of the epidermis.

Observation has shown that this, like many other skin diseases, is sometimes connected with a deranged state of the digestive organs, sometimes with a constitutional disorder, very little understood, whilst in others (but these are comparatively few) it is more or less dependent on local causes. A judicious treatment of this disease will materially depend on a correct appreciation of one or the other of these circumstances, or of their various combinations.

The constitutional treatment which I have found to be most efficient in cases of psoriasis, is the administration of equal parts of Fowler's solution, and of tincture of cantharides, in doses commencing with ten drops, gradually increased to fifteen or twenty. The effects of this medicine must, of course, be carefully watched. Of all the local applications, most dermatologists are agreed that the best is pitch ointment. It seems to exert a specific action on the skin in these cases, and of its value there can be no question. You will occasionally find, however, that it causes considerable irritation, in which case it should be diluted with an equal part of lard. In Case I. this occurred, and simple ointment only was employed, which, combined with the internal use of arsenic, was sufficient to produce a cure. In Case II., the arsenical and cantharides drops, combined with pitch locally, brought about recovery; whilst, in Case III., the same result was obtained by the local application alone. Such is a fair specimen of what you may expect in the treatment of this disease. Some cases getting well under arsenic alone; others, under pitch ointment alone; whilst a third class will require the action of both.

LECTURES ON GENERAL THERAPEUTICS.

By Douglas Maclagan, M.D., F.R.S.E.

(*Continued from page* 471., Vol. X.)

Lecture IV.—mechanical remedies—demulcents.

In now entering upon the consideration of the different classes of remedies in detail, let me mention to you the general plan which I propose to follow in treating of each class. I shall first state to you what is known of their physiological action,—then what therapeutical effects they are calculated to produce,—and illustrate these by mentioning some of their uses and applications in practical medicine,—and then I shall notice any bad effects which may result from their employment. I do not pledge myself always to follow this order of discussion; but I shall, as far as possible, do so for the sake of brevity and method.

Mechanical Agents.—These, as their title implies, act, in the first instance, in accordance with the ordinary laws of physics. They might be made to produce their primary, or purely mechanical action, on the body dead as well as living, and so far, therefore, may be said to be independent of vital action. Sooner or later, however, under the influence of mechanical remedies when applied to the living body, changes in the vital actions of the part must follow; and thus, after all, their action, which at first is mechanical, becomes vital, and it is to their secondary effects on the vital actions that their therapeutical effects are essentially due. Thus, an incision into the seat of an abscess is itself a mere mechanical effect of an instrument; but the cure which ultimately follows is essentially vital. The pus which distended the part being discharged, the irritation to which its presence gave rise, and the consequent symptoms, subside; the nerves, freed from the pressure of the pus, and the heat of the engorged blood-vessels, are relieved from the throbbing pain; the sac of the abscess collapsing, and its walls coming together, take on a new action, they adhere, the cavity becomes obliterated, and the cure is complete. These, then, are vital actions following the incision and the discharge of the pus, which in themselves were purely mechanical.

Pressure, also, is in itself purely mechanical, but it produces therapeutic results by modifying vital action in the part to which it is applied, and the same is true with regard to friction. Whatever good effects are produced by these remedies, are in great measure due to their action on the vessels, and to their retarding or promoting the flow of blood through the tissues. By their thus modifying the state of the circulation locally, the functions of nutrition and absorption in the part are modified also, and thus the removal of effusions of solid matter and of serum in external parts, the diminution of swellings, and even the arrestment of inflammation of an acute character, may be effected by these mechanical remedies. The cure of a chronic bubo (effusion of solid matter), or of a hydropic joint (effusion of serum), by bandaging, or the treatment of swelled testicle (acute orchitis) by strapping, but which, though I mention it, I do not recommend as a surgical measure, may be cited as examples of the effects of pressure.

In the restoration of warmth to a part benumbed by exposure to cold, and the re-establishment of the circulation in a person rescued from drowning, by rubbing the surface, and the relief effected in local rheumatic attacks by the process of shampooing, we have illustrations of the use of friction; and in all these cases, the effects produced are, I believe, mainly to be traced to the mechanical action on the blood-vessels of the part;—pressure acting by preventing the free ingress of blood, and friction promoting the circulation, by forcing the blood to flow more freely through the vessels.

When I placed before you, however, in the tabular view of the classification

of remedies, friction, percussion, pressure, and instruments, as examples of mechanical agents, I did so only with a view to assigning them their place in the classification, and not for the purpose of discussing them at present. For these do not constitute classes, but are individual remedies, and whatever I have to say of them will be submitted to you when we treat of imponderable remedies in detail. We shall then have an opportunity of considering the special uses of pressure and friction. Percussion may be dismissed at once, for it is now only employed as a means of diagnosis, and has ceased to be used as a remedy, though it is not long since it was thought that ovarian dropsy might be cured by it. To say anything more of instruments, than that we rate them in our classification as mechanical means, would be to enter into discussions foreign to the present course of lectures; and thus, in our division of remedies, as relates to general therapeutics, there remain demulcents, which alone form a class of remedies, and to them I shall now direct your attention.

Demulcents.—By this term (*demulcere*, to soothe), we mean bland matters which operate by mechanically sheathing and protecting the tissues. In a great many works on materia medica and therapeutics, you will find that the word demulcent is used as synonymous with emollient; but you will see that, in the arrangement of remedies which I have adopted, they have been completely distinguished and widely separated from each other. And so they ought to be; for an emollient implies something which has the power of relaxing, and thereby, as it were, softening contractile fibres—an action essentially vital; whilst a demulcent has no such effect at all, and merely protects the tissues by mechanically covering them. A warm poultice, applied to an abscess, for example, is correctly denominated an emollient; it exerts a relaxing, or, so to speak, softening effect upon the vessels of the inflamed part, and by thus modifying their vital contractility, lessens the inflammation, deadens the throbbing pain, and promotes resolution, or aids in expediting the maturation of the suppuration. On the other hand, a piece of gum or a lozenge, sucked in the mouth in a case of catarrh, is a demulcent. It does not in itself do anything towards curing the slight inflammation which affects the mucous membrane of the throat and glottis. If this slight inflammation require any direct treatment at all, it must be sought for in other remedies; all that the demulcent professes to do, is to diffuse itself, in the act of swallowing, over the mucous membrane of the glottis; it protects the part from the direct contact of the inspired air, and thus the patient can draw a breath without having cough excited by it. You constantly hear people say, that they have had a slight cold, which they have cured by a little extract of liquorice, or a few jujubes. The practice has been good and effectual, but the therapeutical explanation is wrong. The jujube did not, in strict propriety, cure the cold; it was a small ailment which required no treatment; it subsided in a few hours spontaneously. Had it been so severe as to have required treatment, the jujube would not have sufficed for the purpose. Was the lozenge then of no use? Certainly it was. It sheathed the slightly inflamed parts; it prevented the inspired air from exciting the feeling of rawness in the throat, which caused the patient to hawk or cough, and thus he was kept comfortable till his little malady had subsided. Such was the mechanical action of the demulcent.

The body possesses naturally several demulcents, which are in constant operation in preserving its comfort. The tears are the demulcent of the conjunctiva. Their epithelial coverings, watery exhalations, and mucous secretions, are the demulcents of the nose, air passages, and digestive canal. The epidermis is a demulcent which protects the exquisitely sensitive *cutis vera;* and when we have recourse to a demulcent remedy, it is for the purpose of supplying a want of some of these natural protectors. Keeping this in view, you will at once see what are the kinds of matters which are likely to be useful in this respect. They are substances which, in themselves, have no irritant or other active qualities, but which have mechanical properties rendering them easily applied to the parts which they are intended to protect. They are such slightly adhesive

articles as gum, sugar, and gelatine, bland fixed oils and fats, and some soft fabrics, such as cotton wool and goldbeater's skin.

The applications and uses of demulcents may be illustrated by the following examples:—

They are used to sheathe and protect mucous surfaces, when their own proper secretion is either wanting or is insufficient for their due protection. In the case of the catarrh, formerly alluded to, the mucous membrane being slightly inflamed, its secretion was arrested, one of the first effects of inflammation, in many instances, on an exhalent surface, and the air, which, in the natural state of the parts, passed along the moistened air passages without in the the least irritating them, now, in passing over the dry mucous membrane, excited cough, or a feeling of something in the larynx, which the patient made vain efforts to hawk up. The soluble demulcents, gum, sugar, or gelatine, especially in the not unpalatable form of lozenges, sucked in the mouth, and thus allowed to diffuse themselves over the throat, supplied the place of the fluid which was wanting. The only objections I know to their use in such a case, are, their inefficiency in cases which are severe enough to require something like direct remedial measures; and the circumstance, that people are often apt to take too much of these sweet articles, and, especially in the case of children, thus to disorder their stomachs. Nothing answers better as a demulcent for a cold in the throat, than a piece of clean picked gum arabic. It melts slowly in the mouth. It perhaps adheres a little better to the mucous surface than the more soluble saccharine matters. It is not so apt to be taken in excess; and if so, is not so liable to cause acidity of stomach. Most lozenges are mixtures of some kind of gum and sugar, with sometimes starch or clarified gelatine.

Demulcents are also used to sheathe and protect the mucous membrane of the alimentary canal. Thus, in cases of irritant poisoning, as with arsenic or metallic salts, the patient may be allowed to swallow milk, or raw white of eggs, or farinaceous decoctions, or almond emulsion; and in the case of poisoning with the alkalies, oil is the proper demulcent. In making the selection in these cases, we may keep in view a two-fold indication,—first, to get a mere demulcent, a bland matter which may mechanically protect the mucous membrane; and, secondly, to produce a certain amount of neutralizing or antidotal effect on the poison. The milk, in the case of arsenic, appears to be useful, by coagulating in the stomach, enveloping the particles of the poison, which is most generally in the state of powder, and thus promoting their ejection by vomiting; and raw white of eggs would act in a similar manner. In the case of more soluble poisons, such as corrosive sublimate, and the salts of copper, these albuminous matters, besides enveloping any solid particles, combine with any of the poison that may be in a state of solution, and form compounds which are insoluble and comparatively inert. In the case of poisoning by the alkalies, oil not only supplements the epithelium of the mouth and throat, which is generally destroyed by the poison; but, by being partially saponified by the alkali, converts it into an unirritating compound. The oily demulcent may be continued with advantage, for relieving the excoriation of the mouth, long after the alkali itself has been got rid of. I can speak from personal experience of the cool grateful feeling, and exemption from pain, procured by the application of oil under these circumstances, from having once had the misfortune to peel the inside of my mouth, by sucking into it a concentrated solution of caustic potash with which I was filling an apparatus. In cases of excoriation of the mouth, whether from irritant substances or from aphthous inflammation, the food should be such as to act the part of a demulcent. Plain animal jelly, or soup made without salt, and smooth starchy matters, such as arrow-root, answer this purpose, and ought to be used nearly or quite cold.

In states of irritation of the lower portions of the alimentary canal, as in acute diarrhœa and dysentery, you will find the use of demulcents indicated in most systematic works as a part of the remedial treatment of these diseases, thin arrow-root, mucilage of gum-arabic, barley water, and similar prepara-

tions, being recommended. It is not probable, however, that in such cases these really act as demulcents. They do not pass down the alimentary canal, and sheathe or protect its mucous membrane. They undergo digestive changes, and are more properly regarded as appropriate articles of food, or rather drink; and are to be allowed for the purpose of quenching thirst, and only in small quantities at a time, because, when taken to a large amount, they are apt to do harm rather than good, by exciting the action of the bowels.

In dysentery, however, where there is much irritation of the rectum, and where, consequently, tenesmus (ineffectual straining at stool) is a prominent symptom, these bland matters may be applied topically as true demulcents, for the purpose of sheathing and protecting the imflamed mucous membrane. Hence, small enemata of starch, at a lukewarm temperature, often give great relief, and are especially valuable as vehicles for introducing opiates and other active remedies *per anum*. The clyster, however, ought always to be in small bulk, never exceeding two or three ounces, such quantities being quite enough for smearing over the mucous membrane, whilst a larger bulk is certain to excite contraction in the irritable gut, and so lead to its own immediate expulsion. In many cases, irritation of the rectum may exist unconnected with dysenteric inflammation, as when the bowel is pressed upon by uterine or other pelvic tumours. The portion thus contracted in its calibre becomes irritated, or perhaps abraded, by the passage of the fæces over it, and then a little olive oil, injected once or twice daily, not merely acts as a demulcent, but, by lubricating the part, enables the fæces, especially in costive habits, to pass more easily.

The employment of mucilaginous drinks in acute pulmonary affections, involving the mucous membrane of the air cells, as bronchitis and pneumonia, and also in inflammation or irritation of the urinary passages, cannot be said to be on the principle of their acting towards these mucous surfaces as demulcents. The gummy matters can hardly be supposed to pass in substance to these mucous membranes, and protect them from external irritation. They, or rather perhaps the diluent fluid in which they are taken, act in the one case as expectorants, and in the other as diuretics, and will fall to be noticed hereafter.

Demulcents, besides being used in affections of the mucous membranes, are also employed to protect the skin, and here act by supplying the place of the epidermis, when it has been abraded or removed. The application of a bland ointment to intertrigo, or fretting of the skin from the rubbing of one part against another, is an example of this. The skin of young children at the axillæ, and between the thighs and pudenda, is exceedingly liable to this kind of fretting; and an infant may be saved from much pain and annoyance by smearing the part with a little lard, or dusting it over with some bland powder, such as starch. I need hardly say, however, that these applications will do little good, if due attention is not paid to cleanliness, and preventing the urine and other matters from irritating the parts. With a dirty nurse, who contents herself with merely soothing the child's cries, by covering up the irritated parts of the skin, we will not get rid of this trifling though annoying ailment; all that will be done, is to relieve one bit of the skin, and let the complaint attack another.

The practice of smearing lard over portions of skin affected by erysipelas, and the dressing of burns with carron-oil, or the much better plan of enveloping the part in cotton wadding, are also examples of demulcents applied to the skin. In all these practices much relief to the stinging or burning pain is obtained by the application, and it is chiefly owing to the exclusion of the air, the contact of which appears to keep up the painful sensation. I do not mean to say that this is the only benefit obtained in burns from the wadding. It is applied with gentle pressure, and this acts beneficially in preventing or moderating vesication; but the immediate relief from pain which is experienced, is, I believe,

due to the exclusion of the air. In erysipelas of the face, dusting the whole visage over with flour is an established practice; but I have found it in a majority of cases more agreeable to the patient to keep the part smeared over with simple liniment or lard. The stinging heat is generally much relieved by either plan; but I prefer the oily application, though less cleanly than the other, because it can be applied more uniformly over the whole affected surface, and thus more effectually protect the part.

When a portion of the cuticle has been thoroughly removed from the surface, as in abrasions by mechanical violence, the application of such substances as goldbeater's skin and black plaster, "to keep out the air," is not in general a good practice; and the same is true of the application of cotton to burns, where the blisters have been broken and the cuticle removed, which accident often happens in taking off the burned person's clothes with injudicious haste. When an abrasion of the skin is of small extent, and quite recent, and especially on exposed parts, such as the face, a bit of black plaster is generally the most convenient and seemly application. But you will find that when a portion of denuded skin has been fairly exposed to the air, it almost invariably takes on a suppurative action, and hence any application by which matter can be pent up and kept in contact with it, only tends to convert a very slight injury into a nasty and perhaps troublesome sore. In such circumstances, whether the original injury has been a mechanical abrasion or a burn, and the skin is completely denuded, by far the best application is a bit of lint wet with water and covered with oil-skin,—it matters little in the case of an abrasion whether it be applied cold or warm,—this protects the part sufficiently well to prevent it from being painful, it absorbs the pus, and if removed once or twice in twenty-four hours, so as merely to keep the part clean, but not so often as to disturb the healing process, a new cuticle will soon form, and the abrasion will heal much faster than if it be covered up with patches or plasters.

Part Fourth.

PERISCOPE.

MEDICINE.

CONNECTION OF GOUT AND RHEUMATISM.—TREATMENT BY THE PHOSPHATE OF AMMONIA. BY DR S. EDWARDS.

In a paper, read at the Westminster Medical Society, on the 27th April, Dr S. Edwards made some remarks as to the proximate causes of gout and rheumatism, considering them in the light of blood diseases, arising from a morbific matter circulating in the blood, originally formed in the primary and secondary assimilating processes, thereby occasioning a disturbance of longer or shorter duration, in the nutrition of parts to which it is attracted. He considered there was sufficient analogy between the two affections to cause them to be viewed as mere varieties of the same disease. Reference was made to the opinions of Barthez and Chomel, who considered it almost futile to attempt a diagnosis beyond marking the parts affected in each; and this view was confirmed, by reference to Dr Todd, who asserts, "Gout shows, at first, a decided predilection for the small joints—those of the hand and foot, but in time *all* the articulations are obnoxious to it, and not only they, but also tendons, ligaments, bursæ, and synovial sheaths." Dr Edwards referred to the case of an old lady

who had been troubled with gout for some years. The metatarso-phalangeal articulations of the great toes frequently threw out chalk stones, and a fluid containing urate of soda; when this subsides, she is troubled with chronic rheumatism of the whole body; but on the gout and secretion returning, the rheumatic pains entirely leave her, showing such a connection between the urate of soda and the rheumatism as to warrant the belief of their being cause and effect. Dr Edwards then went on to state, that from a number of chemical and microscopical experiments, he had arrived at the conclusion that these diseases had an excess of lithic acid for their essential cause; that the elements of this acid and its combinations are supplied by the nitrogenized elements of the food, as well as by the changing tissues of the body, a sufficient reason why the disease should attack the young and badly nourished, as well as the older, more plethoric, and those who live highly. Dr Edwards confirmed the fact, that in gout uric acid existed in the blood, and in several cases he had obtained rhomboids of uric acid from the same fluid in rheumatism in much larger proportions than in health, but not to the same extent as in gout. This was accounted for from the circumstance of the lithic acid being partially thrown off by the skin and kidneys in the one disease, whilst it was retained in the system in the other. Dr Edwards had experimented in fifteen cases, and made reference to Simon, and also to Becquerel's cases in proof. The urate of soda had been considered to be a secretion peculiar to gout; Dr Edwards referred to Dr Macleod's statement of the articular cartilages in capsular rheumatism, being covered with it occasionally; also to a case of rheumatic gout, given by Dr Golding Bird, where an eczematous eruption of the legs was frosted over with microscopic crystals of it; also, to a case which he had himself observed, where a patient had died of heart affection, and where deposits of urate of soda existed in the valves of the heart. This patient had had rheumatic fever, but not gout. Reference was made to the connection between rheumatism and granular disease of the kidneys, and some other renal affections, in which the relative proportions of the ingredients in the urine are diminished, and, according to Rayer, are found to exist in the blood and serous effusions; and in which, on the examination of the urine, Prout and Christison state, that, according to their observations, the lithates are most strikingly deficient. The author considered himself justified in asserting, that in these two diseases there was a large and undue proportion of lithic acid or its compounds in the system, and asked, may not this deposition, then, occasion such an extent of acrimony in the fluids of the body as to irritate, and excite to a morbid action, the lymphatics and minute terminations of the arteries in the several parts of the body, and not improbably of the lining membrane of the larger arteries, becoming in fact, a source of irritation wherever deposited, more especially in parts, such as the joints and sheaths of the ligaments and tendons, which, being inextensible, would sooner probably become affected by such distempered excitability. Dr Edwards then spoke of the physiology of the beneficial action of the phosphate of ammonia in these affections. On being taken into the system, and coming in contact with the uric acid or urate of soda, it became decomposed, a phosphate of soda and urate of ammonia would be produced—thus exchanging a very insoluble for very soluble salts; but this was not all, for Baron Liebig had shown that the phosphate of soda has a remarkable effect upon uric acid, rendering it soluble with facility in water. By these means, therefore, the free and combined uric acid existing in the system in these diseases will be dissolved, and rendered capable of easy elimination by the kidneys. Dr Edwards then remarked that he had used the phosphate of ammonia in almost every variety of gout and rheumatism, and almost always with the most beneficial effects. He had frequently warded off attacks of gout by its early employment. Before using it, he generally prefaced it by a dose of calomel, colocynth, or some other purgative; and in the acute articular rheumatism adopted the usual local and general antiphlogistic treatment. Under its use the tendency to attack fresh joints had diminished, and the chronic form, so frequently left

behind from the acute, was generally obviated, and when existing, usually yielded in a few days. It had been found extremely beneficial in some of those cases of a local character which bear a resemblance to neuralgic disease. Dr Edwards also drew attention to an important fact in connection with the use of this salt in *acute rheumatism*. He had employed it in fifteen cases, and in no one had *heart* symptoms accompanied it. When it is remembered that heart complication occurs in about one-fifth of the cases of rheumatic fever, the subject deserves attention. Dr Edwards had found it of great use in subduing the swelling which so frequently occurs subsequently to gout, and spoke highly of its powers in preventing the formation of chalk stones, as well as arresting their increase when forming. He also described its solvent power as great in uric acid gravel, and asked, might it not be available in uric acid calculi? He had given it a comparative trial with phosphate of soda and benzoic acid, and found it far more useful in its effects. In some few cases in gout he had used a lotion of it, with good and soothing effects, especially where a concretion of urate of soda appeared to be forming.

RECENT DISCUSSION ON ACUTE ARTICULAR RHEUMATISM, IN THE ACADÉMIE DE MÉDECINE OF PARIS.

Notwithstanding all that has been written on the subject of rheumatism, it must be confessed that neither its true pathology nor appropriate treatment has been hitherto discovered; or, if discovered, they at least have not been as yet generally accepted. It is obviously of the utmost importance that the true principles of treatment, in a disease so common and so destructive, should become established; and in the present state of our knowledge there seems no prospect of effecting this, except by the application of the numerical method to large numbers of clinical observations carefully recorded. These observations must include all the modes of treatment which have been vaunted as successful, whether they are purely empirical or founded upon *rational* indications. One other mode of solving the difficulty certainly suggests itself, viz. to unravel the complicated pathological questions regarding the nature and causes of rheumatism, and then to apply to therapeutics for the appropriate remedies. But although this would be undoubtedly the more satisfactory mode of procedure, its accomplishment must at present be regarded as impossible, involving as it does the perfection of medical science, and the establishment of an unimpeachable code of therapeutics—the golden age of physic. While, therefore, we are of opinion that no effort should be spared to investigate the pathology of rheumatism, and firmly believe that the knowledge thus acquired will be found invaluable in directing to a rational mode of treatment; we are far from undervaluing the information drawn from the experience of physicians who have treated large numbers of cases upon a fixed plan, even when their practice rests upon what we ourselves consider an incorrect pathology.

A recent discussion on rheumatism, in the Parisian Académie de Médecine, gives us the opportunity of laying before our readers the opinions of some of the most distinguished practitioners of France. The subject was introduced by M. Martin-Solon, who read a report upon a memoir of M. Dechilly, physician to the hospital of Vaucouleurs (Meuse), on the treatment of acute rheumatism by the early and repeated application of large flying-blisters to the whole of the joints affected. M. Dechilly's practice is, if the patient be plethoric, to commence with a general bloodletting; if the brain, pericardium, or pleura be implicated, he applies leeches locally; as for the inflamed joints, blister after blister is had recourse to, until the disease is expelled from the system. We find no mention made of the use of internal remedies, except small doses of nitre.

Of the efficacy of this system M. Martin-Solon reports favourably, but in most of his own clinical experiments, he seems to have combined the use of large doses of nitrate of potass, or of sulphate of quina, with the vesicant treatment. He finds that, when the disease is not very intense, or confined to a small number of articulations, it may often be subdued in a few days by the

simple application of blisters. Thus some of M. Dechilly's cases terminated favourably on the fourth, fifth, or seventh day. But when the affection is more acute and diffused, its duration is generally much protracted, for the rheumatic inflammation passes in succession from joint to joint, notwithstanding the use of the blisters. In one of M. Dechilly's cases, convalescence was not established till after eighteen days of treatment, and the application of thirteen blisters. Neither does this treatment prevent relapses, for one of Dechilly's patients, who was twice largely bled at the arm, twice leeched, and five times blistered, with considerable relief to the articular symptoms, was six days afterwards attacked with rheumatism in the chest (pleuritis?), which yielded to further depletion, but was succeeded by rheumatism in the muscles of the left arm. M. Martin-Solon considered the practice of repeated blistering especially applicable to cases accompanied with constitutional or morbid exhaustion, or with symptoms referable to the digestive organs forbidding the use of contra-stimulant internal remedies. He had himself obtained incontestible advantage from the use of sulphate of quina given to the extent of one and a-half to three grammes in the course of twenty-four hours. But the practice which he generally followed was that recommended by Brocklesbey in 1764, viz., the administration of nitrate of potass in large dose. He prescribed from 24 to 36 grammes (6 to 9 drachms) in three litres (5 lbs.) of lemonade, or of some agreeable infusion with sugar, to be taken in the course of the day. It was necessary that the nitre should be given in a large quantity of fluid in order to avoid its irritating properties. In a few cases, complicated with endocarditis, he had likewise ordered bloodletting and leeches, but, in general, he trusted to the nitre alone.

M. Rochoux regarded rheumatism as an inflammation; nay, as the very type of inflammations.

M. Bouillaud was far from allowing, like M. Martin-Solon, that nitre, quina, antiphlogistics, and blisters, might be had recourse to indifferently. If the disease were confined to the joints, its grave character would be much diminished; but it was well known that it did not rest there; and that too frequently organic diseases of the heart arose when the patient was regarded as cured. Woe to the patient in whose case rheumatic fever lasted more than fifteen days! Even if the articular symptoms have yielded, he would almost certainly be sooner or later the victim of incurable organic disease. M. B. had no confidence in modes of treatment directed against the pains of the joints; it was the visceral, and especially the cardiac, complications which should guide the physician's practice. He then proceeded to describe the success which he believes he has obtained by venesection practised, "coup sur coup." In more than 600 cases, this treatment had never disappointed him, removing the most intense rheumatism within a week, and surely proventing the lesions of the heart. But to insure its efficacy, it was essential that this treatment should commence on the first day of the disease if possible, or at latest before the termination of the first week. He denied the specific nature of the rheumatic process; believing it to be a disease of the system, "*une maladie générale;*" an inflammation, just as pneumonia and pleurisy were inflammations, and no more dependent upon a specific virus than these two diseases were.

M. J. Guerin made some remarks upon the effect of subcutaneous puncture of the articulations in rheumatism. He could not account for the relief which he had observed to follow this practice. It could not act antiphlogistically, for only a drop or two of blood escaped.

M. Gerdy believed that rheumatism differed from ordinary inflammation in the frequent and sudden alterations of its site; in its want of periodicity (for all true inflammations had periods during which they augmented in intensity, or were stationary); and, finally, in the lesions which were observed in fatal cases.

M. Grisolle argued against the inflammatory nature of rheumatism, because the fever was often observed to continue when the articular pains were gone,

and when yet there was no evidence of internal mischief; because rheumatism never terminated in gangrene; because it never terminated in suppuration. The cases in which this latter termination was supposed to have occurred, he regarded as examples of purulent infection, with pains in the joints, and subsequent effusion of pus. Besides, if rheumatism were inflammatory, why, when seated in the joints, did it not follow the same course as local inflammations, which sometimes manifested themselves during an attack of rheumatism? Why should the pleurisies and pericardites, in such circumstances, follow one course, while the pretended arthrites followed another?

He denied the efficacy of the practice of venesection, "coup sur coup." Sydenham, who was once an advocate for profuse bleeding, towards the end of his career condemned the practice as useless. At Montpellier the system once was to bleed thrice a-day; but there, too, it was now abandoned. M. Legroux, who had employed the so-called "nouvelle formule," and had acquired his knowledge of it from his master, M. Bouillaud, had recently publicly declared, that it only served to prolong the convalescence, to favour complications, and to induce chloro-anemia.

M. Piorry, after alluding to some original experiments of his own upon the amount of blood which can be abstracted without inducing death, and after claiming for himself the priority of the application of the "nouvelle formule des saignées suffisantes," proceeded to develop his own views regarding the pathology of rheumatism. Under the term acute articular rheumatism, had been united conditions the most dissimilar, both in their nature and in the mode of treatment which they require. He instanced the fever, accompanied with acute articular pain, which is sometimes observed after delivery, and what is known in this country under the name of gonorrheal rheumatism, as differing essentially from the disease under discussion. In true rheumatism there were two elements under which the symptoms might be ranged. One of these was the state of the blood, whose serum was surcharged with fibrine; this condition he denominated "hémite." The other element was the "arthrite," a most decided inflammation, often affecting several joints together, or in succession, and very rarely terminating in suppuration.

The following was the treatment which he considered applicable to "hémit-arthrite." In the early stage of the disease, while the morbid condition of the blood was still well marked, the physical examination of the heart, arteries, veins, capillary system, liver, lungs, &c., demonstrated the presence of an excessive quantity of blood; and, at this period, it was advantageous to bleed abundantly, once, twice, or even thrice a day. The amount and frequency of the venesection should be directed by the physical signs referable to the viscera, and by the properties of the blood drawn. It was rarely necessary to bleed after the third day. Sometimes the disease was thus subdued within twenty-four hours. In chronic cases, or in those in which the symptoms had already existed for several days, cupping and leeching might be substituted for general bloodletting. In cases of relapse, the treatment of the acute stage was to be had recourse to. In all cases, in order to combat the "hémite," and prevent endocarditis, large quantities of diluent drinks should be given, emollient applications should be made to the joints, and great advantage would be derived by keeping them *in an elevated position*. He had never seen good effects from the use of quina, except when the symptoms recurred with periodicity, when the spleen was implicated, or when, after the acute symptoms had abated, neuralgic pains, connected with the articular lesions, were observed. Tartarised antimony might be useful from its property of producing serous discharges, but its good effects were more equivocal than those of bloodletting, and its use was attended with more danger. Opium, and other narcotics, he condemned as remedies empirically directed against the mere symptom of pain, whereas this symptom was the result of the pathological conditions which ought to be the subject of treatment. He had never obtained any species of success from the employment of narcotics, of nitre, or of any of the vaunted specifics

against rheumatism. Blisters were most useful when the disease continued to cling to certain joints after the diseased condition of the blood (*hémite*) had disappeared. "In short," said M. Piorry, in concluding, "acute articular rheumatism is not a simple disease always demanding the same treatment. It is a compound of different *organopathic* elements, which are far from identical in degree in every case, and which each present particular therapeutical indications. According to my doctrine, rheumatism is not to be combated by a formula indicated *a priori*, but by directing against the different lesions which present themselves, means of cure adapted to and regulated by the state of the organs of individuals who are the subjects of treatment."

M. Parchappe argued, that acute articular rheumatism is a disease of the whole system (une maladie générale). He maintained that all such diseases have determinate periods of development and duration, which it was not in the power of the physician to abridge. He consequently deprecated as futile any attempt to *jugulate* rheumatism, and preferred a modified expectant treatment, including antiphlogistic remedies adapted to each indication which might present itself,—remedies to relieve symptoms, to combat complications, to prevent the rheumatism from passing into the chronic state, and *to lessen the duration of the disease.*

[How to lessen the duration of a disease which has a *determinate* duration we are not informed].

Those who regarded rheumatism as a local inflammation, appealed, in support of their opinion, to the nature of the local symptoms; to the concomitant fever—an inflammatory synocha; to the state of the blood, which always shows the inflammatory buffy coat; to the inflammations with which the disease was often complicated; and, finally, to the effect of antiphlogistic treatment.

He repudiated the axiom,—*naturam morborum ostendit curatio.* To support a theory by the alleged good effects of a mode of treatment suggested by the theory was to reason in a circle. The inflammatory nature of the articular lesions, and of the complications, did not necessarily prove that the disease was an inflammation. There were diseases non-inflammatory in their nature, which included among their local manifestations inflammations proceeding even to suppuration. He instanced variola. As to the state of the blood, he admitted that the great preponderance of fibrine might be regarded as an indication of the inflammatory diathesis. He pointed out a fallacy in the ordinary mode of calculating the amount of fibrine in analysis of the blood. The fibrine is always directly estimated, being obtained by agitating or stirring the freshly drawn blood. But as a certain quantity of plasma containing fibrine, is included within the envelopes of the blood corpuscles, it is plain that a corresponding amount of fibrine must elude the detection of the analyst. The proportion of fibrine *obtained* must consequently be modified by the variable amount of the blood corpuscles, being largest when the corpuscles are present in smallest number.

M. Bouchardat said that, in rheumatism there were morbid elements which might exist simultaneously or separately. The inflammatory element was often present, as was shown by the buffy coat of the blood. This product, according to M. B., consisted of three principles—1st, a matter identical with pure albumen, and capable of dissolving in water faintly acidulated, which he termed *albuminose;* 2d, a substance insoluble in acidulated water, presenting a great analogy with the characteristic element of epidermis and epidermic productions; 3d, a matter which gelatinises when boiled in water;—it was this matter which characterised the inflammatory buffy coat, and which was *not* present in fibrine. He detailed a series of experiments, which he thought proved, beyond all question, the existence of this gelatinous principle in the blood of acute articular rheumatism. But besides the inflammatory element, there was a rheumatismal element which he could not better define than as *a particular state of certain organs.*

Adopting the result of observation, that acute rheumatism, like pneumonia, &c., could be produced by exposure to cold, he proceeded to inquire what could be the effect produced upon the system by such exposure? The secretions, including those of the skin, might be immediately suppressed or altered. The characteristic elements of the cutaneous secretions might either be abnormally developed in other tissues or membranes, or might simply cease to be produced. All the apparatus continually in operation in the living body, furnishing secretions, either acid or alkaline, might be regarded as an assemblage of batteries, whose poles might be reversed by the influence of a perturbing cause, or whose regular action might cease for a time. Thus membranes generally lubricated with alkaline fluids, might be brought in contact with fluids faintly acid, whose degree of acidity did not exceed that of the perspiration or of muscular tissue; and this modification of the secretions, trifling as it might seem, was yet sufficient, under the influence of the temperature of the body, to effect important chemical changes on the tissues containing gelatine. He had, in fact, shown that membranes might even be dissolved by immersing them in water at the temperature of + 38 Cent. (100 Fahr.), containing less acid than the perspiration. According to individual constitutions, such perversion of secretion might be often produced in the same subject, either in the membranes of the articulations, or in the respiratory apparatus, under the influence of cold; and the result would be, according to the parts affected, acute rheumatism or pleuro-pneumonia.

In reviewing the modes of treatment, M. B. admitted the propriety of blood-letting, but expressed some doubts as to the assumed harmless nature of copious venesections practised at short intervals, and as to their utility when opposed to the grave complications met with in certain cases.

No treatment was better than that by sulphate of quina—yet in small dose it was useless, and in excessive dose it was a poison. The quantity given should be such as to produce some temporary disturbance in the system, but beyond this limit it should not be pushed. One to two grammes (15 grs. to ʒss.) in twenty-four hours, and in divided dose, was sufficient for an adult. This treatment abridged the duration, and diminished the intensity of the disease: it was not yet shown to be efficacious in preventing complications. Nitrate of potass was a valuable remedy, and might even be given to the extent of from 40 to 60 grammes (617 to 926 grs.) in twenty-four hours; but it must be given in a large quantity of water. The dose must be administered regularly at stated intervals throughout the whole twenty-four hours, and must not be persisted with unless the kidneys act well; for, according to the experiments of Orfila and M. B., the presence of 20 or 30 grammes (309 to 463 grs.) in the blood may occasion death.

Digitalis, squill, and colchicum, he did not consider sufficiently trustworthy or manageable. Of tartarised antimony in contra-stimulant doses, he had no better opinion. He allowed that opiates were, in certain cases, of service, not only in relieving pain, but even in favourably influencing the progress of the disease. Blisters were theoretically indicated at the very commencement of the disease, and he recommended that their dimensions and activity should be proportionate to the violence and extent of the disease under treatment.

A discussion followed between M. Bouillaud and M. Grisolle, regarding the possibility of cutting short the disease by repeated blood-letting. From this discussion, and from the comments of the different journals, particularly of the "Gazette des Hôpitaux," it would appear that M. Bouillaud stands almost alone as the advocate of his "nouvelle formule," and that he labours under some illusion as to the duration of the disease in the cases to which he continues to apply it. M. Michel Lévy next proceeded to analyse the evidence in favour of M. Dechilly's proposal, which he proved to be defective, and moved that the Académie should request the author to *pursue and complete* his researches on the treatment of acute articular rheumatism by the exclusive use of blisters.

This motion was adopted by the Académie.

[In the course of the above discussion, we find no mention made of certain modes of treatment which, in this country, have numerous advocates. We allude to the employment of sudorifics, of calomel and opium, and of aconite. We desire to express no opinion upon the several merits of these modes of practice; but surely they were at least worthy of notice in a discussion in which so many contradictory views were expressed upon the appropriate treatment of this very common and too destructive disease. Can it be that the accomplished physicians, who took part in the debate, are not well informed as to the practice pursued in their own day in Britain?]

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

MENSTRUATION IN RELATION TO PREGNANCY. PROFESSOR DUBOIS.

Conception may take place in a woman not yet arrived at the age at which she ought to menstruate. There are, in fact, many women who do not menstruate till the seventeenth, eighteenth, or nineteenth year. Such women may become pregnant at that time of life, although they have never menstruated. M. Dubois has seen a woman become pregnant two years after the cessation of the menses. The woman, finding her abdomen enlarging, entered the medical department of an hospital. The physician under whose charge she was, had so little idea of the woman's being pregnant, that he delivered a clinical lecture on the case, as one of ovarian dropsy. On making an examination of the case, M. Dubois easily made out the pulsation of the fœtal heart. In fact, labour very soon supervened.

The menses may be suppressed physiologically, and yet pregnancy take place. Thus, it is not rare to see nurses become pregnant before menstruation has re-appeared. We find also, that women who are extremely irregular, who, for example, menstruate only once or twice a year, become pregnant; although, in general, this state is one very unfavourable for conception.

Various diseases and changes in habits may derange the menstruation, and give rise to the idea of pregnancy. This error occurs frequently, chiefly to persons anxious to become in the family way. Nothing is more common than to find the menses suppressed for sometime after marriage. It is also very frequently observed, that women leaving the country to reside in the town, suffer from suppression. This may be said to occur habitually in young women coming from the country into domestic service in Paris.

In other cases, the menses, after having been suppressed for three or four months, re-appear suddenly, with some profuseness. This is sometimes taken for the occurrence of abortion, when it is merely the recurrence of the menses after they have been suppressed, in consequence of some change in the habits of the female.—*Journal de Médecine et de Chirurgie*, p. 207, May 1850.

NEW FORM OF DYSTOCIA, CONSISTING OF THE POSITION OF THE FOREARM OF THE INFANT BEHIND THE NECK. BY DR SIMPSON.

The "Gazette des Hôpitaux" gives, under the above title, an account of the case of obstructed cranial presentation, described and figured by Dr Simpson, in the April Number of this Journal, p. 389. The translator states, that he is not aware that any instance of dystocia, from this malpresentation had been before recorded; but he adds, that Professor Paul Dubois informs him, that he and his father had each met with a case of the same kind. We have been informed, that Dr Spiess, of Frankfort, had in one instance seen labour obstructed by the same malposition of the arm in a head presentation. —*Gazette des Hôpitaux*, May 25, 1850.

NOVEL MODE OF ARRESTING HEMORRHAGE IN A CASE OF THROMBUS OF THE VULVA. BY M. FABRE.

M. Fabre was called in haste to a woman in labour, who was taken with violent hemorrhage. A thrombus was discovered in the labium of the right side, in size equal to the head of a child at the full time, and having at its centre a crevice, whence the blood flowed as water from a sponge. The woman, who had been in attendance, attempting to make a vaginal examination, and finding, instead of a free passage, an enormous swelling, had mistaken it for the bag of membranes, and unfortunately succeeded in rupturing it.

Having no ligatures at hand to apply, nor the necessary means for producing regular compression, M. Fabre resorted to the following ingenious expedient, in order to arrest the bleeding. He procured a green willow-wand, of about three centimetres in thickness, so elastic, that when split up from one of its ends, the two halves came again into close apposition, and so strong, that in separating the two parts at one end it did not split up to the other. The wand was twenty-two centimetres long, and split along three-fourths of its length by a knife. Both halves of the wand were well covered with lint to protect the soft parts against the hard wood.

An assistant pinched up the edges of the laceration. These were then engaged between the two parts of the wand, which seized them strongly in consequence of its elasticity. The hemorrhage was in this manner completely arrested.

The woman was delivered three days subsequently. The apparatus required to be removed to permit the passage of the head. The hemorrhage did not return, and the woman did well.—*Revue Médico-Chirurgicale*, March 1850.

[In such cases the hemorrhage is venous, and, like other venous hemorrhages, is generally easily arrested by compression in any form. And it is usually perfectly stopped by compression of a few hours' duration only.]

ON THE USE AND ADVANTAGES OF OPIUM IN THE PRACTICE OF MIDWIFERY. BY J. C. W. LEVER, M.D.

Abortion sometimes depends upon the mother, sometimes upon the condition of the ovum itself. The maternal causes of abortion are, for the most part, physical or mental; we must add such as are accidental, those which take place from habit, and those which occur from the effects of poison, as syphilis. But abortion most generally occurs from fœtal disease or imperfection; so that the premature emptying the uterus is but an effort of nature to get rid of that which she cannot perfect. In the management of cases of threatened abortion, the best rule is, if possible, to get a thorough knowledge of the immediate or exciting cause of the hemorrhage or pain, or both; secondly, before using opium, to ascertain the state of the os uteri, and especially whether the anterior part of the neck has lost its plumpness and firmness, and has become soft and baggy. If with the discharge we have a patent state of the os uteri, and if the neck be soft and loose, the exhibition of opium will do harm, by retarding the emptying of the uterus, which must sooner or later take place. But while Dr L. does not advocate the use of this drug under the circumstances related, he speaks loudly in its praise after the abortion has occurred, especially if such have been attended with a large loss of blood: it will allay excitement, tranquillise the circulation, and procure sleep. These remarks, however, do not altogether apply to those cases which menace from accident, or from mental causes, or those which may be said to be due to habit. In these, the application of cold, perfect quietude, and unstimulating diet, along with the exhibition of opium by the mouth, or a cold starch injection with opium thrown into the bowel, and repeated every night, or more often according to existing circumstances, are followed by the best results.

But the value of opium is still more clearly exhibited when it is administered to alleviate those pains which precede the establishment of labour in the latter

weeks or months of gestation. Many a patient, by its agency, has been carried on to the full end of term, who but for it would have prematurely parted with her offspring. A lady, six weeks from the completion of her full term, fell on her back. The liquor amnii was evacuated. In addition to absolute quietude, she took opium at irregular intervals till the end of the ninth month, when a living child was born; and this was the more satisfactory, as she had on two previous occasions been prematurely confined, although opium had been administered, but with a sparing hand.

Further, we find this drug of great value in certain varieties of natural labour. For instance, at the commencement, there may be irregular and spasmodic pains. They are recognised by their acuteness, by the want of consentaneous action in the uterine fibres; some portion of the uterus during their continuance is hard and contracted, the other portion is soft and yielding; there is no distinct or regular interval of time between the occurrence of pain; and, if untreated or unrelieved, the strength of the patient is exhausted before the establishment of true labour pains; or the child, which at the commencement presented normally with the head, has its position changed to that of the shoulder, by reason of the uterus contracting on one side and forcing its contents over to the uncontracting or yielding side. In such a case, the utility and value of opium are most marked. It may be exhibited per mouth or per anum. It will calm the spasm, subdue irregular action, alleviate pain, procure sleep; and, after this, true and regular uterine action will be established.

Not unfrequently in women who marry late in life, and in those who marry very young, do we find the liquor amnii pass away very early—in fact, before the os uteri has commenced to dilate; this may occur spontaneously, or be the result of violence. At all times this is to be regretted; for, in addition to our losing the efficiency of the bag of water to prepare the way for the passage of the child, the fœtus is brought into close contact with the uterus, which is therefore more strongly stimulated; the head is brought into direct contact with the os internum, the most sensitive part of the uterus; the labour is more painful, and the birth of a living child is rendered more doubtful. Here the cautious and judicious exhibition of opium controls hyper-uterine action, alleviates pain, and gives a better security for the welfare of the child.

Again, in practice we find women who have suffered in early or unmarried life from one of the forms of dysmenorrhœa, when pregnant and in labour, with the os uteri thin, sharp, knife-like, so that its edge is scarcely to be felt—in fact, is often overlooked by the unpractised finger. The sufferings of the patient are intense; the dilating stage of labour is protracted; and, if untreated or unrelieved, by the time the os uteri is dilated nature is exhausted, uterine effort fails, and such a case is frequently terminated either by the forceps or by craniotomy. In most cases, these evils may be averted by the timely employment of opium, and the best mode of securing its good offices is by administering it in the form of enema.

Further, we occasionally find the first stage of labour rendered tedious by a hardened, undilatable condition of the os uteri, in women who have suffered from chronic inflammation of the neck of the uterus, or those who have worn mechanical contrivances for the purpose of supporting the viscus, and in those who, from disease, imaginary or real, have been submitted to the influence of some escharotic, at the present day by far too commonly applied. This condition of the os uteri needs no description; the sufferings of the patient are excessive and protracted, and, if unrelieved, may be followed by results serious to mother, and fatal to child. In addition to blood-letting, applicable to some cases, to the warm bath, of immense value, to the exhibition of antimony, and this is of the greatest service, we find, when the latter has been exhibited, and has produced its desired results, relaxation of the os uteri, and increase of discharge, that opium given in a full dose will render such permanent, and thus prove a most valuable agent in completing a safe delivery. Opium has been recommended most strongly in cases where the os uteri is callous; but, i

the callosity depends upon previous injury, or is the result of disease, its value, probably, depends upon its power to curb uterine action until vaginal interference removes the obstruction to the passage of the fœtus. But there is another condition of os uteri in which opium acts, and like a charm :—in women who have suffered from irritable uterus where the vagina is generally dry and hot, although not over-sensitive, but the moment the examining finger touches the os uteri, the patient shrieks out, shrinks from the attendant, and by her cries and motions proves the suffering she endures. In addition to subsidiary measures, as the warm bath, the injection of linseed-tea into the vagina, great benefit is to be derived from the use of opium, given either by the mouth or by the rectum. Further, in cases of transverse presentation, where it is necessary manually to interfere to bring the long axis of the child to correspond with the long axis of the uterus, we may assist in relaxing the os uteri, and abate uterine contraction, by the exhibition of a full opiate ; but I am no advocate for repeated doses. By such treatment the patient becomes narcotized, uterine efforts arrested, and at the time we need contraction to complete the delivery and prevent hemorrhage, nature fails, and our patient is placed in a situation of extreme peril.

Again, in convulsions, especially those of the hysterical form, occurring as they do more frequently during pregnancy than during labour, opium is a valuable remedy. This form of convulsions, evidencing itself as it does most frequently during gestation, is readily recognized by the predisposition of the patient, often induced by over-fatigue, mental anxiety, irregularity in diet, &c., preceded by intolerance of noise, sleep short and interrupted, twitchings, startings, copious flow of limpid urine, oppression at the chest, difficulty of breathing, globus, pain at the upper part or back of the head; and when the convulsions manifest themselves, the larger muscles are more often affected than the smaller; here we find, after the paroxysm is over, that a mild opiate soothes the patient, allays the twitching, calms the respiration, and procures sound and refreshing sleep.

Secondly, in the anemic form of convulsions, associated as they not unfrequently are with large losses of blood, where the face is pale, the eyes glazy, the features shrunken, the countenance betokening exhaustion, the lips colourless, the skin cool, the chest heaving, the breathing laboured, the pulse small, quick, and irritable, with noise in the ears, and pain or weight at the top of the head, where there is sleeplessness or restlessness, partial amaurosis, strabismus, and sometimes delirium ; while close attention is paid to the position of the patient, especially to the position of the head ; while stimulants are administered with judgment ; while the contraction of the uterus is secured, opium will be found to act like a charm. Again, in genuine eclampsia, where vascular excitement and relaxation of the soft parts have been accomplished by bleeding, purgation, and tartarized antimony, and where the repetition of the fits seems to depend upon irritation, Dr L. has seen them occasionally checked by the administration of a full opiate. Labour may also be complicated with tumour ; here opium will allay inordinate action until we employ those manual or surgical means which are necessary to remove the obstructing cause to delivery. It is true, opium cannot take away the mechanical obstacle, but it may and will lessen inordinate uterine action; for in practice we find that, if there be any difficulty in the passage of the child, the uterus is stimulated to undue action, and if such be not allayed, or be overlooked, rupture of the viscus itself may take place. We may observe also the efficacy of opium in the treatment of those grave cases where the uterus or vagina is lacerated, or the bladder or diaphragm have been ruptured. The two latter lesions are, indeed, to be regarded as all but hopeless; but not the former, for Dr L. has seen cases treated by the administration of full doses of opium, and these repeated at varying intervals for several days, and then terminating successfully. To one woman Dr L. was called when there was a band in the vagina, the result of a previous lelivery; in this case, the laceration was so extensive that the hand could be

passed into the abdomen. Although the patient appeared to be dying, and the last rites of her church were administered to her, she rallied, recovered, and is still alive. Soon after her convalescence, she had the ill fortune to lose her husband, but she also had the good fortune not again to be pregnant. But the value of opium is perhaps most emphatically demonstrated when exhibited after floodings, whether such occur in the earlier months of gestation or in the latter, depending either upon the position of the placenta or its partial separation; whether the loss take place after the birth of the child and the casting or throwing off the placenta; whether this be retained by irregular contraction or morbid adhesion; or whether the hemorrhage take place after the complete evacuation of the uterus. In these cases, where there is great exhaustion, alarming syncope, great irritability, severe vomiting, and plain, evident, and undeniable indications of great depression of the sanguiferous and nervous systems, or, to use the graphic language of Dr W. Griffin, " When the countenance is sunk, the eye hollow and glassy, the lips blanched, the skin cold, and the whole person corpse-like; when the pulse is all but gone at the wrist; when the beat of the heart is scarcely perceptible, and stimulants, even brandy, are vomited or useless, opium will act like magic, and save the patient from an untimely grave; but, to do good, it must be exhibited in full doses of one to two drachms of the tincture, or three to four grains, repeating two grains every half hour or hour until the pulse becomes distinct, the breathing calm, and the jactitation allayed." Whatever may be the "ratio medendi," whether the congestion produced in the brain be what is necessary to maintain the proper tension of the cerebral vessels, whether it restore the loss of nervous power in the brain itself, is still a point "sub judice;" but no man of much obstetric experience will deny its value under the circumstances thus detailed. And in the subsequent constitutional treatment its exhibition must not be forgotten: it will lessen exhaustion, diminish restlessness, allay vomiting, calm gloomy forebodings, and procure sleep.—*Lond. Med. Gaz.*, Nov. 1849.

NEW VAGINAL PLUG FOR ARRESTING UTERINE HEMORRHAGE. BY DR GARIEL.

Dr Gariel, of Paris, has constructed a new form of plug. It consists of a slim bag of vulcanized caoutchouc, of the size of a small orange, but capable, by insufflation, of being expanded easily to three or four times that volume. In order to allow of its insufflation, a slender tube, of the same substance, twelve or eighteen inches long, but with thicker parietes, is attached to the bag. The bag and tube weigh only one or two drachms; the small soft empty bag is easily introduced into the vagina; and when properly insufflated it accurately adapts itself to the vagina, forming a very perfect plug, and, as we believe, one that may be found very effective and useful. A ligature or knot, thrown round the tube, retains the bag in a distended state. This air is allowed to escape before the withdrawal of the plug, thus greatly facilitating its extraction,—a part of the procedure with the common sponge and other plug, that is generally accompanied both with some pain and difficulty. It has been successfully used in France in arresting hemorrhages, both connected with labour and with diseases of the uterus. A similar application of caoutchouc, as a simple and perfect vaginal plug, was, we know, made some time ago by Dr Hamilton, of Falkirk. —*Journal de Médecine*, April 1850.

SUPPURATION OF THE THYMUS GLAND, A PATHOGNOMONIC LESION IN INFANTS DYING OF SYPHILIS. BY PROFESSOR DUBOIS.

In his researches upon the pathological anatomy of the thymus glands, Haugsted, about twenty years ago, published two cases of young men who had died when labouring under syphilis, and in both of whom there was a deposit of pus in the thymus. In both subjects, however, the lungs also were destroyed by tubercles. Several years ago, in examining the body of a child who had died of congenital syphilis, Professor Paul Dubois found the thymus gland to

contain a great number of small purulent deposits. Since that time he has made a variety of similar observations, and, in a late communication on the subject, in the "Gazette Médicale," he concludes that,—1. the presence of pus, disseminated or collected in deposits in the thymus of new-born infants who had died of syphilis, ought to be considered, not as a single coincidence, but as an effect and an evidence of the special disease under which they had perished; and, 2. this alteration should authorise us, in the absence of all other matters explanatory of the death of the fœtus, in prescribing an anti-venereal treatment, in order to prevent the similar death of subsequent children. M. Dubois believes the pus in these cases to result from inflammation of those reservoirs and ducts, which Sir Astley Cooper discovered in the structure of the thymus gland. One source of fallacy will require to be avoided—viz., the mistaking of the white viscous contents of these reservoirs for pus.—*Gazette Médicale*, May 25, 1850.

DEDUCTIONS REGARDING RETENTION OF URINE IN THE FŒTUS, AS A CAUSE OF DYSTOCIA, &c. BY M. DEPAUL.

1. The urinary secretion takes place at an early period of fœtal life.—2. When a congenital defect, or any other cause, prevents the excretion of urine, which at this period can be poured out only in the cavity of the amnios, it accumulates in the bladder, which latter may then become greatly distended, and acquire such dimensions that easy parturition is impossible, even where the pelvis is well formed, and though pregnancy may have run through all its stages.—3. The difficulties which such a state of things may give rise to are such, that the head has been torn off, without the obstacle being overcome.—4. Whenever the parts have been carefully examined, it has been shown that the development of the urinary receptacle coincided with hypertrophy of its parietes, especially of the muscular coat,—a circumstance which, added to many others, will give me an opportunity to prove, in another paper, that the bladder acts not only as a passive reservoir, but that that organ makes often renewed efforts, through the whole course of gestation, in order to expel the liquid which it receives.—5. If the consideration of the preceding facts shows that it is almost impossible to recognise, during gestation, such a defect of conformation, I have no doubt that during parturition a very good guess may be made regarding this defect.—6. Though unusual phenomena observed during gestation (which may, however, be caused by various pathological changes) ought to be taken into account, it is nevertheless incontestible that the hand introduced into the uterus can alone give the clue to the real state of things, or will at least allow the certainty to be made out, that a collection of liquid exists in the child's abdominal cavity.—7. The great rarity of simple ascites, carried to this extreme degree, will be a presumption of the bladder being distended; and if the exploration of the genital organs allowed those congenital defects to be traced which I mentioned before, I think that a retention of urine might unhesitatingly be diagnosed.—8. In both cases, however, the accoucheur must act in a similar manner. When moderate tractions have no result, efforts should be directed to the evacuation of the liquid. Of all the means which may be used, this is the simplest in its application, and will be the least dangerous to the mother in well-practised hands.—9. As it has been shown that the changes in the urinary organs, which have just been treated of, do not necessarily endanger the viability of the child, the indication is to puncture the bladder with all the care such an operation requires in the adult. The implantation of the cord will be a sure guide for the choice of the most favourable spot.—10. It is not impossible that a new operation after birth might re-establish the natural passages for the excretion of urine, and thus preserve children who, without that precaution, must inevitably have been sacrificed.—*Lancet*, June 1, 1850.

MATERIA MEDICA AND THERAPEUTICS.

ON SOME REMEDIES FOR TAPE-WORM, AND ESPECIALLY THE "KOUSSO." BY M. MARTIN-SOLON.

The remedies for tape-worm are now so numerous, that it is worth while to ascertain their comparative value. The tin powder has long enjoyed a certain reputation, but has been abandoned on account of the accidents which it has occasioned. Bourdier's remedy—castor-oil with ether—is sometimes employed with success, but is repugnant to many persons; and oil of turpentine is still more difficult to administer. Yet this last, in the dose of about half an ounce in mint water with yolk of eggs, often gives satisfactory results. The male-fern-root, much vaunted for some time, and purchased by the government as a secret remedy, has generally been improperly administered, and has, for the most part, fallen into disuse. Among the "vermifuges" most commonly employed at present, must be specified the pomegranate-root-bark and the kousso.

The pomegranate-root-bark, long esteemed by the ancients, as appears from Celsus and other authors, had fallen into neglect, till Gomez of Lisbon, and Merat, brought it again into notice some years ago. Two ounces of the root bark, especially when recent, and taken in the form of decoction, at intervals of a few hours, according to the effect, generally expel the tape-worm. But the repugnance excited by the very disagreeable taste of the decoction, has generally led to the adoption of other boasted remedies, or to special preparations of the pomegranate,—such as the powder, extract, &c. These last certainly get rid of the disagreeable taste of the decoction ; but they do not prevent the remedy exerting on the digestive organs, and even on the brain, an action of such a severe and prolonged character, that both physicians and patients have often been apprehensive as to the consequences, and have therefore hesitated to continue its employment.

In this state of matters, kousso has been introduced, and several practitioners have hastened to make use of it. The value of the kousso, or kwoso, as a remedy for tape-worm, was set forth in an article in the 32d volume of the "Bulletin de Thérapeutique." The facts which have fallen under M. Martin-Solon's observation, confirm the character of that remedy for facility of administration and rapidity of action.

Dr Martin-Solon considers the kousso—the male-fern-root, if properly prepared and administered—or the castor-oil with ether, preferable to the pomegranate, which is disagreeable to take, and apt to occasion troublesome nervous symptoms. If the price and rarity of the kousso oblige practitioners to have recourse to the fern root, care should be taken, in using the powder of this plant, to use the fresh root, rapidly dried and powdered, and to make the dose from ten to twelve drachms, which is to be given in divided doses in the morning, and followed by a purge of castor-oil.

Well prepared etherial oil of fern root, in doses of half a drachm, mixed with enough of the powder of the root, or of gum to make ten pills, which may be taken at intervals of an hour, is generally efficacious,—it kills and often expels the tænia. M. Debout, of Geneva, uses an alcoholic extract, giving a drachm and a-half in three doses at intervals of an hour. Sometimes, however, after both these preparations, a dose of castor-oil is needed to expel the worm. The kousso in this respect is superior. It is to be hoped that commerce will enable us to obtain this remedy at a price which will render it accessible to all classes. —*Bulletin de Thérapeutique*, April 1850.

[These observations are full of extraordinary errors. Tin-powder, first proposed as a vermifuge by Dr Alston of this University, was distinctly stated by him to be of comparatively little use in tænia, and has never been employed, in

this country at least, as a remedy for that variety of intestinal worm. "Accidents" it never occasions, its action being merely mechanical. Pomegranate-root-bark, one of the most efficacious of anthelmintics in tænia, has never presented in British practice, either at home or in India, where it was first used, any of the inconveniences or dangers imputed to it by M. Martin-Solon, and which we take leave to say are altogether fanciful. It was not introduced into European medicine either by Gomez or Mérat, but by Dr Buchanan of the East India Company's Service in 1807 (*Edin. Med. and Surg. Journal*, iii. 22). Mr Breton wrote a fuller paper on it in 1822 (*Medico-Chir. Trans.*, xi.), which left nothing more to be said on the subject. It is true that, in 1832, Mérat published an octavo volume about it, for which he was awarded a prize of 6000 francs by the French Institute, as if he had been the discoverer. But his book adds nothing to the practical information supplied by Mr Breton, and is merely an apt illustration of that peculiar art by which our Gallic neighbours endeavour to appropriate to themselves the property of their coadjutors in science. The male-shield-fern, which M. Martin-Solon passes by so cursorily, may have been "purchased by government," but was no secret, having been a well-known vermifuge since the days of Dioscorides at least; and in modern days its efficacy was fully established, and not as a secret, by Peschier of Geneva, who ascertained that the etherial extract of the true plant (*Lastræa Filix-mas*), collected at the proper season, never failed in several hundred cases in which it had been tried by various physicians in his neighbourhood.

As for the kousso, for which the author has paved the way by depreciating all our most valued anthelmintics, and about which a good deal has recently been said in Paris and London,—in the first place, it is no new remedy. About thirty years ago it was proposed for the treatment of tænia by M. Brayer, a French physician resident at Constantinople. In 1823, Kuntz ascertained that the plant, which is a native of Abyssinia, and is derived solely from that country, belongs to the sub-order *Agrimonieæ*, of the natural family *Rosaceæ*, and constitutes a new genus, to which he gave the name of *Brayera anthelmintica*. From the flowering raceme, of which we have received a small specimen, it evidently resembles very much the common agrimony of this country, the *Agrimonia eupatoria*. In most of the journals of Europe the remedy was noticed in 1822 and 1823. In "Geiger's Magazin für Pharmacie" for 1823, it is said to have been used at Constantinople as a certain vermifuge in tænia; and M. Brayer, we are told, had confirmed its reputation by actual trial there. Mérat notices it in his "Dictionnaire de Matière Médicale" in 1829, but says that "its disagreeable odour and taste sometimes cause nausea, colic, &c." Wibmer also makes mention of it in his "Arzeneimittel und Gifte" in 1831, and quotes "Pierer's Annalen" for 1822 as his authority. It is superfluous to quote more authorities. But we may conclude with the following extract from Dr Lindley's "Vegetable Kingdom," published in 1846. Speaking of the *Rosaceæ*, he says, "One of the most powerful anthelmintics in the world belongs to this family. It is an Abyssinian plant, called cusso, or laboty, and known to botanists by the name of *Brayera anthelmintica*. Upon the authority of Brayer, after whom it is named, two or three doses of the infusion are sufficient to cure the most obstinate case of tænia."—P. 564.

We do not presume to doubt what has been said by M. Brayer, and latterly in Paris and elsewhere, of the virtues of this falsely-called *new* remedy for tænia. But grave doubts may be entertained whether it is in any respect superior to three well-known remedies,—oil of turpentine, pomegranate-root-bark, and the etherial extract of the male-shield-fern,—which seem scarcely ever to fail when duly administered. The last, in particular, can be given with great facility. It has not yet received, in this country, the attention which Peschier's investigations show that it deserves. The plant abounds in all parts of Britain. With a little care on the part of the herbalist, it cannot be mistaken. The manufacture of the etherial extract is very simple; its administration in mucilage or syrup easy; nor does it produce any great nausea

or other inconvenience. Its cheapness is also a consideration, when contrasted with the kousso, a single dose of which costs about eighteen shillings, and of which, as we learn from a London establishment, the present holder of the sole supply in France refuses to sell, to wholesale druggists in this country, less than L.400 worth.]

TEST FOR THE PRESENCE OF CHLOROFORM.

Dr Snow placed on the table, at a recent meeting of the Westminster Medical Society, the apparatus which he used for detecting the presence of chloroform in the dead body. He said that the process was a modification of one described in the "Journal de Chimie Médicale," for March 1849. The blood, or portion of the body to be examined, was put into a flask, from which there proceeded a tube, which was made red-hot in part of its course. Another glass tube, attached to the extremity of the latter, was moistened inside with a solution of nitrate of silver, and terminated in a Wolfe's bottle, the interior of which was also moistened with the same solution. Heat being applied to the flask by means of the chloride of calcium bath, the vapour given off had to pass through the red-hot tube, and any chloroform which might be present was decomposed, and the chlorine and hydrochloric acid gas being set free, were arrested in the next tube, where they formed a white precipitate of chloride of silver, which became rapidly darkened in colour by the action of light. The nature of the precipitate could be further proved by cutting the tube with a file, and introducing a drop or two of nitric acid into one portion, and of solution of ammonia into the other. He had distinctly detected the presence of chloroform by this process in two kittens, killed by inhaling the vapour, on six successive days after the death of the animals, although no precautions were taken to protect the bodies from the air, and the quantity taken by each animal must have been less than one minim. The parts of the animals examined were the viscera of the chest and abdomen, the brains and the muscles of the body and extremities. From all these parts clear evidences of the presence of chloroform were obtained. He had also obtained a precipitate of chloride of silver, by operating on some portions of the muscles taken from the leg of a child amputated under the influence of chloroform at St George's Hospital. The process indeed was one of such delicacy, that he had been able to clearly detect the presence of the hundredth part of a grain of chloroform when dissolved in a thousand grains of water. The only substances which could yield chloride of silver by this method, were Dutch liquid, chloride of ethyle, and some other bodies similar in composition and effects to chloroform, and which, however, were not in common use, or even kept on sale. There were chlorides in the human body, but these could not be decomposed below a red heat, and certainly not before the part became dry; whilst, in the process he employed, the heat to which the part under examination was exposed, was only that of boiling water, or a very little more; and it could not become dry in the most protracted examination, as the greater part of the moisture given off became condensed in the tube, which inclined upwards, and flowed back into the flask. The method, therefore, was liable to no fallacy or objection. He had received from Mr John Parrott some portions of the viscera of a woman, lately found dead, under very mysterious circumstances, in the Wandsworth Road. The parts had been enclosed up air-tight from the time they were taken from the body. They included a portion of the brain and of the liver, and though they were kept boiling for two or three hours in their own serosity, not the least trace of chloride of silver was obtained; whilst, in the instances where chloroform had caused death, the precipitate began to appear when the heat to which the part was exposed reached to about the boiling point. In the muscle taken from the child's leg, the chloroform was only a few minutes longer in being detected, although the quantity present must necessarily have been much less than in a

case where death had been caused by it. He, therefore, concluded, that the death in the Wandsworth Road could not have been occasioned by chloroform. —*Pharmaceutical Journal*, 1st June 1850.

MEDICAL JURISPRUDENCE.

TRIAL FOR INFANTICIDE—ABANDONMENT OF THE CHARGE OF MURDER.

Hannah Mitchell was tried before the High Court of Justiciary, 4th January 1850, on a charge of child-murder and of concealment of pregnancy, under the act 49 Geo. III., cap. 14.

The following were the facts of the case as given in evidence :—

The prisoner and two fellow-servants, R. and H., who were in the service of the Rev. Mr E., all slept together in one bed on the night of Monday, the 20th August 1849. Their bed-room was near the kitchen, a staircase only intervening. The prisoner rose about 4 o'clock on Tuesday morning to prepare for a washing. The other two servants remained in bed. She returned shortly afterwards, after having lighted the kitchen and boiler fires, and wakened the two other servants. At this time she said she was not well, and then went out again. R. followed her in about ten minutes, and after searching for her in the kitchen, and not finding her there, went out to look for her in a gig-house at the back of the manse, where the washing-tubs were kept. When about half-way to the gig-house, she heard the cries of a child coming from it. On this she stopped and called twice to the prisoner, "Are you there Hannah?" The prisoner told her to go to the kitchen and put coals on the fire. On this she came back and told the other servant what had passed, and asked her to go to the gig-house. H. then went and met the prisoner coming out of the gig-house with a tub in each hand. She helped her into the house with them. There were marks of blood on one of the tubs.

They all three then breakfasted together, and after breakfast the two servants, having had their suspicions raised, went out to search in the gig-house while the prisoner commenced washing. They saw at this time marks of blood on the gig-house floor, and found some bloody cloths, but nothing else. This was about five o'clock in the morning. They then searched the bed-room, where they found marks of blood on the carpet, with ashes spread over them, and in the prisoner's chest they found what afterwards proved to be the after-birth of a child. During all this time the prisoner continued washing, and did not go to bed until about 8 o'clock.

Mr E. having by this time been informed of what had happened, a midwife was sent for, and a second search was then made in the gig-house, when the body of a child was found, dead, naked, and dirty, lying on its face behind a box at the back of the gig, with a little hay spread over it. It was taken up and put into the box. The gig-house was then locked up until the medical men came, when the body of the child was put into their hands for examination in the same state in which it had been found.

The floor of the gig-house was partly paved and partly causewayed. The washing-tubs were kept on the south side, between the gig and the wall. The greatest quantity of blood was in the angle behind the door on the same side ; and there were drops of blood leading from this spot to the box at the back of the gig.

The following was the medical evidence in the case :—

William Steel, surgeon in Forfar,—Called with Dr Smith, accompanied by the sheriff and procurator-fiscal, and made an examination of the body of child found in the gig-house. We also examined prisoner's person. Made two reports, which he proved and read to the Court.

[The following is the report as to the body of the child. We omit the

report as to the prisoner's person, which is unimportant, as she acknowledged having been delivered of the child.]

"*Forfar*, 21*st August* 1849.

"We this day accompanied the sheriff and procurator-fiscal to the manse of ————, for the purpose of examining the body of a new-born infant, found in the gig-house there.

"On our arrival, the body was presented to us lying on a wooden log, and wrapped in an apron which was much saturated with blood. It was a full-grown and well proportioned male child, weighing rather more than six pounds, and measuring from the vertex to the heel twenty inches. Life appeared to be extinct only a few hours. The umbilical cord had been torn off close to the abdomen. There was a considerable quantity of meconium about the nates, and blood flowed freely from the mouth and nostrils on the slightest motion of the body. The skin was coated with the sebaceous matter peculiar to new-born infants. The body having been washed, the skin appeared pale and blanched; and the following marks of violence presented themselves:—The lower jaw-bone was fractured about an inch from its articulation with the upper jaw on the left side, and on laying open the mouth we ascertained that the soft parts about the root of the tongue, and the back part of the throat on the left side, were much lacerated, apparently by the fractured extremities of the bone, with extensive extravasation of blood into the cellular tissue; and there was a patch of ecchymosis near the tip of the tongue. Its whole structure was much swelled; and on cutting into it several clots of coagulated blood were observed.

"The whole of the left cheek, left side of the forehead, left ear, nose, lower lip, and right eyebrow, were swelled and discoloured; on cutting into these parts, all of them presented extravasated blood in the cellular membrane; two contusions, with abrasion of the cuticle about an inch long, and half an inch broad on the left side of the neck, a little behind the ear, and a similar one about half an inch in diameter under the right ear; extravasated blood was also found under these; a very severe contusion on the lower and back part of the head.

"The chest presented considerable convexity anteriorly, and on being opened, the lungs were observed of a light red colour on the surface, and also in their internal structure, and felt spongy and crepitant beneath the fingers. They in a great measure filled the cavity of the chest, their lower lobes nearly covering the pericardium. We tied the blood-vessels, and removed the lungs with the heart and thymus gland attached; the whole floated when put in water. The lungs when separated from the other organs, weighed fully an ounce and a-half, and on being put in water showed great buoyancy,—a considerable portion floated above the surface of the water,—and when cut into numerous pieces, each piece floated after strong compression; and when pressed under water, bubbles of air were distinctly noticed rising to the surface. The heart and lungs contained very little blood. The foramen ovale was open, and the ductus arteriosus pervious, but collapsed and empty.

"On dissecting back the scalp, between it and the skull, a layer of coagulated blood, about two inches in diameter, and nearly a quarter of an inch thick, was found on the lower and back part of the head, under the contusion before mentioned; there was no tumefaction nor discolouration on the crown of the head, such as is usually observed when the labour has been severe and long-protracted. The structure of the brain and its membranes was natural and healthy. None of the bones of the skull were fractured. All the viscera of the abdomen were healthy. The stomach contained a small quantity of glairy mucus. The urinary bladder was empty, and the ductus venosus was pervious, but collapsed and empty.

"We are of opinion that this infant had been born alive, and had breathed for a short time after birth; that the injuries on the head, neck, and face, and lower jaw-bone, had been produced by some blunt substance;—that the tongue had been injured by thrusting the fingers or some other substance into the

mouth; and that death had been caused by the number and severity of those injuries, and the loss of blood arising from them, and from the umbilicus. All which we certify on soul and conscience.

(Signed) "ALEX. SMITH, M.D.
"WILLIAM STEEL, Surgeon."

Cross-examined for prisoner.—Inferred that the infant had breathed from the appearance of the lungs. It had breathed, and therefore had been born alive. Child can breathe without being wholly expelled from the mother. If it breathes without being expelled, the appearances would to a considerable extent be the same, but not developed to the same extent. It is possible that a child not fully expelled might cry. Has heard a child cry though not fully expelled. The child has breathed if it cries. If the child had cried and breathed before full expulsion the appearances would not be so much developed as if it had breathed for some time. The state of the lungs was the main reason for holding that the child had been born alive, *i. e.*, wholly expelled. Knows Taylor's book on Medical Jurisprudence, and that his opinion is that respiration is not proof of child being born alive, but that it has breathed. Thinks it may have been incautious to state in the report that the child had been born alive. Must confess that it would have been better to have made the report with some qualification, and put it as to child having breathed.

All the injuries were on the head and neck. It is possible that a female in the extremity of labour might make forcible attempts to deliver herself. Natural presentation of child is by head, and hence marks of such attempts would be found about head and neck. That is where he would expect to find them. Medical men must sometimes use great force. Injury may be caused by inexperienced hands. Can't say that more injury may result from attempts at delivery by the mother. Has himself been obliged, in delivering women, to use all his force. It is difficult to account for the fracture of jaw-bone by any ordinary attempts of mother to deliver herself. Does not think that a probable cause. It is possible, but not very probable, that it may have been from a fall. A fall on the causeway was more likely to have caused it, or a fall on the edge of a tub from a considerable height. It might possibly occur from a fall from the arms on a similar projecting substance.

Has read of bones being fractured by natural expulsive action of womb. Thinks that the labour in this case was rapid, and might have been severe. "Could the injury on the back of the head have been caused by a fall?" "The quantity of extravasated blood there was very great, and that renders it less likely that it should be from a fall, but it is possible that this injury, too, might have been from a fall." "Were the injuries on the tongue likely to have been caused by the mother in aiding delivery?" "Yes, if she used very great violence. The ecchymosed marks might more readily be accounted for by efforts of mother."

All these marks could not have been occasioned after death. Extravasation of blood might be caused within a few hours after death—the blood is thinner and the tissues more readily infiltrated. Severe blows or falls might cause extravasation. Does not think the injury on tongue could have been produced after death. It is the only one that is not likely to be inflicted after death. Appearances of injuries inflicted during labour would be the same as of those inflicted after birth. They have the same character. Women often miscalculate their time. Considerable exertion may bring on labour-pains prematurely.

Re-examined.—Does not think that the delivery was protracted, from the absence of appearances which occur in such cases. When he spoke of the possibility of injuries from falls, meant at least as high as from prisoner's arms. Does not think all could have arisen from one fall. It is not very probable, but it is a possibility, that the injury on back of head, and layer of blood, as mentioned in the report, might be caused by a fall from a height of some feet. Thinks child had not fully breathed, *i. e.*, as much as if it had lived for some days. The appearances in the lungs were such as he should expect in a child

which had been born alive, and had soon after ceased to live. The lungs were such as if the child had lived a few minutes. Sound of the cry of a child, in the act of birth, is not so vigorous as the cry of full-born child.

By the Court.—Perhaps the passage in the report as to the child having been born alive, should be that our opinion was that it had breathed, but that it was impossible for us to state whether or not the whole body had been expelled previous to its having breathed. It is impossible to say if it had breathed after the whole body was expelled. Impossible to say whether respiration had taken place during birth, and ceased before expulsion, or whether it had breathed after it was born. Certainly it had breathed. In report did not mean to say that it had lived some time after birth. Impossible to be confident on that point. Impossible to say whether child had been fully expelled or not when respiration ceased. Would now word the report thus: We are of opinion that child had breathed, but it is impossible to say that it had breathed after it was born.

Alex. Smith, M.D.—Swore to the reports.

By Court.—Cannot say that the facts enabled him to say that the child had breathed after it had been fully born, only that it had breathed, but whether after being fully born cannot say. The sentence, on this point, in the report, is too strong, and would require to be modified. The child had breathed, but witness cannot say whether it was wholly born when it breathed, or whether it continued to breathe after it was wholly born. Cannot say that the child was born alive, *i. e.*, wholly out of the mother's body alive. Has seen children breathe as soon as head was expelled. Some persons think a child can breathe even before expulsion. Breathing might have ceased after head was born, and before more of the body was expelled.

On hearing the above evidence, the Crown Counsel gave up the charge of murder. A plea of concealment of pregnancy was then offered, a verdict returned in terms of this plea, and the prisoner sentenced to six months' imprisonment.

[We have to express our obligations to Mr James Adam, advocate, for the notes of the evidence in the above case, which affords a well-marked example of the difficulty which exists in procuring convictions for infanticide.]

Part Fifth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXIX.—MEETING XIII.—*May* 15, 1850.—JAMES SYME, Esq., President, in the Chair.

CORRESPONDENCE OF THE LATE JOHN WALKER, ESQ., F.R.C.S.

Mr Brown read a letter of the late John Walker, F.R.C.S., in which he detailed, with great minuteness, his mode of treating strictures of the urethra, by the application of potassa. The instruments used by Mr Walker were likewise exhibited to the Society. It appeared that Mr Walker had been led to the adoption of this practice by the example of Sir Everard Home, whose fellow-pupil he had been in the school of John Hunter, and whose name occurred frequently in the extracts from his correspondence, which were read by Mr Brown.

The *President* stated that the opinions of Mr Walker were not new to him; for after that gentleman's retirement from practice, he had often conversed

with him upon the same subject, which was so fully treated of in the letter just read by Mr Brown. Mr Walker was certainly a talented and enthusiastic practitioner, and was actuated by the best intentions in advocating the treatment of stricture by caustic. His principle was to apply the potassa to the sides of the stricture, and not to rest satisfied with pressing his caustic against its anterior extremity. Members might judge for themselves whether the mechanical contrivances exhibited could possibly accomplish this object. Now-a-days, when such an instrument as he held in his hand, of the full size of the healthy urethra, could be passed through a stricture, further dilatation would be hardly thought necessary. As to the cure of a very tight stricture by any such means, he believed it to be quite impossible. It was also remarkable that Mr Walker and others, who recommended the use of caustic, reserved the nitrate of silver for the most intractable cases, and used the potassa (a far more unmanageable and destructive agent) in treating cases in which ordinary dilatation would have effected a cure. Forty years ago Mr Benjamin Bell collected the opinions of the most celebrated surgeons in London and in the provinces, upon the subject of the treatment of stricture by caustic; and by the kindness of his grandson, Mr Benjamin Bell, he had just been put in possession of these opinions. They consisted of letters from Mr Pearson of London, Mr Hey of Leeds, Mr Park of Liverpool, and other men of note; and the arguments against Sir Everard Home's practice, which were urged by these eminent surgeons, were precisely such as were used at the present day. They detailed cases in which the practice had induced enormous mischief, by hemorrhage, fistula, hernia humeralis, and obliteration of the urethra. Although the fear of opposing the fashionable practice of the day had, forty years ago, deterred these gentlemen from publishing their opinions, there was now fortunately no occasion for such reserve; and accordingly, with Mr Bell's permission, he proposed soon to lay before the public a part, at least, of this interesting correspondence.[1] The caustic was now condemned by the majority of the profession as dangerous. Simple dilatation by the bougie was held to be the appropriate mode of treatment for most cases; and his own opinion was, that when a case was not benefited by this plan, the division of the stricture, by cutting on a grooved instrument passed through it, would safely effect a cure. But upon one point he must particularly insist,—a grooved instrument must be passed as a preliminary step in the operation. Objections had been urged against his operation, founded on the unsuccessful result of certain cases of so-called impermeable stricture, which had been treated by passing an instrument down to the seat of obstruction, and then groping for the canal behind it with the bistoury. He had found it necessary to point out the essential difference between this operation and the one which he had himself recommended; and had been asked in return, what treatment he proposed for cases of *impermeable* stricture? He had replied, that he had never met with a case in which an instrument could not be passed through a stricture, and that he would be happy to pay the travelling expenses of any patients sent to the Royal Infirmary, with certificates of impermeable stricture, signed by a London hospital surgeon. He trusted that he could demonstrate, by the public treatment of such cases, that the impermeability had no real existence when tested by the careful use of bougies of sufficiently small size.

Dr Maclagan had witnessed Sir Everard Home's practice in St George's Hospital, many years ago. The use of the caustic caused an amount of suffering which it was painful to recall; it was followed by hemorrhage in a great number of cases, probably amounting to a tenth of those treated; it never effected a cure which could not be attributed to the simple dilatation of the urethra by the bougies employed; and relapses were extremely frequent in Sir Everard's practice. It was well known that the results observed in Cal-

[1] This will be found at page 1 of our present Number.

cutta by Dr Hare, in the practice of certain surgeons who used the caustic, were anything but favourable.

Dr Dunsmure expressed himself as highly gratified with the success of Mr Syme's operation in two cases treated in the Royal Infirmary, in which he had himself adopted it.

Mr Kerr mentioned a case in which a French surgeon in Malta had inflicted such injury on a patient's urethra by the application of the caustic, that he was obliged to send him to England to be cured.

CASES IN MIDWIFERY.

Dr Charles Bell then read an account of two cases in midwifery requiring manual and instrumental interference. In the first it was found necessary to pass the hand into the uterus, in order to effect the removal of the placenta. For several days after delivery, the case appeared to go on favourably ; the pulse, however, was constantly above a hundred, and the patient did not sleep. On the evening of the seventh day she was suddenly seized with trismus. She continued in this state for upwards of six hours, when the spasms ceased, and did not return. Camphor, acetate of ammonia, and mur. morphiæ, were now prescribed. On the afternoon of the tenth day she had a severe rigor, followed by loss of speech, and hemiplegia of the whole right side of the body, but not of the face. Her pulse being so feeble as to contra-indicate bleeding, powders containing calomel, with James' and Dover's powders, were given every second hour ; and a mixture prescribed, containing liq. ammon. acetat., camphor, and vinum antimoniale. On the twelfth day there was diarrhœa, which soon yielded to chalk, catechu, and laudanum ; the calomel and Dover's powder being at the same time omitted. On the sixteenth day a blister was applied to the occiput, and ext. of colocynth and of hyosciamus given, along with small doses of croton oil. The blister was repeated, the raw surface occasionally pencilled with the aromatic acetic acid, and the croton pills given for some time twice a-day. A large slough formed on the back, and as it gradually separated, the paralysis, which had already yielded slightly to the counter-irritation, almost disappeared ; so that, six weeks after her confinement, the patient was able to go into the country. She can now walk and speak nearly as well as before her illness, the only trace of her complaints remaining being an apparent difficulty in finding words to express her ideas.

The second case was one of twins. The head of the first child—a remarkably small female one—presented in the third position, and the labour pains being ineffectual, was finally extracted by the forceps. It was not found necessary to carry the forehead round into the hollow of the sacrum. The child at birth showed but faint signs of life. By the use of the warm-bath, artificial inspiration, and of stimuli applied to the chest and nostrils, respiration was in about half an hour fully established. The uterine contractions were not renewed for two hours, and were at first lingering and ineffectual. What appeared to be the second bag of membranes was therefore ruptured ; but the fluid which came away proved to be blood, which it was afterwards ascertained had been contained in a cavity between the two sets of membranes. The second set having been ruptured, a loop of umbilical cord descended, and was followed immediately by the head of the child. Every effort was made to protect the cord from pressure by pushing it upwards during the intervals of the pains ; it ceased, however, to pulsate a few minutes before the delivery of the head ; and the child—a remarkably large one—was still-born. The conclusion of the paper was occupied with remarks upon the relative frequency of the third and other presentations of the head, which the author thought had been overstated by Naegele ; and upon the mechanism by which the contraction of the round ligaments and of the circular and longitudinal fibres of the uterus direct it into the axis of the pelvis, retain it there, give a semi-rotatory motion to the head of the child, necessary for its adaptation to the intricate passage of the pelvis, and finally accomplish the delivery.

MEETING XIV.—*June* 5, 1850.—JAMES SYME, Esq., President, in the Chair.

CASE OF CÆSAREAN SECTION. BY GEORGE HARLEY, ESQ.

Professor Simpson read a case of Cæsarean section recently performed by Mr Harley in the Edinburgh Maternity Hospital. The mother died from effusion into the air-passages, consequent upon disease of the heart. The Cæsarean operation was had recourse to by Mr Harley, a few minutes after her death, and the child was now doing well.

(This case is given at p. 21 of our present Number.)

CASE OF CÆSAREAN SECTION. BY DR NIMMO.

Dr Simpson read another case of Cæsarean section, performed upon the living mother, by Dr Nimmo of Dundee. In this case there was extreme deformity of the pelvis. The mother died a few days after the operation. The child was saved. The pelvis, a fine example of malacosteon, was exhibited to the Society.

(This case will shortly be published in this Journal.)

The President remarked that a woman whose osseous system was in the condition indicated by the pelvis on the table, was certainly a very unfavourable subject for any surgical procedure. He agreed with Dr Simpson that great credit was due to Mr Harley for the promptitude and resolution which he had shown in operating on the patient in the Maternity Hospital. It was remarkable that the mothers who had had Cæsarean section performed in this country had all died; yet there were records of cases, in which the abdomen had been torn open by a cow's horn, and in which both child and mother had recovered.

Dr Simpson spoke of the rarity of the Cæsarean operation in Scotland. It had not been performed on the living mother in Edinburgh for fifty years, nor in Dundee since 1823. Dr S. alluded to two cases in which nature had accomplished delivery by rupture of the uterus, and subsequent laceration of the abdominal parietes. There were two cases on record in which the abdomen had been torn open, and the child turned out by goring by the ox's horn; a third occurred in India, near the military station where his friend Dr Balfour happened at the time to be; and a fourth had been mentioned by Dr Thatcher to Dr S., as having occurred in the King's Park, in Edinburgh. In all these four the mothers recovered. The successful result of such cases, and under such rude surgery, showed that the soundness of the constitution of the patient formed the most important element in the result. It had failed often, as in Dr Nimmo's interesting case, when performed with all surgical tact and care, but when, as in this case, the constitution of the patient was unsound, and previously broken by the preceding labour. He had lately, when at Bonn, an opportunity of seeing a woman, who had been thrice operated upon by Professor Kilian, and who looked forward with great seeming indifference to the probability of a fourth operation being requisite.

Dr Thatcher had had considerable experience of the operation as performed after the death of the mother. It had been, he thought, too much neglected after cases of sudden death; indeed, he considered it a duty to perform it in all such cases, if pregnancy was advanced to the seventh month, and, if, from stethoscopic or other evidence, any sign of the vitality of the child was obtained. That the child was to a certain extent endowed with a vitality independent of the mother, was quite certain, and was shown by the motions observed in utero after the death of the mother, and by the successful result of cases in which the section was performed in such circumstances. In three of the cases in which he had himself operated, the child lived for two or three hours. The operation might be the means of saving many lives. In the case alluded to by Dr Simpson, where the operation was accidentally performed by a cow's horn, the child was saved as well as the mother.

Dr W. T. Gairdner referred to Buffon's experiments upon the extreme tolerance of asphyxia, observed in animals just born.

Dr Simpson said that the tolerance of injuries decreased directly as the age of the child increased. In corroboration of this doctrine he instanced the ex-

traordinary spontaneous amputations which take place in utero, and expressed his belief that, in early fœtal life, the head itself was sometimes destroyed by inflammation and the process of spontaneous amputation, without the existence of the fœtus being necessarily compromised. The experiments of Buffon had been generally confirmed by Edwards, and it was quite clear that newly-born animals could bear a want of respiration for a considerable time. Baillard, who made many accurate observations in the Hôpital des Enfans Trouvés, never observed symptomatic or traumatic fever in a child under a month old. The nearer the condition of the fœtus was to that of the lower animals, the greater was its tolerance of injuries, and its power of repairing them. Some years ago he showed to the Society several cases of intra-uterine amputation of the arm, in which there was an attempt at the regeneration of the hand, such as is seen after injuries of the extremities in amphibia, &c.

TREATMENT OF AFFECTIONS OF THE LARYNX. BY DR JOHN SCOTT.

Dr John Scott read a communication on the treatment of certain affections of the larynx, by the local application of the nitrate of silver in solution.

(This communication appears at p. 15 of our present Number.)

Dr Spittal believed that the sponge would be most easily passed within the rima glottidis, if the patient were desired to take a full inspiration while the attempt was made to introduce it. In a case of attempted suicide by cutting the throat, he had had an opportunity of seeing how widely the rima expanded during inspiration.

The *President*, *Dr Maclagan*, and *Dr Gairdner*, severally expressed themselves as satisfied that the operation was practicable, safe, and in certain cases beneficial.

Dr Bennett stated that he had lately treated several cases of chronic laryngitis, and of the follicular disease of the pharynx, described by Dr Horace Green. He fully agreed with Dr Scott as to the great value of the treatment he had brought before the Society. Every practitioner must have met with cases of individuals who for years had been more or less troubled with cough, and been in the habit of taking expectorant and anodyne mixtures, cough drops, lozenges, &c. Such cases were generally considered as examples of chronic bronchitis, although examination of the chest failed in discovering any physical signs of the disease. Dr Bennett was persuaded that many such cases depended on disease of the pharynx and upper part of the larynx. He had a case now under his care of this description, where a few applications of the caustic solution had already mitigated the cough, although it had been of some years' duration. He had lately conversed with some practitioners in London, who had had considerable experience of this practice, and had been told by some of them, that in phthisis-laryngea it was not so beneficial as in the simple follicular disease. There could be no doubt, however, that in this country our experience of the practice, and its comparative usefulness in various forms of pharyngeal and laryngeal disease, had yet to be acquired. He considered that the application of caustic solutions to the laryngo-pharyngeal membrane should not be made indiscriminately, and by inexperienced persons; and that a thorough knowledge of auscultation was often necessary to establish the diagnosis, and to ascertain how far the pulmonary organs were implicated. Employed rashly or injudiciously, it was more likely to prove injurious than beneficial.

Dr Bruce, an American physician, who was present as a visitor, said that he had himself once been a sufferer from chronic laryngitis and follicular disease of the pharynx, and that after four months annoyance, he was greatly relieved by the application of a strong solution of nitrate of silver (ʒj. to ʒj.) to the parts affected. After the first sensation of choking, the relief was indescribable, he felt "as if he had no larynx," so completely did all uncomfortable feelings leave him. The remedy was in general use in America. He had not witnessed any good effect from its employment in croup or diphtherite.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION IX.

SPURIOUS PREGNANCY—ITS FREQUENCY AND NATURE.

Dr Simpson first showed how frequent and marked the phenomena of spurious pregnancy were among domestic animals, as in the bitch, cow, &c. In these animals the phenomena of pregnancy often occurred both subsequently to unsuccessful sexual intercourse, and also frequently after seasons of heat, but during which there was no intercourse with the male. Occasionally the phenomena of pregnancy in them were suddenly interrupted, and terminated a short time after their commencement; sometimes, however, they went on increasing to the full term of pregnancy, and ended in a series of puerperal phenomena, as the presence of milk in the mammæ, &c. &c. Dr Simpson adduced a variety of facts, showing the great frequency with which the same phenomena of spurious pregnancy occur in the human female in a more or less marked degree, and how these phenomena are often mistaken for dyspepsia, hysteria, and various anomalous and perplexing ailments. And as these phenomena sometimes occur in the virgin state among the lower animals, so do we find them also sometimes occurring in the unmarried human female, and giving rise to symptoms that have hitherto defied any nosological arrangement. Lastly, he spoke of the great use of aperients combined with tonics, and with nux vomica and galbanum in the subjugation of the phenomena of spurious pregnancy.

SPURIOUS PREGNANCY, FOLLOWED BY SPURIOUS PARTURITION.

Dr Keiller communicated the particulars of a case in which the symptoms of spurious pregnancy, and subsequently those of spurious parturition, existed to such a remarkable degree, as to induce the patient and her friends not only to prepare for the expected accouchement, but, when the supposed full time arrived, to believe in the actual commencement and continuance of a very painful labour, which ultimately became so protracted as to demand, according to the opinion of the attending accoucheur, the immediate and unavoidable performance of the *Cæsarian section!*

Dr K., having been requested to visit the case for the purpose of satisfying the friends of the patient as to the propriety of having recourse to such an extreme mode of accomplishing the delivery as that which had been seriously proposed to them by the individual in attendance, was astonished to find, on making an examination, without any misgivings as to the parturient condition of the patient, that all the supposed symptoms of pregnancy and of parturition had been, and were still, *entirely spurious*, the uterus being evidently unimpregnated!

This startling opinion was anything but credited at the time by the females present, who altogether ridiculed the idea of the case not being one of "*real labour*," as the motions of the child had long been not only distinctly felt but even seen "through the walls of the much distended abdomen," the patient herself insisting that the child's movements were so violent that she felt "as if it would leap through her side!"

The result of the case, however, sufficiently proved that her painful attempts at delivery could not possibly have been rendered less futile by the *Cæsarian section*, or any other *obstetrical* aid, as *pregnancy never had existed!*

The patient remained for a considerable time afterwards under the immediate care of Dr Keiller, who stated that he considered the entire group of anomalous symptoms (which she presented in a very singular degree, and which he purposes recording) in a great measure referable to hysteria.

COLD AS AN EXCITANT OF FŒTAL MOVEMENTS. BY DR SIMPSON.

A variety of observations and experiments were stated, showing that, contrary to the common opinion, the mere application of cold (as a cold hand, &c.)

to the surface of the abdomen of a pregnant woman, had not the effect of exciting motions in the fœtus. The application of portions of ice, even of the size of the hand, had no such property.

INDIAN HEMP AS AN OXYTOCIC.

Dr Simpson stated, that, in the early part of the winter session, he had given Indian hemp (Cannabis Indica) in several cases of tedious labour, with the view of ascertaining if it possessed any oxytocic effect (like ergot of rye) in increasing and exciting the parturient action of the uterus. He had been induced to try the effects, if any, of Indian hemp during labour, in consequence of Dr Churchill stating, that it possessed powers similar to those of ergot of rye in arresting hemorrhage, when dependent upon congested states of the *unimpregnated* uterus. In the few cases of labour in which it was tried, parturient action seemed to be very markedly and directly increased after the exhibition of the hemp; but far more extensive and careful experiments would be required, before a definite opinion could be arrived at relative to its possession of oxytocic powers, and their amount.

Meeting V.—*April* 24, 1850.—Dr David Wilson in the Chair.

CASE OF SLOUGHING AND REPRODUCTION OF THE NIPPLES. BY DR MORTON.

The following communication from Dr Morton, of Glasgow, was read to the Society:—

On the morning of April 1, 1849, I was called to Mrs P——, a small and slender woman, in labour of her first child, and in the course of two hours she was easily and safely delivered of a small but healthy female child. I saw her daily till the 7th inclusive, when I discontinued my attendance, as she was now convalescent. During this time she complained a little of the nipples being sore when the child was put to the breast, and on examining the nipples they were observed to be small, though well shaped, and perhaps less firm, and also of a less florid colour, than usual, but more like the colour of venous blood, or of a purplish tinge.

On being asked to see her four days subsequently, I was much surprised, indeed, to find that both nipples had sloughed, the ash-coloured slough of each still lying in *situ*. These were easily lifted off, and exposed clear granulating sores, depressed below the level of the surrounding skin, from which exuded a little milk, mingling with the purulent discharge natural to such surfaces. She was directed to use warm water dressing, with a covering of oiled silk, and to clean the parts thoroughly and frequently. The milk continued to exude from the ducts occasionally, thus keeping them pervious; and this she encouraged as much as possible, as I have since learned; and on seeing her two months afterwards, I was very much gratified, indeed, to find that she already had nipples which the child could suck—a result which I certainly did not anticipate, and a much more fortunate one than I had expected. It is almost superfluous to add, that, had I anticipated even the sloughing, but more especially the restoration of these organs, the case would have been more assiduously watched.

After the sloughing of the nipples, the child was nursed by another female for three weeks, or about that time, when the one breast of the mother was so much improved as to admit of the child sucking it twice or thrice a-day. This was the left breast. The right one, which seemed at first to have suffered most, the slough from it being the largest and deepest, was a fortnight later in arriving at the same condition; so that the breast which suffered most severely had been occasionally drawn every day for fully three weeks before I saw them, as it unfortunately proved, for the last time. The nipples were still very small, but firmer and more florid in colour than the previous ones had been; in fact, they presented an exceedingly smooth and beautiful appearance. The child could supply itself from them with great apparent ease, which fact I then verified by personal observation, although, had I not seen it, I would

have been disposed to doubt its possibility; but the child, though small, was plump and firm, and what is called a *good sucker*.

As the cause of the sloughing, I felt disposed, at the time, to assign the apparently low state of vitality of the nipples themselves, which rendered them unable to sustain with impunity the pressure produced by the gums of the infant. By this, the structure of the nipples seemed to have been completely destroyed, and the process of separation was nearly completed when I saw them. No other cause was known to the parties in the house, and this is the only explanation which occurs to my own mind. It is probable, however, that in both breasts a part of the erectile tissue of the nipple remained undestroyed—a mere trace, it may be, which would form a basement or matrix for their regeneration; and it cannot be doubted that the suckling of the infant, and even its very presence, would tend to foster the growth which had already been begun.

On the 21st September last, I called at the house where my patient had resided, for the purpose of examining the nipples, expecting to find them now much better developed, three months having elapsed since I last examined them. I was much disappointed and grieved to learn that, about ten or twelve days before, being in Saltcoats, her native village, Mrs P——, her father, mother, and brother, had died of epidemic cholera, and her child of hooping-cough and its complications—all within the space of three days. A few days afterwards I saw the husband, now widower, who informed me that the nipples had become much better developed after I had seen them last, and that the one was always much stronger and better formed than the other, a circumstance frequently observed in ordinary cases.

While the history of this case inculcates upon us a more implicit reliance on the powers of nature, it also obviously suggests the valuable practical inference, that in such or similar cases, we ought hopefully to employ every means in our power to favour the production of healthy granulations, more especially where there has been a loss of substance.

During the very limited course of my reading, and any search which I have been able to make into obstetric works, or others referring to the mamma, or to reproduction of different parts of the human body, I have not met with any recorded case either of sloughing or reproduction of the nipples. Should this be, therefore (and, so far as I know, it is), the only case of the kind hitherto observed, it is valuable as a physiological fact, forming a small item among the number of our recorded observations,—a sufficient reason for obtruding it on your notice. I had almost omitted to mention, that no appearance of chopping was observed on the original nipples; there was no inflammation, or swelling of the mammæ themselves, consequently no retraction of nipples, seeing there was no cause for it.

[We are obliged to postpone, till our next Number, a Case of Trismus Nascentium, communicated by Dr Imlach of Liverpool, which was read at this meeting.]

SIMULATED DISEASE.

A strange case of this description has lately been detected in the Sussex County Hospital:—A woman, Betsy Ginn, aged 23, was brought before the weekly Hospital Board, charged with wilfully producing the disease for the cure of which she had applied to the hospital. The statement made against her was, that very numerous diseased patches of the skin, over nearly the whole of her body, limbs, and face, were the result of her own application to the parts of hydrochloric acid. Several patches were nearly as large as the palm of the hand; and they were in different stages, the recent ones being yet in a gangrenous condition, others (from which the sloughs had separated) were deep and troublesome ulcers, and many (the majority) had healed, but with disfiguring scars, and in some places to the injury and contraction of the adjacent sinews. The woman, after many denials and prevarications, at length admitted

her guilt ; and further, that she had been practising the deception for a period of nearly three years, four months of which she had spent in the Colchester Hospital, and nine weeks in University Hospital, London, without the imposture being discovered. While begging for mercy, she stated that she had been induced so to act, in the hope of obtaining a better home than a workhouse. To the foregoing instance of fraud, which we abridge from the *Essex Herald*, we are enabled to add a few particulars :—When in University College Hospital, this person suffered, or at least complained of suffering, almost constantly from nausea, and she took little food—in fact, refused almost all of a solid kind. The case was at the time regarded as identical with those described by Sir B. Brodie, under the heading, " Peculiar Species of Dry Gangrene of the Skin," in his latest published work, viz., " Lectures Illustrative of various subjects in Pathology and Surgery." (Page 382.) The foregoing circumstances were well calculated to lull suspicion ; but a question now arises, as to whether the loathing of food was real, or only pretended, the better to carry out the deception. Food might have been procured and eaten at night. The cases mentioned by Sir B. Brodie as having been observed by Mr Keate and himself must, we apprehend, be placed in the same category as that above noticed, at least until some *bona fide* example of the same appearances, arising without a like cause, shall be placed on record.—*Medical Times*, May 18, 1850.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

It has been decided that there is to be no Medical Section at the meeting, which commences on the 31st of this month, in Edinburgh. The number of societies, and the facilities for publication which exist in the city, seem to render it unnecessary. Anatomical, physiological, and pathological papers will, therefore, as heretofore, be brought before the Natural History Section. The College of Physicians have decided on entertaining the leading members of the Association at Barry's Hotel, and about 150 will be invited to dine with the President of the College of Surgeons, Mr Syme, at Millbank.

VARIETIES.

Edinburgh Botanical Museum.—In addition to the usual grant for maintenance of the Botanic Garden, the Commissioners of her Majesty's Woods and Forests have this year placed at the disposal of Dr Balfour, a sum for building a large room, in which a botanical museum is to be formed. There is a magnificent collection of this kind at Kew,—the only one, we believe, in the kingdom,—which, there can be little doubt, will soon have a worthy rival, through the active exertions of the Edinburgh professor.

Persecution of an Edinburgh Graduate, by the London Apothecaries' Company.—In the " Lancet " of June 8, 1850, will be found an account of a series of actions, brought by the London Apothecaries' Company, against Dr J. G. K. Burt, of Wisbeach, an Edinburgh graduate, and a most respectable practitioner, of twenty-two years' standing. His only crime was the possession of such superior acquirements as to have gained the confidence of numerous families, and thereby excited the jealousy of certain English apothecaries. The case is still pending ; but all the circumstances only afford another proof of the mischievous tendencies of the worshipful company—impotent in defending the public against ignorance and quackery—active in upholding absurd privileges—powerful in injuring men of undoubted respectability and ample medical qualification. The case is most disgraceful to the Apothecaries' Company.

Duties of an Apothecary.—The " London Journal of Medicine " gives the following quotation from C. Bullein's work, entitled, " Bulwarke of Defēce

against all Sickness, Sornes, and Woundes, that dooe daily assaulte Mankinde." (London: 4to. 1562.):—

"That he neither increase nor diminische the physiciā's bille; and kepe it for his own discharge.

"That he neither buie nor sell rotten drugges.

"That he peruse often his wares, that thei corrupt not.

"That he put not in *quid pro quo* without advysement.

"That he maie open well a veine, for to help the pleurisy.

"*That he medle onely in his vocacion.*

"That he delite to rede Nicolaus Myrepsi, Valerius Cordus, Johannes Placaton y^e^ Lubik, &c.

"*That he dooe remember his office is onely to be the physician's coke.*

"That he use the true measure and waight."—*Apotecaries' Rules, by W. Bullein.*

METHOD TO AVOID THE EXPENSE OF SOLITARY IMPRISONMENT.—In order to save Government the enormous expense of prison establishment for individuals condemned during life to isolation, Dr Froriep has published at Weimar a work, in which he proposes to employ an apparatus that would, in one moment, deprive the condemned of sight and hearing. In this way, he says, perfect isolation would be obtained, while all the individuals were contained in one place. —*L'Union Médicale, 30th Avril,* 1850. [This is evidently copied from the "Mysteries of Paris."]

ROYAL GRATITUDE.—Some three hundred years ago, the King of Savoy was the subject of obstinate constipation,—royalty being in those days a very sedentary profession, and apt to induce this troublesome affection. After vainly appealing to the skill and knowledge of the physicians and apothecaries in different parts of his dominions, the prince was finally fortunate enough to find means of deliverance from his distresses. It was at Belley that this triumph of art took place, as may be gathered from the following extract from the patent of nobility, conferred upon a practitioner of that city by the grateful monarch, "*propter libertatem alvi nobis feliciter redditam.*"—*Gazette Médicale de Paris.*

SUCCESSOR TO M. BLAINVILLE.—M. Duvernoy, who was honourably associated with Baron Cuvier in some of his great works, has been appointed to fill the chair at the Jardin des Plantes, vacant by the death of M. Blainville.

SUPPRESSION OF MEDICAL SCHOOLS IN THE FRENCH MILITARY HOSPITALS.—The economical disposition of the French Government has induced it, besides diminishing the salaries of the professors of the medical faculty, to suppress the medical schools in the military hospitals of Paris, Lille, Metz, and Strasbourg. The ground alleged is, that an experience of fourteen years has shown that no especial advantage is to be derived from these establishments.

THE BELGIAN PHARMACOPŒIA.—There has been considerable difference of opinion as to what language the new Belgian Pharmacopœia should be published in. The Academy desires that it should be in French; the medical commission and the druggists wish it to be in Latin. The government has decided that it shall be printed in *both* languages.

VACCINATION IN TURKEY.—Four physicians (two German, one French, and one Italian) are now authorised by the Turkish government to vaccinate in Constantinople. At first christian families only took advantage of their services, but at length many of the Turks followed their example. In 1847, during six months, 2128 individuals were vaccinated. Similar arrangements have been made in all the large Turkish cities, especially in those under the influence of European commerce.—*Gazette Médicale,* April 27, 1850.

NEW THEORY OF THE ORIGIN OF TUBERCLE.—M. Meigs thinks that tubercles are produced by a clot, which at first forms in the heart, and then divides in the circulating blood, so that small particles pushed in different directions within the capillaries constitute the origin of these deposits.—*Gazette des Hôpitaux,* March 2, 1850.

Surgeons and Undertakers.—Mr Sadler, a great metropolitan undertaker, in his evidence before the Board of Health, in regard to the sepulture question, states that "it is known that surgeons take commission on funerals;" so that, after an unsuccessful operation, the surgeon, besides being paid for his performance, may derive some further *solatium* for the loss of his patient.—*Report on Extramural Sepulture*, p. 133.

Expulsion of Foreign Bodies from the Larynx.—Dr Charles Hansford, of Knoxville, in a letter to the editors of the *N. W. Med. and Surg. Journal* (May 1849), relates the case of a coloured girl, who had accidentally got a pin into the windpipe, and was suffering from all the distressing symptoms consequent upon obstruction of the air-passages from a foreign body. She was directed to lie upon a bench, face downwards, with the head projecting over the edge, and to take a full inspiration. Whilst in this position, with the lungs filled with air, a smart blow or two on the back with a pillow, made hard and firm by compression, had the effect to expel the pin at once from the larynx. In this case, the first blow moved the pin about an inch, the second forced it into the mouth. "Since that time," Dr H. writes, "I have had several opportunities of trying the maul made of a pillow. I have driven out water-melon seeds in this manner, on three different occasions; a grain of corn, at one time; and a large glass bead at another."—*American Journal of the Medical Sciences*, July 1849, p. 294.

Death of the Siamese Twins.—These twins are said to have recently died of marasmus. On post-mortem examination, the livers were found to have been united by a process of half-an-inch in thickness, thus confirming the opinions of those who believed that an intimate connection existed between the peritoneal cavities, contra-indicating any attempt to separate them by operation.—*Gazette des Hôpitaux.*

Mr Syme's Challenge.—A difference of opinion exists between Mr Syme and certain London surgeons as to the existence of impermeable stricture of the urethra. Mr Syme says there is no such thing, and undertakes to pass a bougie through any stricture that may be presented to him. More than this, he agrees to pay the expenses from London and back again of any patient that may be sent to him, with a certificate from a London hospital, stating that the man has an impermeable stricture; and undertakes to treat him publicly in the wards of the Royal Infirmary, and publish his case.—*Lancet*, 1850.—[This challenge has not yet been accepted.]

BOOKS RECEIVED.

Is the Practice of Medicine in 1850 a Degenerate Pursuit? By a Practitioner. Second Edition. London: 1850.

On the Development of the Great Anterior Veins in Man and Mammalia. By John Marshall, F.R.C.S., &c. (From the Physiological Transactions.) London: 1850.

Iodognosie ou Monographie Chimique Médicale et Pharmaceutique des Iodiques, &c. Par Dorvault. Paris: 1850.

The Sumbul: A New Asiatic Remedy, &c. By A. B. Granville, M.D., F.R.S., &c. London: 1850.

An Inquiry, How Far Consumption is Curable, with Observations on the Treatment, and on the Use of Cod-Liver Oil, and other Remedies. By James Turnbull, M.D., Physician to the Liverpool Infirmary, &c. Second Edition. London: 1850.

Hospital. (An Article from the Cyclopædia of Practical Surgery.)

A Short Account of the Steam-Engine, and of its Inventors. By George Hamilton, M.D. Falkirk. Edinburgh: 1850.

Memoir on the Influence of Hypertrophy and Dilatation on Diseases of the Heart, and on some Points in their Diagnosis and Treatment. By A. Halliday Douglas, M.D., &c. Edinburgh: 1850. (Reprinted from this Journal.)

Some Remarks on the Contamination of Water by the Poison of Lead. By James Bower Harrison, M.R.C.S., &c. London: 1850.

The Unity of Nature. By Charles Bland Radcliffe, M.B. London: 1850.

The Causes, Symptoms, and Treatment of Eccentric Nervous Affections. By William John Anderson, F. R. C. S. London: 1850.

Rendiconto dei Lavori dell' Accademia Medico-Chirurgica Napolitana. Nos. for 1847, 48, and 49.

EXCHANGE LIST.

British and Foreign Medico-Chirurgical Review.—Received regularly.

Dublin Quarterly Journal of Medical Science.—Do. do.

American Journal of Medical Science.—Do. do.

Annales Médico-Psychologiques.—Do. do.

Schmidt's Jahrbücher.—Do. do.

Zeitschrift der K. K. Gesellschaft. Vienna.—None received this year. We have, consequently, stopped our exchange.

Zeitschrift für Rationelle Medicin von Henle und Pfeufer.—None received this year. Our exchange must stop.

Bibliothek for Læger.—A parcel lately received.

Hygiea. Stockholm.—Received irregularly, and at long intervals.

Psychological Journal.—Regularly received.

Veterinary Record.—Do. do.

Pharmaceutical Journal.—Do. do.

London Journal of Medicine.—Do. do.

Archives Générales de Médecine.—May number received.

Philadelphia Medical Examiner.—First two numbers for 1850 received. From inadvertence on our part, the exchange has not been conducted so regularly as, we trust, it will be in future.

Charleston Medical Journal.—Always sent by post. Our exchange has been stopped.

Journal de Médecine et de Chirurgie Pratiques.—Nos. for Mars, Mai, et Juin not received.

Revue Médico-Chirurgicale de Paris.—Nos. for Mars, Avril, et Juin not received.

Chemical Gazette.—Regularly received.

Provincial Medical and Surgical Journal.—Do. do.

Medical Gazette.—Do. do.

Medical Times.—Do. do.

Dublin Medical Press.—Do. do.

Gazette Médicale de Paris.—Received regularly, with the exception of Nos. 6, 7, and 10, which have not reached us.

Gazette des Hôpitaux.—Regular.

L'Union Médicale.—Do.

Bulletin Général de Thérapeutique.—Do.

Gazzetta Medica Italiana Lombarda. We have not received Nos. 2 and 4 of Serie Terza. With these exceptions, regular.

Oppenheim's Journal.—Exchange is stopped, as not a single number for 1850 has reached us.

Boston Medical and Surgical Journal.—We shall be happy to exchange from May 1850.

British American Journal of Medical Science. Montreal.—We have sent a complete set of our Nos. for 1850, and shall be happy to exchange.

Henke's Zeitschrift für die Staatsarzneikunde.—We shall be glad to exchange, and have sent our May number.

Journal für die Kinderkrankheiten von Behrend und Hildebrand.—We shall be glad to exchange from May 1850.

Rendiconto dell' Accademia Medico-Chirurgica Napolitana.—We beg to thank Professor Vulpès for the numbers sent, and shall be happy to exchange.

N.B.—French Journals are forwarded to us by J. B. Baillière, Paris.

German Journals should be sent through the House of Hartmann in Leipsic, to care of Williams and Norgate, Henrietta Street, Covent Garden, London.

No foreign parcels can be received unless sent free. This rule is absolute, and its infringement has put us to the necessity of stopping several exchanges, especially with American Journals.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Stricture of the Eustachian Tube, with an account of the appearances presented on dissection.* By JOSEPH TOYNBEE, F.R.S., Fellow of the Royal College of Surgeons in England, and Senior Surgeon to the St George's and St James's Dispensary, London.[1]

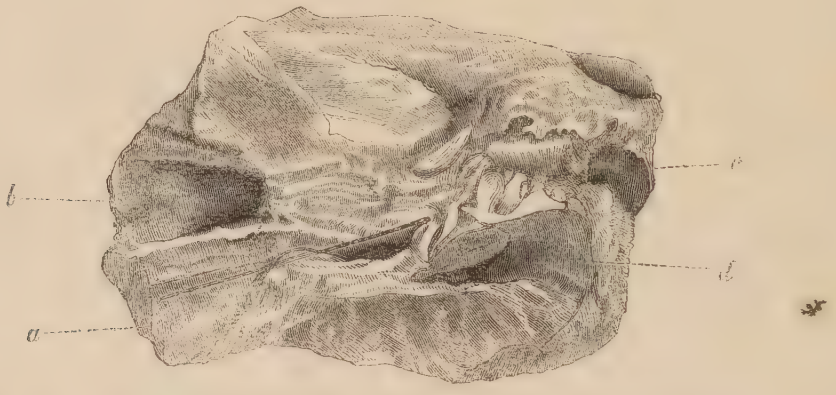
WRITERS on diseases of the ear have enumerated various sources of obstruction of the Eustachian tube. Of these, the principal are stated to be collections of mucus or blood, thickening of the lining mucous membrane, and the adhesion of the parietes. But I am not aware that hitherto the existence of these pathological conditions has been verified by the examination of the parts after death. Convinced that the latter course is the only one which can lead to a more complete and accurate knowledge of the diseased conditions of the ear, and believing that in no part of aural surgery is there so great a want of precise information as in regard to the affections of the Eustachian tube, I take the opportunity of laying before the profession the following case:—

C. J., æt. 45, was visited by me in the month of November 1849. At the time I saw him he was dying from tubercular disease; I was, therefore, precluded from making so minute an examination as was desirable. The history of the origin and progress of the deafness, as nearly as could be ascertained, was as follows:—About twenty years ago the patient suffered from a violent inflammation in the *right* ear, followed by a discharge from the external meatus, which had continued almost without intermission up to the time he was seen by me. For a considerable period this ear had been unable to distinguish sounds. The *left* ear had been gradually growing less and less sensitive to sonorous vibrations during the preceding six or seven years. No pain had been felt in it, however, and there had been no discharge from the external meatus.

Upon inspection by means of a speculum and lamp, the *membrana tympani* of the *right* ear was observed to be absent, while the mucous membrane lining the tympanic cavity was very thick, and covered by a considerable quantity

[1] This case was read before the Royal Medical and Chirurgical Society of London, February 12, 1850.

of purulent matter. In the *left* ear the lining membrane of the external ear was slightly reddened, and the membrana tympani was as white as writing paper; the handle of the malleus, usually so distinctly seen, was not distinguished from the surrounding membrane. Towards its centre, the surface of the membrana tympani had lost the natural shining appearance, but a small portion of the surface on each side of the handle of the malleus, although perfectly white, was so smooth as to reflect the light thrown upon it by the lamp. As the patient was in a state of great debility, the otoscope[1] was not applied to either ear to ascertain the condition of the Eustachian tubes. The patient died a few days after the examination.



a, The Eustachian tube; a bristle is introduced through the strictured portion.
b, The carotid canal.
c, The mastoid cells.
d, The external meatus, at the inner extremity of which the membrana tympani is seen.

Post-mortem Inspection.—Right Ear.—The mucous membrane lining the tympanic cavity was thick and ulcerated in parts, and the bone forming the upper wall of the cavity, with which the ulcerated membrane was in contact, was carious. The Eustachian tube was healthy.

Left Ear.—The central portion of the membrana tympani was found to be white and thick, but those parts of it which lie anteriorly and posteriorly to the handle of the malleus were soft and attenuated, and the white appearance was due to the presence of mucus in the tympanic cavity immediately in contact with the inner surface of the membrana tympani. So softened was this portion of the membrana tympani that, on applying very slight pressure, the fibres of which it was composed gave way, and three small orifices were produced. The cavity of the tympanum and the mastoid cells were filled with white thick mucus, and no air was discoverable. The mucous membrane lining the cavity was also thicker than is natural.

The Eustachian Tube.—The internal portion, for the length of half an inch, was healthy; but, at about that distance from the cavity of the tympanum, there was a sudden constriction, and for the length of about a line and a-half the tube was so contracted that, even when its anterior wall was removed, it was with difficulty that an ordinary-sized bristle could be introduced into the opening. This stricture resulted from the external and internal walls of the tube pressing against each other, the small space still permeable being above the upper part.

The more remote cause of the stricture, however, would appear to have

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[1] An elastic tube about a quarter of an inch in diameter, and eighteen inches long, with an ivory or ebony tip at each extremity—one end being placed in the external meatus of the patient, the other is inserted in that of the surgeon, and by this means sounds emanating from the tympanic cavity of the patient can be distinctly heard.

been an enlargement of portions of the bone constituting the external and internal osseous walls of the tube. The external osseous wall was at this part twice its natural thickness, and somewhat rough; while the internal wall was forced outwards by the dilatation of the carotid canal, which thus pressing upon the cartilaginous portion of the Eustachian tube with which it was in contact produced a flattening of the natural concavity of the internal wall. The mucous membrane lining the Eustachian tube was in a natural state.

Observations.—It is important to consider the effect of an impervious stricture of the Eustachian tube on other parts of the organ of hearing, and to understand in what way the functions of that organ are thereby impaired, and the degree of restoration of which they are susceptible.

The existence of this stricture would have been detected during life had the Eustachian catheter and otoscope been resorted to, but the great debility of the patient made me desirous not to do any thing which was liable to increase his discomfort. Strong presumptive evidence, however, of the obstructed state of the Eustachian tube existed in the peculiar condition of the membrana tympani, as described above; for, with the exception of cases of accumulation of mucus in the tympanic cavity, arising from a thickening of the lining membrane (and these cases have other distinguishing symptoms), I do not know of any disease which produces the peculiar whiteness of the membrana tympani.

It is unquestionable that the stricture, in the present instance, prevented the egress of mucus from, and the ingress of air into, the tympanic cavity—the only effect of a contracted Eustachian tube for which medical aid would probably be sought. The *first* effect of a stricture appears to be a gradual accumulation of the mucus secreted in the tympanic cavity and mastoid cells; the *second*, a thickening of the lining mucous membrane; and the *third*, chronic inflammation of it.

The presence of mucus in the cavity of the tympanum, in cases of stricture of the Eustachian tube, is an important symptom; because, when the membrana tympani is perforated for a supposed obstruction of the tube, were the mucus present it must invariably be detected. Now, I believe that in none of the recorded cases, where the operation of piercing the drum of the ear has been successfully resorted to, is there any mention of the presence of mucus in the cavity. In these instances the disease, instead of being seated in the Eustachian tubes, consisted, I believe, in a peculiar condition of the membrana tympani itself, of which I will defer speaking until a future occasion.

In those cases of permanent stricture of the Eustachian tube, where the membrana tympani is not perforated as the result of ulceration, the latter is subject to chronic inflammation, becomes thick and firm, and presents an impassable barrier to the exit of the mucus by the external meatus. The treatment of these cases consists in making an orifice in the membrana tympani, to allow of the

exit of the mucus, and provide for its continuous discharge.[1] The duty of the surgeon, indeed, is here a simple imitation of the mode of relief to which nature resorts, when she establishes a fistulous opening in the membrana tympani, for the discharge of mucus from the tympanic cavity, after inflammation of the mucous membrane of the tympanum, as in scarlet fever, where a very large quantity of thick mucus is secreted, and obstructs the Eustachian tube.

Judging from the history of the case above cited, and from the condition of the ear, it is highly probable that the stricture of the Eustachian tube was produced very slowly; it is, however, interesting to observe, that the stricture had not implicated to any serious extent the other parts of the organ. The mucous membrane of the tympanic cavity, although thicker than natural, would not have materially impeded the passage of sonorous undulations to the cochlea and vestibule. The stapes did not appear to be fixed in the fenestra ovalis more firmly than natural; the other ossicles were not diseased; and it is most probable that the diminution in the power of hearing was produced by the presence of mucus in the tympanic cavity, which prevented the membrana tympani, ossicula, and membrane of the fenestra rotunda, from performing their functions. Had the membrana tympani been punctured during life, or had a fistulous orifice been effected in it by natural means, the mucus would have escaped from the tympanic cavity into the external meatus; and there is every probability that the functions of the organ would have been so much restored as to have enabled the patient to have heard ordinary conversation with facility.

ARTICLE II.—*Results of the Treatment of Fever in Glasgow Hospitals and in Out-Door Practice contrasted.* By JAMES ADAMS, M.D., Fellow of the Faculty of Physicians and Surgeons, Glasgow.

I HAVE often had occasion to observe, that a very marked contrast exists between the mortality occurring in large hospitals in certain diseases, and that of the like diseases when treated at the homes of the patients; and this I have noticed to be especially the case in regard to the treatment of fever,—the mortality in hospitals far exceeding that of out-door practice. From the constant presence of fever in Glasgow—the frequent recurrence and increasing severity of wide-spread epidemics of the disease—the important truth that it prevails most extensively, and proves most fatal, at those ages when

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[1] On account of the great difficulty attendant upon keeping open any incised aperture in the membrana tympani, I have resorted, with entire success, to the practice of producing an opening by the application of an escharotic, chloride of zinc being the one used.

the life and health of individuals are of most value to themselves and to the community—the startling fact, indicated by the bills of mortality, that one-seventh of the whole deaths in Glasgow are caused by fever—and the circumstance that large numbers of fever patients are annually treated in the Glasgow hospitals;—for these cogent reasons, I have thought it an inquiry of no mean importance to ascertain if this excessive mortality is invariable, or only occasional—if dependent upon local or appreciable causes—if capable of being removed—and, in short, to arrive at more exact notions of the value of hospital treatment in fever, as far as regards the safety of individual sick persons, and the protection of the general community.

This inquiry cannot be solved by authoritative opinions; for in these there exists the greatest and most irreconcileable enemy to truth and argument. An inquiring mind is not now satisfied with knowing an opinion: it must inspect the materials from which that opinion has been elaborated, and the machinery by which it is constructed, before deciding upon its merits.

In following out this investigation, I have, therefore, availed myself of such sources of information as have enabled me to bring together a large mass of *comparable* facts regarding the experience of various hospitals and other institutions in the treatment of fever, and especially, among these, of the valuable series of medical statistics and case books, which are preserved in the office of the City Parish Parochial Board, and which contain, for a number of years, the results of out-door practice among the poor, as observed by the city district surgeons.[1] I purpose to adduce so much of these facts as may seem necessary to elucidate without encumbering the subject; for, however valuable the conclusions arrived at by statistical investigation may be, they lose much of that value when the precise data on which they have been founded are withheld.

Although my observations relate chiefly to Glasgow, yet a short reference to the experience of other localities will show, that the question is one of wide connections and of general interest.

In Limerick Fever Hospital, and in the Dispensary out-door practice,[2] the number of fever cases, with the results for the five years 1827-31, were—

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[1] For authorised access to these materials, I am indebted to the ready courtesy of D. Anderson, Esq., chairman of the board. I may here mention, that the office of district surgeon is a public office, for the obtaining of which there has never been a lack of competition. Among the staff there have always been medical officers to public institutions, lecturers, and others of respectable status, and many have subsequently filled the offices of physician or surgeon to the Royal Infirmary. Among the names of former district surgeons which occur readiest to my recollection are Drs John Macfarlane, Andrew Buchanan, Granville S. Pattison, William Young, Auchincloss, William Weir, J. Stirling, J. A. Easton, J. G. Fleming, J. Watt, &c. &c.

[2] Dr W. J. Geary, in "Dublin Journal of Medical Science," 1837, p. 385

Hospital.	*Dispensary.*
Cases, 5962.	Cases, 6130.
Deaths, 296,—or 1 death in 20 cases.	Deaths, 176,—or 1 death in 34 cases.

Here the deaths in hospital are seen to be nearly double those in the dispensary or out-door practice.

In coming nearer home, the contrast is no less strikingly marked. The number of fever cases treated in the Edinburgh Royal Infirmary,[1] during the three years 1840-41-42, were 2248, and the deaths 302—showing a mortality of 1 death in 7 cases. In the practice of the Edinburgh New Town Dispensary,[2] during the same years, there were treated 764 cases of fever, of which number 48 died—showing a mortality of 1 death in 14 cases. The contrast is here rendered more evident from the circumstance, that in the Infirmary cases there are included febricula, or mild cases of fever, while in the Dispensary report typhus cases only are given.

Further examples from other localities might be adduced, but are unnecessary.

The following table is of great interest, as it shows the total number of fever cases *treated* by the Glasgow district surgeons during eleven consecutive years, and also the total number treated in the Glasgow Royal Infirmary[3] during the same period.

Year.	District Surgeons.			Glasgow Infirmary.		
	Cases.	Died.	Mort. per cent.	Cases.	Died.	Mort. per cent.
1838.....	908	67	7·3	2,047	231	11·2
1839.....	255	16	6·3	1,529	253	16·5
1840.....	750	44	5·8	3,385	448	13·2
1841.....	1,221	59	5·2	2,578	265	10·2
1842.....	576	22	3·8	1,194	125	10·4
1843.....	14,584	413	2·8	3,553	179	5·0
1844.....	1,574	73	4·6	1,468	143	9·7
1845.....	352	18	5·1	538	75	14·0
1846.....	1,346	65	4·9	1,800	207	11·5
1847.....	7,038	484	6·9	5,224	804	15·4
1848.....	1,337	93	7·2	1,636	250	15·3
Total	29,941	1,354	4·5	24,952	2,980	11·9

It will at once be observed, that at no date has the mortality in out-door practice approximated to that in hospital, while the gross mortality for the whole period gives, in out-door practice, 1 death in 22 cases; and in hospital, 1 death in 8 cases.

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[1] Annual Statistical Reports.
[2] Edinburgh Monthly Journal of Medical Science, vol. iii., pp. 659 *et seq.*
[3] Annual Reports.

A disproportion so great and so invariable could not escape notice or fail to excite comment, and I find, accordingly, that some of our Glasgow hospital physicians, and also of our district surgeons, have made public allusions to the subject. Thus Dr Moses Buchanan[1] says, that "the difference may be accounted for, in part, by the more severe cases being transmitted to the hospital in an advanced stage of the disease." Dr Orr,[2] alluding to the gross mortality of the hospital in 1846, says that "the severe nature of the cases treated" explains, in part, the increased mortality over the former year; and that another circumstance in explanation is "the fact, that during the year the numbers sent both to this and the *fever department* of the hospital in a dying state were unusually large. The practice of removing patients in such a condition from their own dwelling-houses deserves to be severely censured. This practice, I believe, prevails to a greater or less extent in the experience of most hospitals."

I deem it unnecessary to multiply additional opinions, as the statements quoted convey the substance of all that I can find advanced by hospital physicians in explanation of the increased mortality of fever in hospitals. It does not appear, however, that these opinions are based upon positive facts or actual inquiry, or that they are other than mere assumptions, founded upon individual impressions. And, as very decided counter-statements—to some of which I will afterwards allude—are advanced on the part of the district surgeons, it is evident, as before stated, that no solution of the question is to be obtained from such sources of proof.

The chief conditions which seem to me likely to affect the mortality in fever, and which, therefore, require to be considered before any deductions can safely be drawn, are,—1st, the type of fever; 2d, the sex and ages of the patients; and 3d, the duration of illness prior to coming under treatment. These points I will take up and discuss in their several orders, making such comments upon the nature of the evidence as may be suggested by a common-sense view of the subject, and the impressions derived from my own personal experience as a district surgeon,—discriminating what seems essential, and what may be attributed to modifying influences.

And first, with regard to the type of the fever cases treated in hospital and in out-door practice. Various terms have been used to designate the varieties of fever which are found to prevail in Glasgow. Febricula, simple fever, continued fever, bilious, relapsing, gastro-hepatic, dothinenteric, typhus, &c. &c., are the expressions commonly employed, varying according to the peculiar views of the observers, the prevailing type, or the predominating symptoms. In comparing, as in the present inquiry, an extended series of observa-

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[1] History of Glasgow Royal Infirmary.

[2] Statistics of Glasgow Royal Infirmary for 1846: Edin. Med. and Surg. Jour., No. 171.

tions, it is not necessary to attempt equal minuteness of detail; and I purpose, in my subsequent observations, to include all cases of fever under two names—*i. e.*, continued fever, and typhus. The term *typhus*, or *exanthematous typhus*, is in this city applied by common usage to that form of malignant fever always present in Glasgow, and which, among other peculiarities, is characterised, with few exceptions, by a measly rash appearing between the fourth and tenth days, and also by the circumstance, that the mortality greatly exceeds that of the other forms of fever, taken collectedly. The other common varieties of fever may conveniently, and with sufficient accuracy, be designated continued fever.

From the very excellent pathological statistics of the Glasgow Royal Infirmary,[1] the type of fever treated in that institution can be distinguished for the five years ending 1848. I find that during this period there have been treated in hospital, 10,666 cases of fever,—of which number 4856 were cases of typhus, and 5810 were cases of continued fever. This gives a proportion, in 100 cases, of 45·5 typhus, and 54·5 continued fever.

During the same five years there have been treated by the city district surgeons, 11,647 cases of fever,—of which number 7776 are reported as typhus, and 3871 as cases of continued fever. This gives a proportion, in 100 cases, of 66·8 typhus, and 33·2 continued fever.

There can be no question that the facilities for an accurate diagnosis of the type of fever are much greater in hospital than in out-door practice, where the diagnosis has often to be sought for amid darkness, dirt, and many discomforts. I am, therefore, disposed to think that allowance must be made for this circumstance, and that a considerable proportion of the cases termed typhus in the books of the district surgeon had been, in fact, only severe cases of continued fever. Still, after making every reasonable allowance for such a source of fallacy, *it is still abundantly evident that the number of cases of typhus, or the malignant form of fever treated in hospital, does not exceed the number treated in out-door practice.*

The proportional number treated of males and females requires to be next considered, as this has an important influence upon the gross mortality,—the male sex invariably furnishing the largest number of deaths.

The total numbers of each sex treated in the Royal Infirmary and by the district surgeons, during the eleven years 1838-48, were,—

Total Cases.			*Ratio per* 100 *Cases.*		
	Males.	Females.		Males.	Females.
Royal Infirmary, ...	12,697	12,255	Royal Infirmary, ...	50·8	49·2
District Surgeons,...	13,460	16,481	District Surgeons,...	44·9	55·1

[1] Dr S. Orr, from 1844 to 1847,—afterwards continued by Dr C. Steele.

The difference which is here seen to have existed in the relative proportion of the sexes, had undoubtedly an effect upon the total mortality; and, if sex alone were considered, the mortality in district practice would have been increased by a few deaths, in having had the same proportion of males and females as in hospital. But, as the ages of fever patients have a much greater influence upon the results, no comparison can be satisfactory, unless sex and age are taken in conjunction.

The tables published since 1838, by Drs Cowan, Orr, and Steele, of the ages, &c., of fever patients treated in the Glasgow Infirmary, give, collectively, an analysis of upwards of 16,000 cases,—a number sufficiently great to obviate the risk of any serious error. From the data furnished by this large number of cases, I have calculated, in similar proportion, the total cases treated in the Infirmary for the eleven years 1838 to 1848. The *actual number of cases and of deaths* for each sex are taken from the annual reports already indicated. I have also calculated in this table the proportion for each period of life which would be found in 100 cases, together with the corresponding mortality per cent.

Table of Age, Sex, &c., of Fever Patients treated in Glasgow Royal Infirmary.

Ages.	Cases.			Deaths.			Ratio per 100 Cases.			Mortality per Cent.		
	Total.	M.	F.	Total	M.	F.	Total	M.	F.	Total	M.	F.
Under 5 years,	148	65	83	14	8	6	0·6	0·5	0·6	9·6	12·6	7·2
5 to 10 „	998	592	406	36	15	21	4·0	4·6	3·3	3·6	2·5	5·1
10 to 20 „	7,568	3,495	4,073	414	191	223	30·3	27·4	33·2	5·4	5·5	5·5
20 to 30 „	8,538	4,464	4,074	939	581	368	34·2	35·1	33·2	10·9	12·8	9·0
30 to 40 „	4,090	2,171	1,919	660	420	240	16·4	17·1	15·7	16·1	19·3	13·0
40 to 50 „	2,304	1,256	1,048	544	340	204	8·1	9·9	8·5	23·6	27·0	19·4
50 to 60 „	834	407	427	228	127	101	3·3	3·2	3·5	27·3	31·2	23·6
Above 60 „	400	207	193	123	68	55	1·6	1·6	1·5	30·7	32·8	28·5
Not known,	72	40	32	22	13	9	0·3	0·3	0·3	30·1	32·5	27·2
Total, ...	24,952	12,697	12,255	2980	1753	1227	100·	100·	100·	11·9	13·8	10·0

From a very laborious examination of the daily registers or case-books of the district surgeons, I have collected the necessary particulars of nearly 17,000 cases,—from which data I have prepared a table, in correspondence with the foregoing, showing an analysis of all the cases of fever treated by the district surgeons during the eleven years already specified. These tables are in themselves interesting, as exhibiting on a large scale the nature of the cases occurring in out-door and in hospital practice, and they further furnish materials absolutely necessary to the elucidation of this inquiry.

Table of the Age, Sex, &c., of Fever Patients treated by the District Surgeons.

Ages.	Cases.			Deaths.			Ratio per 100 Cases.			Mortality per Cent.		
	Total.	M.	F.	Total	M.	F.	Total	M.	F.	Total	M.	F.
Under 5 years,	3,592	1,956	1,636	130	89	41	11·9	14·5	9·9	3·6	4·5	2·5
5 to 10 „	2,800	1,332	1,468	86	39	47	9·3	9·8	8·9	3·0	2·9	3·2
10 to 20 „	7,497	3,517	3,980	149	57	92	25·0	26·1	24·1	2·0	1·6	2·3
20 to 30 „	5,479	2,210	3,269	183	86	97	18·3	16·4	19·8	3·3	3·9	2·9
30 to 40 „	4,407	1,897	2,510	225	123	102	14·7	14 0	15·2	5·1	6·5	4·6
40 to 50 „	3,318	1,438	1,880	227	109	118	11·0	10·7	11·4	6·8	7·6	6·3
50 to 60 „	1,569	679	890	140	60	80	5·2	5 0	5·4	8·9	8 8	8·9
Above 60 „	1,279	431	848	214	91	123	4·2	3·2	5·1	16·7	21·1	14·5
Total, ...	29,941	13,460	16,481	1354	654	700	100·	100·	100·	4·5	4·9	4·2

On contrasting the two foregoing tables, the difference which exists in the relative proportion of cases occurring at decennial periods of life is seen to be very great. During the most valuable periods of life—viz., from 10 to 40 years—the cases which occur in hospital comprise more than three-fourths of the whole admissions, and the deaths for the same periods form a proportion not much smaller.

In out-door practice, on the contrary, the cases and deaths occurring at the same periods of life are little more than one-half of the whole number. These facts are important, and must not be lost sight of in estimating the advantages or disadvantages of fever hospitals.

The mortality occurring at different periods of life, both in hospital and in out-door practice, is also seen to vary much. In both there is a great proportionate mortality under 5 years of age. Between 5 and 10 years the deaths are much diminished. After 10 years the mortality steadily increases both in hospital and in out-door cases; the degrees of intensity being greatest in hospital, and increasing in a more accelerated ratio.

With regard to the number of cases treated at each period of life, it is shown that in out-door practice there occurs a much larger proportion of cases in early life, and in advanced age,—at which periods the mortality is very considerable; and it is evident that this disproportion must affect very materially the gross mortality. I find accordingly, on making the necessary calculations for correcting such a disproportion, that if the 29,941 cases treated in out-door practice had consisted of the same *proportion* of males and females, at each period of life, with that of the cases treated in hospital, there would have occurred only 1209 deaths, or 45 deaths *less* than actually did occur. On applying the same test to the hospital cases, I find that had the 24,952 cases treated in hospital consisted of the

same proportion for each age with the out-door cases, the deaths in hospital would have been 3086, or 106 deaths more than actually did occur.

It is, therefore, abundantly evident that the greater mortality of fever in hospitals over out-door practice *is not owing to an over-proportion of cases of that sex, and at those periods of life, in which fever proves most fatal.*

It may be urged that my observations have too exclusive reference to the Royal Infirmary, and that this consideration should limit the deductions which I have drawn. To avoid such an objection, I have made special investigation into the results furnished during the last epidemic of fever, to which I will now refer somewhat in detail. This epidemic, which commenced in July 1846, steadily increased during the spring and summer of 1847, the utmost accommodation afforded by the Royal Infirmary proving very insufficient to meet the demands made upon it. The parochial boards of the city and barony parishes were then driven to the necessity of providing special hospital provision for the requirements of their sick poor, and, within a few weeks, the old Town's Hospital, at Clyde Street, was fitted up with a large number of beds (ultimately exceeding 600), for the accommodation of the city poor; and for the poor of the barony parish temporary wooden erections were prepared at Anderston, for the reception of about 250 patients,—the parochial managers displaying on this occasion an amount of energy and of disinterested exertion which has been ill understood and little appreciated by the general public.[1] Clyde Street Hospital was opened in the beginning of July 1847, and from this date until the decline of the epidemic in June 1848, there were treated in it 5404 cases of fever. In the Anderston Hospital, which was opened August 1847, there were treated in the eleven months ending June 1848, 2620 cases of fever. Both Clyde Street and Anderston Hospitals were filled with fever patients, within a few days of their being opened, thereby affording immediate relief to the over-crowded wards of the Infirmary.

Here, therefore, we have the fairest elements for comparison. Three hospitals open at the same period for the treatment of fever patients—viz., the Infirmary, Clyde Street, and Anderston Hospitals. In the two latter the patients treated were exclusively the very poorest of the population; but I am satisfied that no real distinction of class existed between these and the infirmary patients. The cases treated by the district surgeons were essentially identical with

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[1] No account has previously been given of the nature and results of the cases treated in Clyde Street Hospital. A very excellent report of the operations carried on in the Anderston Hospital was prepared by Dr Paterson, the house-surgeon and superintendent,—an abstract of which was published in the Edin. Med. and Surg. Jour. for 1848. I am indebted to Dr Paterson for much necessary information regarding this hospital.

those treated in the parochial hospitals. There is, therefore, to be compared the same class of poor, the same epidemic fever, and the same period of time. The following shows the numbers treated, and the results :—

	Total number treated.	Total Deaths.	Mort. per Cent.		
Royal Infirmary	3531	... 579	16·4, or 1 death in	$6\frac{67}{579}$	cases.
Anderston Hospital,	2620	... 303	11·5 „	$8\frac{196}{303}$	„
Clyde Street Hospital,	5404	... 799	14·8 „	$6\frac{610}{799}$	„
City District Surgeons,	4852	... 306	6·3 „	$15\frac{262}{306}$	„

The proportionate number of males and females treated in the infirmary at each decennial period of life differed little from the general table, so far as I can judge from the tables prepared by Dr Steele for 1847, and one prepared by myself for 1848. In the cases treated by the district surgeons, the only difference from the general table consisted in a larger proportion of cases under 5 years, and in a greater proportion of males,—the number of both sexes being nearly alike. I consider it, therefore, unnecessary to bring forward tables of the infirmary and out-door cases for this period. The following table shows the age, sex, and results, of the cases treated in the Clyde Street and Anderston Hospitals :—

Table of Fever Cases treated in Clyde Street and Anderston Hospitals.

Ages.	Clyde Street Hospital.						Anderston Hospital.					
	Cases.		Deaths.		Ratio per 100 Cases.	Mort. per Cent.	Cases.		Deaths.		Ratio per 100 Cases.	Mort. per Cent.
	M.	F.	M.	F.			M.	F.	M.	F.		
Under 5 yrs.,	47	43	9	8	1·6	18·8	10	4	1	...	0·5	7·1
5 to 10 „	200	191	21	21	7·2	10·7	115	117	5	4	8·8	3·9
10 to 20 „	709	828	54	63	28·4	7·6	398	434	27	18	31·7	5·4
20 to 30 „	861	809	136	99	30·9	14·0	367	290	55	23	25·1	11·9
30 to 40 „	480	408	95	51	16·4	16·4	208	210	31	19	15·9	11.9
40 to 50 „	283	268	79	63	10·2	25·8	176	137	44	24	11·9	21·7
50 to 60 „	111	84	39	29	3·6	34·9	66	48	22	14	4·4	31·6
60 and upwds	42	40	18	14	1·5	39·0	15	19	7	8	1·3	44.1
Not known,	...	...	...	...	...	...	3	3	1	...	0·2	16·6
Total, ...	2733	2671	451	348	100·	14·8	1358	1262	193	110	100·	11·5

The proportion of cases treated at each age in Clyde Street Hospital is seen to approximate very closely to that of the Anderston Hospital, and the differences which do exist have very little influence upon the gross mortality. A considerable contrast is, however, observed in the general mortality; but this depends on other causes than the age and sex of the patients. I will afterwards allude to this discrepancy, and indicate some of the causes which affected unfavourably the mortality in the Infirmary and Clyde Street Hospitals.

In other respects there is nothing in these tables which seems to claim special notice, or to alter the deductions which may be drawn from those already given.

The next division of my inquiry relates to the period of disease at which cases of fever are brought under treatment.

The following table shows the period of admission and the results in 9477 cases of fever treated in hospital during 1847-8,—viz., all the cases treated in the parochial hospitals as already specified, and as large a proportion of the cases treated in the infirmary during 1848 as I could procure information regarding;—also of 3006 cases of fever treated by the district surgeons during the intensity of epidemic fever:—

No. of days ill on admission.	Infirmary during 1848.		Clyde Street Hospital.		Anderston Hospital.		Tot. Hospital cases.		District Surgeons.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1 day	...	...	6	2	30	3	36	5	20	...
2 days	18	...	101	11	45	...	164	11	158	6
3 „	49	6	336	32	128	8	513	46	160	8
4 „	141	15	787	101	289	23	1217	139	574	32
5 „	191	20	565	68	310	25	1066	113	270	20
6 „	187	21	496	70	324	38	1007	129	358	14
7 „	184	23	249	35	251	30	684	88	170	11
8 „	230	43	551	83	340	39	1121	165	486	55
9 „	95	12	77	14	173	18	345	44	80	5
10 „	91	21	150	23	183	19	424	63	170	23
11 „	35	5	10	2	62	8	107	15	30	3
12 „	36	4	81	13	67	11	184	28	63	5
Above 12	56	5	201	31	178	26	435	62	363	23
Not known	140	39	1794	314	240	55	2174	408	104	12
Total.....	1453	214	5404	799	2620	303	9477	1316	3006	217

This table shows, in a very evident point of view, that *the mortality from fever, both in hospital and in out-door practice, is lessened when the cases are brought under treatment at an early period of the disease*, and, consequently, is a strong testimony to the efficacy of medical aid. But it farther shows that *the greatest proportion of cases are brought under treatment at as early a period as could be reasonably anticipated.* Both of these circumstances will be rendered more apparent from the subjoined table, which is a summary of the preceding, with the addition of columns which exhibit the proportion of cases admitted within each of the four periods into which the table is divided, together with the corresponding rate of mortality; those cases in which the previous duration of illness is unknown, being for obvious reasons omitted.

Duration of illness prior to admission.	Hospital Cases.				Out-door Cases.			
	Cases.	Deaths.	Ratio per 100 cases.	Mort. per cent.	Cases.	Deaths.	Ratio per 100 cases.	Mort. per cent.
1 to 4 days	1930	201	26·4	10·4	912	46	31·4	5·0
5 to 8 „	3878	495	53·1	12·8	1284	100	44·2	7·7
9 to 12 „	1060	150	14·5	14·1	343	36	11·8	10·5
Above 12 „	435	62	5·9	14·3	363	23	12·5	6·3
Total.....	7303	908	100	12·4	2902	205	100	7·0

In the ratio of admission, no very great discrepancy is observed between hospital and out-door cases, the latter having a slight advantage in the very early cases, counterbalanced, however, by the larger proportion of cases at an advanced period of the disease. In both kinds of practice there are fully one-fourth of the cases brought under treatment within four days of the commencement of illness, and three-fourths of the whole within the eighth day of illness. In both, the mortality is lowest among the cases seen from the early periods of the disease, and in both it continues afterwards to increase, —always, however, preserving this important distinction, *i. e.*, a very marked disproportion in the rate at which the mortality advances; that of hospital being so high from the very commencement, that, *although all the cases treated there had been received within the first few days of the attack, the gross mortality would not have been greatly diminished.* There is also a farther contrast in the circumstance, that in out-door practice the mortality diminishes in cases seen after the twelfth day of the fever. Nor does the disadvantage experienced in hospital, of having a smaller proportion of the early cases, make any notable difference in the mortality; for, if the cases admitted into hospital at the four periods shown in the table had been in the same proportion with the cases treated in out-door practice, I find, on making such a calculation, that the deaths in hospital would still have been 900, or only 8 deaths less than actually did occur.

And here I cannot forbear digressing to express my surprise that the cases admitted into hospital at an early period of the disease are so numerous, considering the obstructions that interfere with their prompt and ready admission. I do not allude to the unavoidable loss of time in sending the fever conveyance for the patient, but to the delays which occur in consequence of the patient's friends, and not unfrequently the surgeon, being under the necessity of hunting after employers, subscribers, or others, the patrons of the Infirmary,

to procure an order for the admission of the patient, or to get the medical certificate countersigned. This faulty state of matters applies chiefly to the permanent hospital, the Infirmary; in the temporary hospitals, during the pressure of the epidemic, prompt admission was given upon the mere certificate of the district surgeon.

Another cause exists by which the number of late admissions is increased, *i. e.*, the type of fever. Typhus is often insidious in its approach, and in some cases it is difficult for the first few days to determine that the case is one of fever; while several days invariably elapse ere its peculiar nature can be recognised with certainty. It becomes, therefore, a matter of some consequence to ascertain the period at which typhus cases are received into hospital, and how far the mortality is thereby affected. The following table shows the number of cases, and the ratio of admission and of mortality of typhus cases included in the table (p. 109), and treated in the Royal Infirmary and Anderston Fever Hospital—the cases in Clyde Street Hospital being omitted, as in it the type of fever was frequently not stated:—

Table of Typhus Fever Cases treated in Hospital.

Ill prior to admission.	Cases.	Deaths.	Ratio of admission.	Mortality per cent.
Under 4 days ill.....	238	45	11·4	18·9
5 to 8 „	1159	189	55·7	16·3
9 to 12 „	544	94	26·1	17·3
Above 12 „	140	25	6·7	17·8
Total known.....	2081	353	100	16·9
Length of illness unknown.....	247	64	...	25·9
Total.....	2328	417	...	17·9

The results which this table exhibits are at variance with the general table including both kinds of fever, and are to me somewhat surprising, not as regards the large proportion of cases received between the fourth and twelfth days, for this might have been anticipated, but as regards the fact, that cases of typhus fever, sent into hospital within the first few days of the attack, should die in larger proportion than cases sent at a more advanced period of the disease. Several returns which I have procured from other places, exhibiting the results of a limited number of cases, are also confirmatory of this fact;—whatever explanation may be given of the circumstance, this inference remains clear, that the gross mortality from fever in hospital is not increased over that of out-door practice, neither by an excessive proportion of typhus cases received at an advanced period of the disease, nor by a disadvantageous proportion in the general ratio of admissions of all the cases of fever treated in hospital.

But it has been sometimes stated that the district surgeons were in the practice of sending cases of fever to the hospital so soon as such cases commenced to put on unfavourable symptoms, and that in this way they were more likely to have selected cases under treatment. Before referring to the special testimony on this point, which has been given by various district surgeons, I have thought it better to analyse a large number of cases, to show the interval which occurs from the time of fever patients being first visited by the district surgeon to that of the case being sent into hospital. The following table, containing the experience of 1401 cases, has been taken at random from the books of seven districts:—

No. of Days ill when first visited.	Total cases.	Sent to Hospital after an interval of							
		A few Hours	1 Day.	2 Days.	3 Days.	4 Days.	5 Days.	6 Days.	Above 6 days.
1 day	33	2	2	2	14	4	4	2	3
2 days	130	32	44	14	12	12	8	6	2
3 „	174	52	44	38	22	10	6	1	1
4 „	290	134	82	32	22	10	6	3	1
5 „	186	66	68	26	12	6	2	4	2
6 „	190	74	60	32	10	10	2	2	...
7 „	80	30	26	16	6	2	...	...	...
8 „	194	78	58	28	6	6	8	6	4
9 „	18	4	8	6	...	...	...	...	...
10 „	38	12	20	...	...	...	...	4	2
11 „	6	2	4	...	...	...	...	...	...
12 „	10	8	...	...	2	...	...	...	...
Summary.									
1 & under 4 dys.	627	220	172	86	70	36	24	12	7
5 — 8 „	650	248	212	102	34	24	12	12	6
9 — 12 „	72	26	32	6	2	...	...	4	2
Above 12 „	20	6	2	8	...	2	...	1	1
Not known.....	32	4	14	12	...	...	2	...	...
Total	1401	504	432	214	106	62	38	29	16

Here there is very decided proof that no unnecessary delay takes place on the part of the district surgeons in sending their cases into hospital, for fully *one-third* of the whole are sent within a few hours of their being first visited, and not more than a tenth part are removed after the third day from being first seen. Moreover, in a great proportion of those cases which are sent at a late period of the disease, the district surgeons seem anxious to prevent any impression being entertained that the removal had taken place at their instigation, for they append notes to their daily case-books to the effect, that the patient has been "sent by his employer," or "by his friends," or "by the landlord of the property," or "because there is no attendance," &c.

So far, therefore, as figures can elucidate the subject, there is no

foundation for the statement in question; and indeed it must be evident that the district surgeons had been gifted with a nicer tact or intuitive sagacity than they are likely to claim for themselves if they could, at the commencement of a case of fever, or within the short period spoken of, discriminate the worst cases and weed them out to be sent off to hospital, even supposing their impressions and their feelings of professional responsibility sanctioned such a line of conduct. As this point is, however, one of much importance, I think it necessary to cite additional evidence, and of such a nature as to supersede the need of argument.

Dr William Weir, presently one of the physicians to the Royal Infirmary, and whose experience as a district surgeon was most extensive, says, in reference to this question, "This I know has not been the case for some years, but rather the contrary; and I have been so impressed with the fact, that removing patients after the fever has advanced, and the low typhoid symptoms are present, is attended with bad consequences, that I seldom recommend any to the hospital but the mildest; and in this, I believe, I am joined by the majority of the town surgeons."[1] In the 5th vol. of the Glasgow Medical Journal, the editor gives the assertion in question "a most positive contradiction;" expresses surprise that it should have been made, and states, "it was almost uniformly the mildest cases of that disease that were sent to hospital; and when severe cases were sent it was contrary to the desire of the district surgeon, who knew well that the removal of a patient lying dangerously ill of fever was uniformly attended with a very serious increase of symptoms, and not unfrequently with the conversion of a mild case into a very serious one." Dr J. A. Easton, presently one of the physicians to the Royal Infirmary, and also of great experience as a district surgeon, complains that "such heartless conduct" should be imputed to the district surgeons, "as that of sending only the worst cases of fever to the Infirmary, converting thereby this noble charity into a sort of receiving-house for the unclaimed dead;" and he gives "the gratuitous aspersion an unqualified denial."[2]

The convictions of my own experience as a district surgeon, which has also been considerable, induce me to add my assent to the foregoing testimony. I am well satisfied that the majority of the district surgeons have not at any time been so ignorant or so unreflecting as to subject their worst cases of fever to the risks generally associated with the idea of removal to hospital, nor to dally unnecessarily with such cases as were removed with their sanction.

A very interesting and useful subject of inquiry now remains open —*i. e.*, to trace the causes which imperil the life of an hospital fever patient, and so frustrate the efforts of active benevolence, and the

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[1] Glasg. Med. Jour., vol. i. p. 222.

[2] Glasg. Med. Jour., vol. vi. p. 286.

objects sought to be attained by a lavish expenditure of money. This inquiry does not, however, form a necessary part of my present task, and I will, therefore, limit my remarks on this point to a short consideration of a few circumstances which contribute to the result.

In the first place, there seems no good reason for doubting the numerous facts collected by various recent parliamentary commissions, and which show that the aggregation in one building of a large number of human beings in a state of disease re-acts with deleterious influence, and lowers the general tone of vitality of the whole inmates. When the wards or sleeping apartments become crowded, the effects are very apparent, and a public institution so circumstanced has been aptly compared to "a town without party walls in a fire." Illustrations might readily be adduced if space permitted. I may, however, allude to the great but varied mortality which occurred in the Glasgow hospitals during the late epidemic fever, as connected with the subject of over-crowding. During the intensity of the epidemic, and up till July 1847, at which date the Glasgow parochial directors had provided accommodation for their sick poor, the Royal Infirmary was subjected to a pressure which, under the existing circumstances, could not be resisted, and every available space in the fever buildings was "filled" with fever patients. In January 1847 the average number of fever cases in the Infirmary was 259,[1] and this large average increased steadily until in July it reached the *maximum* of 369 cases. With the opening of Clyde Street hospital, the average number in the Infirmary fell immediately to 278, and continued thereafter to decline in a steady ratio. Simultaneously with a declension in the average number of cases, there also occurred a fall in the average mortality; for the deaths which, in January, had averaged 12 *per cent.*, and had increased *pari passu* with the increasing average numbers of the inmates, until in July the mortality reached nearly 18 per cent., now underwent a decline. In August the average mortality fell to 14 per cent.; in September to 12 per cent.; and in October to 11 per cent. In commenting upon the mortality in the Infirmary for 1847, Dr Steele remarks,[2] "It is worthy of notice in this place, though, perhaps, to the prejudice of our fever hospital, that the mortality returns of the fever cases treated throughout the city, both in the parochial hospitals and by the district surgeons, averaged much less in proportion than those of the Infirmary, and it scarcely admits of question that the accommodation afforded by the regular hospital is infinitely superior to that outside. These facts tend to favour the supposition, that the miasmata engendered by long concentration in the wards of an hospital has a decidedly prejudicial effect on the disease itself."

In Clyde Street hospital the mortality was less than in the Infirmary. Clyde Street hospital is an old low-roofed building, at one

[1] Annual Reports. [2] Edin. Med. and Surg. Jour., 1848, p. 106, *et seq.*

time the city poor-house. Some temporary additions to its accommodation were made soon after its being opened, and the extreme accommodation, when completely filled, consisted ultimately of 634 beds. Such, however, was the extreme pressure of the epidemic that 720 patients were admitted within the first two or three weeks. In August not less than 992 new cases were admitted, and the average admissions from July to November, inclusive, were upwards of 800 new cases per month, while the average number constantly in hospital during the same period exceeded 600. I have no doubt that the large mortality which did occur would have been still greater had the building been previously occupied as a regular fever hospital, and its wards, consequently, affected by "the miasmata engendered by long concentration." I am also of opinion that the lesser mortality in Clyde Street hospital, as compared with the Infirmary, is to be attributed in part to the circumstance of its patients having to be conveyed a comparatively short distance.

The lowest hospital mortality was experienced in the Anderston hospital. This building consisted of temporary wooden sheds, with slated roofs, constructed for the occasion. The wards were, consequently, and *unavoidably*, well permeated with currents of air; and the hospital not having been exposed to the same pressure which overflowed the Infirmary and Clyde Street hospitals, the wards were never in a state that could be called crowded. Mr Hannay, chairman of the committee which executed all the arrangements connected with this hospital, has informed me of an incident worthy of being noticed. At one period, on account of soot and rain finding a ready ingress through the interstices of the slated roof, it was considered advisable to form a temporary ceiling by pasting together in close layers large sheets of brown paper between the rafters, and this expedient was accordingly adopted in one section of the building. But coincident with this alteration in the structure of the wards, there occurred such a marked increase in the malignancy of the symptoms and in the number of deaths, that the temporary paper ceiling was destroyed without hesitation, and the experiment, as regarded the other parts of the building, abandoned. The unfavourable conditions mentioned then disappeared. A less crowded and better ventilated condition I consider to have been the chief influence which determined the comparatively low rate of mortality in this hospital.

Whilst much of the increased mortality of fever in hospitals must be ascribed to the deleterious influence of concentrated exhalations from the living body in a state of disease, some influence must be ascribed to the moral effects upon the weakened mind of a patient who finds himself suddenly removed from the solacing attentions of those who from natural affection loved him, and, instead, tended by strangers upon whose sympathies he feels he has no claim, and surrounded by the dying and dead of the hospital ward. The depressing effect of such an influence cannot well be estimated; but that such

an effect is produced, is in accordance with general experience, and must be obvious.

But the mere transference of a fever patient to hospital has an immediate influence upon the general mortality, which requires notice. In the records of hospitals there are frequent entries of cases termed "moribund on admission." The deaths from fever in Glasgow Infirmary in 1846 were 207, and Dr Orr[1] states, on the authority of "a very accurate table kept for him by Mr Brown, the janitor of the Royal Infirmary," that, "In 74 of the deaths in the fever wards, the patients had been less than thirty-six hours in hospital, and of these 38 died within twenty-four hours, and 16 within twelve hours after admission." Such a statement is very startling, but no fair inference can be deduced unless the previous duration of illness is also known. To illustrate this point I have constructed the following table, showing the length of illness prior to admission, and the total duration of illness at the period of death in 1316 fatal cases of fever (see p. 109) treated in the Royal Infirmary, Clyde Street and Anderston hospitals, and a similar table of 217 fatal cases occurring in out-door practice. I have also prepared corresponding tables, which show the proportion out of 100 deaths which occurred within successive intervals of four days after coming under treatment:—

	Duration of illness prior to admission.	Total Deaths.	Total duration of illness.							Ratio per cent. of Deaths at each period.						
			Under 4 days.	5 to 8 days.	9 to 12 days.	13 to 16 days.	17 to 20 days.	21 to 24 days	Above 24 days.	Under 4 days.	5 to 8 days.	9 to 12 days.	13 to 16 days.	17 to 20 days.	21 to 24 days.	Above 24 days.
Hospital Cases.	1 and under 4 days,	201	2	48	27	31	21	16	56	1·9	23·9	13·4	15·4	10·4	8·0	27·8
	5 — — 8 —	495	...	34	114	98	63	45	141	...	6·9	23·0	19·8	12·7	9·0	28·9
	9 — — 12 —	150	...	...	16	44	22	22	46	...	...	10·7	29·3	14·7	14·7	30·6
	Above 12 —	62	...	...	...	6	7	11	38	...	...	...	9 7	11·2	17·7	61·3
	Not known,.....	408	139	64	44	36	24	27	74	34·0	15·7	10·8	8·8	5·8	6·6	18·1
	Total,.....	1316	141	146	201	215	137	121	355	11·0	10·9	15 3	16·6	10·2	9·2	26·7
	Total Cases known	908	2	82	157	179	113	94	281	0·2	9·0	17·3	19·7	12·4	10·3	30·9
Out-door Cases.	1 and under 4 days,	46	3	8	9	12	5	6	3	6·5	17·4	19·5	25·2	10·8	13·0	6·5
	5 — — 8 —	100	...	3	27	31	17	7	15	...	3·0	27·0	31·0	17·0	7·0	15·0
	9 — — 12 —	36	...	...	7	15	6	3	5	...	...	19·4	41·6	16·6	8·3	13·8
	Above 12 —	23	...	...	...	7	1	7	8	...	...	...	30·4	4·2	30·4	34·7
	Not known,.....	12	8	...	1	1	1	...	1	66·6	...	8·3	8·3	8·3	...	8·3
	Total,.....	217	11	11	44	66	30	23	32	5 0	5·0	29·4	30·4	13·8	20·6	14·7
	Total Cases known	205	3	11	43	65	29	23	31	1·4	5·4	20·9	31·7	14·1	11·2	15·1

This table presents several points of interest which do not properly come within the scope of my present inquiry. One circumstance requires special notice—*i. e.*, the large proportion of deaths which occur in hospital within a few days after admission. The greatest number of deaths, both in hospital and in out-door practice, take place between the 13th and 16th days of illness,—the proportion of deaths gradually rising till this critical period, as it may be termed,

[1] Edin. Med. and Surg. Jour., 1838.

and declining afterwards, as from a culminating point. But, while in out-door practice this ratio is regular and progressive, there is in hospital an irregular and suddenly increased proportion, who die within the first few days after their admission, *and prior to the critical period.* This peculiarity will be better understood on referring to that portion of the table which shows the ratio per cent. of deaths occurring at given periods ;[1] but the matter will be placed in a still clearer light by a reference to the number of cases which proved fatal within 48 hours after admission into hospital. The following table includes all such cases in the experience of Clyde Street and Anderston hospitals; but does not include the Infirmary, as the materials from which I have prepared the Infirmary cases show the duration of illness only in days :—

Ill prior to admission.	Total Deaths.	Died.		
		Under 6 hours.	6 and under 24 hours.	24 and under 48 hours.
Not more than 4 days.....	18	2	4	12
5 to 8 days	36	5	13	18
9 to 12 „	9	2	6	1
Above 12	8	...	3	5
Not known	72	8	39	25
Total	143	17	65	61

The inference deducible from such facts undoubtedly is, that the mere act of transference to the hospital *accelerated* the deaths of this over-proportion of patients who died within a few hours of their admission into hospital. It is also only reasonable to infer, that the same injurious influence, which in so many cases had a well-marked effect in *accelerating the period of death*, must have operated prejudicially upon these and upon other cases, and so *increased the susceptibility for death.* It appears, indeed, that, although in most of these cases the fitness of the parties to bear removal had been previously determined by a medical man, there were, nevertheless, upwards of one in every nine of the whole fatal cases, which proved fatal within 48 hours of being admitted into hospital.

In out-door practice the district surgeon has not unfrequently to regret that he is only called in to be present, and to sanction with the name of medical attendance, the struggle of death; but the pro-

[1] The table is read thus :—Of 100 fatal cases, which, when admitted into hospital, were from 1 to 4 days ill, 1 died within a few days after admission; 23 died within 4 days after admission; 13 died within the next 4 days ; 15 died between the 13th and 16th day of their illness; and so on.

portion of such cases does not amount to one-half that which occurs in hospital experience, and it is chiefly limited to those cases seen about the critical period, and not at that early stage of the disease when the energies of the patient are comparatively vigorous.

The mere removal of a fever patient must, therefore, be reckoned a great evil as regards the patient. It has always been so considered by the district surgeons. Dr Easton[1] regards it as one of the most important of those influences which increase the mortality from fever in hospital. He says, that "the *vis medicatrix naturæ* is counteracted by the removal, to insist on no other evil, of fever patients to the Royal Infirmary. The risk of removing a patient, after fever is fairly formed, to any place, is, of course, equally as great as it is to the Infirmary. Almost every one who has had much experience in the treatment of fever knows that perfect rest, low diet, regulating the bowels, and applying cooling lotions to the head, &c., are in six cases out of ten all the requisites in the cure of fever, and that under such means the disease will exhaust itself, by running its regular course; reverse the circumstances of the patient,—let him, after he has been under the complaint for four or five days, be removed in the erect position to the distance of half a mile, more or less,—let him there be subjected to the coarse operation of the tonsor, and the debilitating influence of the warm bath,—let his 'throat thereafter be made a thoroughfare for wine,'—and will any man deny that there is treatment which, in nine cases out of ten, the case, if left at home, would not have required? It is *the removal of patients*, then, after the disease is formed, which is the chief cause of danger, but not the only one. Having, however, already recited the prologue, I need not pursue the tragedy; yet, O, shade of Armstrong! think only of the *opium*, the *cerevisia fortis*, the *spiritus vini Gallici*, &c. &c., and say do such follow out the indications of cure!"

Setting aside all allusions to difference in medical treatment, which, indeed, are here evidently made in a sportive manner—for Dr E. distinctly states, that there is no intention, on the part of the district surgeons, "to arrogate a superiority to their neighbours in the treatment of fever"—there is very much of truth in his remarks. I have been told of patients dying on their way to hospital; of others, who, on their arrival, were in such an extreme state of exhaustion, that there was really not time to have their names transferred to the medical books ere they expired; and I have seen entries in fever hospital books of patients, who, on their arrival, were placed in the *customary warm bath*, and who actually "died in the bath!"

Such facts, with others sufficiently familiar to those having much experience in hospital practice, show that there are circumstances, some of which are partially, and others entirely, beyond

[1] Glasgow Medical Journal, op. citat.

the control of the physician; and which run up the total number of deaths, and, consequently, limit very much the accuracy of any deductions drawn from hospital statistics, in reference either to the skill of the medical officers, or the results of any special mode of treatment.

Enough has been now adduced to establish the fact, that the mortality from fever in hospital very far exceeds that of out-door practice; and that this difference is not attributable to any disadvantage with regard to the nature of the cases which come under treatment. If this conclusion is admitted (and it seems unavoidable), it then becomes a matter of very grave consideration, how far fever hospitals are serviceable to the community, and how far they can claim the suffrages of the medical profession. This question is not now started for the first time. In the Fifth Annual Report of the Registrar-General, there occurs the following striking passage:—

"When the house is small, and perhaps dirty, it appears a great act of charity to remove a poor man—suffering from fever, for instance—to an hospital, where he is provided not only with skilful medical attendance and with physic, but with the proper diet, warmth, and nursing. But have the benevolent founders of hospitals, and the medical officers who generally give their services gratuitously, sufficiently reflected on the probable consequences of bringing 50, 100, 300, or 400 sick bodies under the same roof, and into a few yards, which the sickly breath saturates? Have they ever compared the results of cases treated in hospitals, and in the poorest houses? When hospitals are crowded, the increase of mortality soon becomes striking. In the Hotel-Dieu, at one time, one in every four persons who entered the pestilential walls died. The mortality in the large metropolitan hospitals is twice as great as in the smaller country hospitals. This cannot be ascribed to defect of medical skill in the metropolis. Erysipelas and gangrene are still not infrequent in hospitals. The patient is fortunate who escapes phlebitis or purulent deposits, after any serious operation, in an hospital. It is the adventitious disease, and not the knife of the surgeon, that is fatal. Are not the effects of general hospitals of the same nature as those that have, more than once, led to the evacuation of lying-in hospitals? I have rarely seen any statement of the mortality of fever, small-pox, or any other disease, in which it did not appear that a person was twice as likely to die in an hospital, as a person suffering from the same disease out of doors. It is generally said,—'Yes, we admit that our mortality is high; but the worst cases are sent, in the last stage of illness, to the hospital.' Is this certain? Will it account for all the difference? I doubt it very much. And I express the doubt, in the hope that the question may be strictly, honestly, and conscientiously investigated by some qualified person, who has time to devote to the subject. Until this be done, no attempt should be made to extend the system of assembling the sick in the same buildings."

This is the question I have endeavoured, in some measure, to elucidate; but it would be to me a matter of great regret, if the tenor of these observations had the effect of impairing the real usefulness of fever hospitals. This is far from my intention. But I believe that much ignorance—or, at least, uncertainty—prevails upon the subject, and I am satisfied that no real evil can result from its fair discussion. While some, from their peculiar and limited experience, raise an incessant appeal for increased hospital accommodation, others are probably too ready to denounce their existence, as

productive of unmitigated evil. I have endeavoured, by a cautious comparison and analysis of results, to arrive at a juster conception of their real value. I have not too rigorously drawn conclusions from the facts which I have collected, knowing that there are many drawbacks upon the absolute accuracy of statistical tables, and that considerable allowance must be made for disturbing causes.

I am now satisfied that, in many respects, fever hospitals are injurious—they entail a great expense—they have but a small influence on those evils which they are intended to prevent—they are subject to other evils which it is almost impossible to avoid—and the hazardous relief they do proffer can be obtained often only by a sacrifice of some of the best feelings of our nature. But I am also satisfied that they must be regarded as necessary evils; and so far necessary, in the present condition of such a city as Glasgow, that I would approve of their number being increased,—providing always, that *the present barriers which prevent the prompt and free admission of fever patients were at the same time done away with.* I would approve of the erection of *district hospitals* (as first recommended by Dr Cowan),—small, unpretending edifices, capable of accommodating fifty or sixty patients, and established in those situations where fever is known most to prevail. Cases do occur—and will, it is probable, always occur—where, but for the fever hospital, our senses would be outraged by the spectacle of the sick, miserable poor, clogging our streets and by-lanes, and dying unrelieved, under circumstances which precluded the possibility of effective relief being afforded. A readier admission into hospital would secure a larger proportion of early cases,—an increased number of hospitals would lessen the distance which the fever patient is carried, and so far lessen the exhaustion, and consequent risk, incurred by the fatigue of removal, —while the increased accommodation made available would lessen the dangers connected with over-crowding, defective ventilation, and a necessarily vitiated atmosphere.

But how much better would it be, if the necessity for fever hospitals did not exist? Such an idea is not Utopian; for, in many places, fever is comparatively unknown, and many likely means are unemployed by which the sanification of our large cities may be, in a great measure, secured. Glasgow, hitherto, has stood proverbial and pre-eminent for the constant prevalence of fever, and the awful extent of its ravages; and I can find no sufficient evidence that its fever hospitals have had any appreciable influence in improving its condition in this respect. Neither have they, as has been shown, sufficiently mitigated suffering, and prevented the loss of human life. These, however, are the benefits they are intended to confer; and for both these objects, it is too generally supposed, they are the most efficient agents, and they, at least, receive the most liberal patronage. Their importance, therefore, has been over-rated, and too much has been expected from them; and, so long as the practice and hygienic measures of our public men, medical or otherwise, are

influenced by such impressions—which I humbly conceive to be erroneous, and arising from inexact observations and hasty generalisations—so long will their personal energies and their influence be withheld from devising and carrying into effect other remedies, which are better calculated to *prevent disease*, or at least to overcome those causes which are known to foster its growth, and aid very greatly its extension,—and so long, also, will the general mass of society be compelled to walk in the path of those who fancy that there are certain stereotyped forms, according to which charity must needs work, and who deem it impossible that mankind can ever be benefited except by the adoption of their theories and the employment of their remedies.

It may be said that other remedies are occasionally had recourse to; but I place no value whatever on those superficial measures adopted during the temporary excitement which occurs during times of epidemic sickness. No medical man of experience in the condition and diseases of the poor, whose opinion I have obtained, considers that the bustling proceedings I allude to, in the way of lime-washing, fumigating, burning of old clothes and straw beds, &c., effect any real benefit either in preventing or in breaking the force of the epidemic. On the contrary, they are frequently injurious; and it too often happens, that the irritative cough, severe bronchitis, or other serious ailments which afflict, and mayhap destroy, the fever patient, are fairly attributable to the damp, pungent exhalations which, for many days, come from the sloppy lime-washed walls of his cold, ill-ventilated apartment. These measures may have the effect of allaying the apprehensions of that timid portion of the public who have least cause of fear, by giving colour to the usual newspaper announcements, that "the authorities are fully alive to the emergency, and are adopting all necessary precautions," &c. But when the frequently recurring epidemic has passed away, after carrying desolation to many hearths, there will still remain all the influences which invited its approach and prolonged its stay. Among these, there will still remain that monster-nurse of epidemic and contagious disease, the low unlicensed lodging-house, without regulation or supervision; dirty, ill-managed, ill-ventilated; occupied promiscuously by both sexes, and crowded to an awful extreme by trampers, vagrants, and all those who have no fixed occupation or residence. There will still remain the dark, damp, filthy, and noisome labyrinths of narrow wynds and deep courts, of loathsome cellars and suffocating garrets, shut out from the sun, and impervious to wholesome currents of air, without sewerage, or water supply, or any of those requisites, the want of which in large towns is incompatible with healthy existence. These will still remain; and with them an unfailing nucleus of pestilential and other diseases, which filter through a thousand unperceived channels among the whole community, originating disease, shortening life, and impoverishing all classes and conditions of society.

ARTICLE III.—*On the Pathological States of the Lung connected with Bronchitis and Bronchial Obstruction.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.

NOTWITHSTANDING the great frequency of bronchitis in European latitudes, and the attention which has always been given to this complaint by physicians, especially since stethoscopic examination came into use, it has appeared to me that what has been written as to its ultimate effects upon the pulmonary texture is not commensurate with the importance of the subject, and in many points not consistent with the truth of nature. Having become deeply impressed with the truth of the views to be advocated in the following papers, and having taken much pains to satisfy myself as to their correctness, I venture to lay them before the profession, together with some of the observations on which they are founded, in the hope that, if corroborated by the further researches of others, they may lead to improvements of some importance, both in pathology and practice.

It is necessary to state that while I have drawn my materials chiefly from personal observation, I have endeavoured to combine and harmonise the conclusions thus arrived at with those of prior observers, so far as known to me; nor have I willingly omitted any species of evidence which seemed to bear upon the subject. It would have been, indeed, an unsatisfactory task to have brought forward the results of personal experience on this subject, had they not been found to agree in many, even in most points, with those of some former authors in respect to matters of detail. In placing these pages in the hands of the public, therefore, I profess myself anxious for judgment as to the *truth*, more than as to the *novelty*, of their conclusions; and shall be satisfied if enough of originality be found in them as a whole, to rescue them from the charge of being a superfluous or burdensome addition to our literature.

PART I.—PRIMARY RESULTS OF BRONCHITIS.

In the present part, the direct and primary results of bronchitis will be discussed; comprising under this head those effects which follow, almost constantly, the accumulation of mucus and inflammatory products in the bronchial tubes, when these are sufficient in quantity to cause serious obstruction. A second part will include the secondary and more permanent disorganizations of the pulmonary texture, which result from the former under peculiar circumstances; being induced either by long-continued intensity of the original disease, or by constitutional states unfavourable to the removal of its results.

Obstruction of the Bronchi.

The ordinary effects of bronchitis upon the mucous and other contents of the bronchial tubes, are, for the most part, well understood and clearly described by authors. The vascular engorgement of the mucous membrane, which at a later stage becomes much softened and thickened, and the loading of the tubes with an altered mucous secretion, intermixed to a greater or less degree with purulent matter, are familiar to every one. The connection of these sources of obstruction with the auscultatory and other signs is also so well known as to require but few observations.

The varieties in the character of the secretion from the diseased mucous membrane are very great. In the earlier stages of the affection, the mucus is profuse, thin, watery, and frothy, being mixed with air bubbles of all sizes. At this period it contains but few microscopic elements, a few altered epithelium cells being only visible, and these in much greater number in the mucus procured from the dead body than in that expectorated by the living patient. At a later period the mucus has become yellowish, more tenacious and viscid, containing numerous pus corpuscles, and more of the altered epithelial elements. Occasionally, but more rarely, the expectoration, or the fluid in the bronchi after death, is composed of pus nearly pure, but always viscid and thick, like that which flows from old abscesses. In these cases the pulmonary tissue is rarely unaffected.

When the disease has been of considerable duration, and has nevertheless been the result of an acute attack, there are usually found, on incising the bronchi, numerous yellowish pellets, or irregular flocculent masses of somewhat curdy consistence, which float in the more recent and thinner mucus, without in any degree becoming amalgamated with it. These masses are found of all sizes, and constitute a well-known form of the expectoration of bronchitis in its advanced stages. They are no doubt formed by the evaporation of the more fluid parts of the mucus, in consequence of the constant passage over it of dry air; the inspissated material clinging to the walls of the air passages, until detached and washed away by renewed exudation from the mucous membrane. Such a mass is not unfrequently seen plugging the whole calibre of one of the larger or smaller bronchi, in such a position as to leave no doubt that in the act of inspiration it must have acted the part of a ball-valve, completely preventing the access of air to the part of the lung involved, by falling back upon the orifices of the smaller bronchi, into which its size would prevent it from entering.

When bronchitis, or bronchial catarrh, has existed for some time in a comparatively slight form, and with the expectoration only of an increased quantity of mucus unmingled with pus, portions of this secretion become inspissated in the form of a thick, glairy, tenacious, semi-transparent material, sometimes resembling raw white of egg. In one or two cases I have pressed out of the smaller bronchi plugs

of altered mucus of a still more tenacious character, like cold glue, and nearly, if not quite, transparent. But this amount of inspissation is not common.

The effects of these morbid accumulations in producing obstruction of the bronchial tubes are well known to the auscultator. So long as the mucus is thin and watery, or even more or less purulent, there is no serious or complete impediment to the passage of air; which, as the fine and coarse mucous rales, accompanied by vesicular respiration, indicate, finds its way through the fluid to the ultimate bronchial and pulmonary vesicles. The fluids, too, at this stage, move freely throughout the bronchial tree even to its minutest branches, and when in excessive quantity, are readily expectorated in the act of coughing. This stage of bronchitis, therefore, is comparatively little apt to be accompanied by urgent dyspnœa, or by changes in the condition of the pulmonary texture.

It is otherwise, however, when after a time the secretions within the bronchial tubes have become inspissated; the mucus having either become purulent and formed itself into the tenacious pellets above mentioned, or assumed the stringy, tenacious consistence proper to the more chronic forms of the disease. When, under these circumstances, expectoration is hindered, either by the tenacity of the mucus itself, the weakness of the patient, or any other cause, the tubes become really obstructed, the sound of vesicular respiration is at some points of the lung much diminished, or it may be altogether lost, and the bronchial râles are correspondingly modified, indicating the passage of a smaller quantity of air with a much greater amount of resistance; the ordinary mucous rattles being supplanted in part by the sharp "clicking" and valve-like sounds so well described by Dr Williams.[1]

It is quite evident to the observer of a patient in this condition, or, indeed, in any stage of bronchitis attended with laborious breathing, that the chief difficulty is invariably in *inspiration*. This act is accomplished only with the aid of all the accessory muscles; and, even then, the contracted state of the chest, especially in children, and the imperfect descent of the diaphragm, show that in proportion to the power employed, the amount of air entering is small. The expiratory act, on the contrary, is always accomplished with comparative ease; and any serious obstruction to the performance of this act is at once got rid of, or dislodged, by the additional impulse given to the expired air in coughing or hawking. These phenomena of laborious breathing, particularly the long-drawn, exhausting, inadequate inspiration, are probably quite peculiar to obstructive bronchitis. They occur, it is true, in diseases of the heart; but in them, I believe, only secondarily, from the accumulation of mucus in the respiratory passages. The dyspnœa of *pure* pneumonia, on the other hand, is something quite different, being a mere *acceleration* of the respiration, without

[1] Diseases of the Chest, 4th edition, p. 80.

any of the heaving or straining inspiration observed in bronchitis, or in cases where the two diseases are combined. So much is this the case, that I have repeatedly observed patients affected with a great extent of pneumonia in both lungs, and in whom the extreme lividity, and the respirations numbering 50 or 60 in the minute, showed infallibly the amount to which the function of the lung was interfered with: and who nevertheless lay quietly in bed, breathing without any of the violent effort, or the disposition to assume the erect posture, so constantly accompanying the more dangerous forms of bronchitis. If this freedom from orthopnœa and laborious breathing be not uniformly characteristic of true pneumonia, it is because that disease comparatively seldom exists, as is well known, uncomplicated by some degree of bronchial affection.

The cause of the inadequate and laborious inspiration in bronchitis while the expiration is comparatively easy, is to be found, I believe, not merely in the smaller power of the inspiratory muscles to dilate the chest, nor in the advantage which the expiratory forces derive in the dislodgement of obstructive mucus from the sudden impulse of coughing, but in the mechanical relations of the tenacious inspissated mucus to the calibre of the bronchial tubes. On this subject I have more to say in the next division of my subject.

One fact of considerable importance, in connection with the total obstruction of bronchi by muco-purulent matter, has been adverted to by Andral, and is illustrated by two cases in the "Clinique Médicale." This is, that the accidental position of such an obstructive plug may be the cause of a very rapid and unexpected fatal issue, in a case by no means threatening from the violence of inflammatory action. In each of the two instances given by Andral, the respiratory murmur became completely suppressed in the upper part of one lung, the patients having been previously affected with moderate bronchitis, and dyspnœa having become suddenly increased after a fit of coughing. The post-mortem examination showed the signs to be due to an obstructive accumulation in the bronchi leading to the upper lobe of the lung; but the absence of respiratory murmur, combined with clearness on percussion, had led during life to the diagnosis of emphysema. There can be, I think, little doubt that many of the paroxysmal accessions of dyspnœa in persons affected with bronchitis are due to accidental change of position of the pellets and ropy masses of inspissated mucus, which accumulate in the tubes. At least stethoscopic examination frequently reveals the signs of obstruction in particular parts of the lung, supervening rapidly, and disappearing again with equal suddenness, in consequence of the accession of cough.

I have now to advert to a condition of the pulmonary texture which appears to me, from the results of my own dissections, to spring more directly than any other from obstruction of the bronchi by mucus of a certain degree of tenacity. As this condition, the collapse of the air-cells, has been but little noticed by authors in this

connection, at least in the adult lung, the subject will perhaps be best introduced by a narrative of my own observations with regard to it, which I shall endeavour afterwards to connect with the facts furnished by others.

Collapse of the Lung as connected with Bronchial Obstruction.

During the epidemic of continued fever which prevailed in Edinburgh in the greater part of the year 1847, it was frequently observed that the lungs, in persons of all ages, were the seat of a form of condensation, characterised by the absence of the friability and granular appearance on section of pneumonic consolidation, and also by the peculiarity of its microscopic elements, the large granular cells which form so common an ingredient in ordinary red hepatization being either very sparingly or not at all present. The condensed portions were usually scattered over both lungs, and often very limited in extent, being accurately circumscribed by the margins of the lobules; in most other respects they corresponded with the descriptions to be hereafter given. These appearances occurred very commonly in typhus fever, whether of the ordinary form or the abdominal typhus (now commonly called *typhoid fever*), accompanied by intestinal ulceration; which latter form had at that time a prevalence quite unusual in Edinburgh. Similar lesions were occasionally, though less frequently, seen in the relapsing fever, which was also epidemic during that year. This state of the pulmonary tissue was the subject of frequent conversation, and repeated microscopic examination, among those chiefly engaged in the pathological theatre of the Infirmary during this period; although my own notes of fever cases at this time are but few, I am fortunately able to compare my recollections with several sufficiently characteristic descriptions of the lesion in question by Dr Waters, whose examinations were recorded with the utmost care and fidelity, with a view to the preparation of a history of the epidemic unfortunately not yet published.[1]

The nature of this pulmonary affection appeared to me, at the

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[1] From one of Dr Waters' reports I quote the following:—"Inferior lobe of left lung felt condensed, and contained less air than ordinary; its section was smooth, not granular, and its consistence somewhat tough, not breaking down under the finger." On another occasion, "The lower lobe of the left lung was much gorged with blood, and of a dark colour, and smooth section, not granular, void of air, and sinking entirely in water." The reader may compare these with the characters which follow (p. 131) of the bronchitic collapse. The following description from the case of a child dying of fever, corresponds to the characters of lobular collapse, combined with the bronchial abscesses to be hereafter described. There were condensed nodules in the lung which "presented at the surface of the organ, where they were recognised by their bluish-black colour and resistent feel; they were not elevated above the surrounding surface, but rather very slightly depressed." * * "On section, the great majority of these masses presented a reddish-blue colour and smooth surface. They were not friable but of firm consistence. A few of them had degenerated into a reddish-gray somewhat friable substance, and two or three were excavated by central cavities, which were empty and collapsed."

time, very doubtful. By some it was looked upon as corresponding with the "typhoid deposits" in the lung described by German authorities; and the imperfect cell-forms often seen under the microscope seemed to favour this view. On the other hand, careful examination showed that there was frequently little or no real *deposit;* the cell-forms differing very little from those which might be procured from the normal lung, or still better, from one compressed by pleuritic effusion, or any other cause. At this time, my experience in pathological studies was not such as to enable me to form a decided opinion; but towards the close of the epidemic, the result of observations inclined me to believe that the lesion so frequently witnessed in fever was not connected with any specific form of morbid deposit, but was, in its purest form, a condition of imperfect expansion or collapse of the pulmonary tissue, similar to that described in the lungs of infants under the name of *atelectasis*, and differing only in its distribution in patches, and in other accessory circumstances, from the condition of *carnification* described by Laennec as the result of compression of the organ.

That the fever of 1847 was not, in any degree, *specially* characterised by these lesions is fully proved by the researches of other authors, as well as by my own subsequent experience. In fact, in the course of the two succeeding years, I had various opportunities of becoming familiar with collapse, or carnification, of the pulmonary tissue, as a disease distinct from pneumonic consolidation, occurring in various forms, and under circumstances where no external compression of the lung could be conceived to account for it. Many of these lesions occurred in the lungs of children, but those of adults and old persons were scarcely less frequently affected. The disease presented itself, also, frequently in combination with other affections, which gave rise to no small difficulty in forming an opinion on the true nature and mode of origin. Neither was it always very easy to distinguish it from pneumonia, more especially in some of the combined conditions to which I shall hereafter allude; and I am satisfied that the terms *lobular pneumonia*, *red hepatization*, or the more indefinite expression, *condensation*, have often sufficed in my own case, as well as that of others, to cover ignorance or imperfect knowledge of the condition in question.[1]

[1] Had opportunities of examining the lungs of very young children frequently occurred to me, I could scarcely have failed to have soon become familiar with all the phases of this lesion; and to have recognised much sooner than I did, the identity of the congenital atelectasis, the "lobular pneumonia," and the carnification or collapse of the adult lung. But owing to the exclusion of all children under five years of age from the Infirmary, my observations were confined almost entirely to the lungs of persons above the age of infancy; and it was only at a comparatively recent period that the observation of a few infantile lungs, and the perusal of the work of M. Legendre (Recherches Anatomo-pathologiques et Cliniques sur quelques Maladies de l'Enfance, Paris, 1846), revealed to me clearly the immense importance and frequency, though not, I believe, the true significance, of this state of the pulmonary texture in the early periods of life.

Notwithstanding these difficulties, however, observations made in the years 1848-9 left me no longer in doubt, that the lungs of adults not unfrequently display portions more or less limited or diffused, which are so perfectly condensed as to sink rapidly in water, and yet differ widely, in appearance and microscopic character, from truly inflamed lung, as well as from all the atrophic conditions of the organ which can be clearly traced to inflammation or structural disease as their cause. In some instances, such portions presented exactly the appearance of fœtal lung; and only the manifest absurdity of the proposition could have prevented the observer from ascribing their state to the same cause as the congenital non-expansion of the lung, or atelectasis of Jörg. In the absence, therefore, of any manifest explanation of the compression or collapse of the tissue observed in these cases, I was obliged, provisionally, to rest satisfied with the knowledge of the fact that most of the lobular and many of the more diffused forms of condensation usually ascribed to pneumonia, in the adult as well as the child, were really the result of some other and unknown condition.

Some time after this collapsed condition of the lung had become tolerably familiar to me, the following case occurred, presenting a marked example of the coincidence of the affection with obstructed bronchi, *both lesions being limited to a very small and circumscribed space in the lung.*

CASE I.—*Lobular collapse of Lung—Obstructed bronchi.*—(P. R. 257.[1])

A man, æt. 18, affected with necrosis of the femur, anasarca, and some degree of abdominal dropsy from disease of the liver, spleen, and kidneys, died in the surgical hospital in the last stage of exhaustion and emaciation. No pulmonary symptoms had attracted notice.

The lungs were generally normal in appearance. At one or two places, however, they crepitated imperfectly over spaces not larger than an inch in diameter; these portions were quite circumscribed by abrupt margins; the bronchi leading to them yielded on pressure a very tough gelatinous mucus (like thick calves'-foot jelly), which contained only ciliated epithelium, and had otherwise the ordinary appearance of mucus under the microscope.

In this case there was little or no room for fallacy in judging of the connection of the pulmonary collapse with bronchial obstruction. The parts affected, with the bronchi leading to them, were quite capable of being isolated; and there was no trace of an inflammatory affection in any part of the pulmonary tissue. The co-existence of the collapse with bronchial obstruction thus accurately limited, could only, in all probability, be explained by the dependence of the one upon the other as its cause. The following case, however, which occurred during the present year, is still more conclusive.

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[1] These figures affixed to a case, refer to the numbering in the Pathological Register of the Royal Infirmary, vol. xii., where the details which I have omitted, as not bearing on the argument, will be found, usually in a pretty extended form.

Case II.—*Extensive Collapse of Lung—Obstruction of Bronchi by a Tubular Membrane.*—(P. R. 301.)

A girl, aged about 21, was attacked, after a surgical operation upon the tongue, with urgent dyspnœa having the character of a laryngeal affection. Tracheotomy was performed, but failed to save her. She died about twenty-four hours afterwards.

Dissection performed Feb. 22, 1850. The appearances in the air passages and lungs, were the following :—

The right lip of the glottis was infiltrated with serum, which distended the epiglottidean fold of mucous membrane so much as nearly to close the opening. The larynx and trachea were occupied by a tubular false membrane throughout their whole length ; it was about a line in thickness, friable, and of a yellowish white colour. This membrane was continued at the bifurcation into the right bronchus, but the left was free from it. It could be traced throughout the bronchi of the right lung even into the minuter ramifications, in which it assumed the form of a very soft opaque matter, like a thick emulsion. On examination with the microscope, the membrane presented the usual appearance of coagulated fibrin, with some pus or mucus corpuscles, but these not in very large numbers.

Both lungs were more collapsed, and contained less air than natural. The left, however, was much less affected than the right. The latter was at some parts completely flaccid and free from air, while others presented an imperfect crepitation. The upper lobe crepitated more than the others, and its colour was mostly natural ; but in its lower portions it approached more in colour to the middle lobe. This was perfectly non-crepitant, of a grey colour, having a smooth flesh-like section, and infiltrated with a thin serosity, having a faint reddish tinge, nearly transparent, and presenting under the microscope only epithelium, pigment cells, and a very small amount of pus cells, and granular matter. The lower lobe was not quite so much compressed, and contained a good deal of blood. The bronchial glands were slightly enlarged, and the whole pulmonary mucous membrane highly vascular.

In this case, as in the former, there were no pneumonic appearances ; for the small amount of pus cells which existed in the serosity scraped from the lung, were probably derived from the small bronchi, and were, at any rate, quite inadequate to account for so remarkable a consolidation. Neither can it be supposed that there was any older structural lesion of the pulmonary substance, as the girl was undoubtedly in good general health at the period of her admission into the hospital, and the whole stages of the fatal disease were too rapid to admit of any complicated structural change. The collapsed lung presented exactly the appearances so commonly seen in organs compressed by pleuritic effusion, except that the middle lobe had rather more of an œdematous appearance than is common under such circumstances. Here, then, is an instance of collapse of the entire lower lobes and part of the upper in the right lung, as complete as it could have been from external pressure, and coinciding with a manifest cause of obstruction in the whole of the bronchi of that lung ; whereas on the opposite side, where the obstruction had been much less considerable, the lung was comparatively expanded and normal. It is impossible not to see here a relation of cause and effect ; and as it will not be maintained that the deposit of false

membrane was the result of the collapse, the converse proposition is, I think, scarcely to be avoided.

The form of bronchial obstruction in this case is a sufficiently rare one in the adult, but by no means unexampled. It is not at all clear whether the membrane had never formed in the bronchi of the opposite lung, or had been dislodged by expectoration. In the former case it is not easy to understand the non-symmetrical character of the fibrinous exudation, while the mucous membrane appeared equally inflamed on both sides of the chest; in the latter it is still more incomprehensible, how the complete expectoration of an extensively ramified membrane could take place, without being observed during life. But of the fact of its presence on the right, and its entire absence on the left side, there is no doubt.

The observation of these two cases, and the conclusions which appeared naturally to follow from them, as to the connection between collapse of the air cells and bronchial obstruction, tended to throw light on many of the obscurities of the preceding observations. More particularly, it seemed to me that they furnished the connecting link necessary for the explanation of the peculiar forms of "lobular pneumonia" (as they were generally considered), which had frequently occurred both in the lungs of children and adults. The peculiar connection of lobular condensation with bronchitis was a matter of common observation with me before this period; the more recent literature on the diseases of the lung in children also seemed to point to the same fact; and it seemed every way probable that many of the more complex phenomena of pulmonary disease, would receive explanation from the more careful consideration of their connection with obstructed bronchial tubes. I therefore renewed my observations with great interest, directing them particularly, and with more care than formerly, to the relative condition of the bronchial tubes and pulmonary tissue, in all cases where either pulmonary collapse or bronchitis existed to any considerable extent. Some of the illustrative cases which have occurred, will hereafter be detailed. The general result to which I have been led is, 1st, that in all cases of collapse of the lung not caused by external pressure, the bronchi have presented unequivocal appearances of obstruction; 2d, that in most, if not all, the instances of severe and fatal bronchitis, especially if the secretions had become ropy or inspissated, more or less collapse of the pulmonary texture has also been present; 3d, that under peculiar circumstances, which will be presently adverted to, a much less amount of obstruction may be attended with collapse of the pulmonary texture, the symptoms in such cases probably attracting little attention.

In order to justify these conclusions, it will be necessary to describe a little more fully the forms under which I have observed the lesions referred to.

Bronchitic collapse of the lung occurs under two distinct aspects: the *diffused* form, and the limited or *lobular* form. Of these, the

latter variety is the more striking and characteristic, and has been, especially in the lungs of children, the subject of more discussion than the former; but the diffused form is by far the more common, and is in fact of very frequent occurrence, at least in its slighter degrees. Both forms present the same fundamental changes of the pulmonary tissue, which is usually of a dark violet colour externally, as seen beneath the pleura; and internally of a more or less deep brownish red, or mahogany tint. The colour, however, is by no means an invariable criterion, depending almost entirely on the amount of blood in the collapsed tissue. The affected parts are always more or less condensed; this condensation may amount to a mere diminution of the crepitation, or to a total absence of it, in which case portions are usually found to sink readily in water. These latter portions are both more flaccid and much less friable than the pulmonary tissue when in a state of red hepatization; and they differ greatly from this lesion in the aspect of their section and the nature of the fluid it yields to the knife. In every variety of true pneumonic consolidation, in which the lung is completely void of air, the air cells are occupied by a deposit, presenting to the naked eye (and still more distinctly to a power of 20 to 30 diameters) the well-known granular aspect of the hepatized lung. If the deposit is fluid, or semi-fluid, it is capable of being pressed out, or scraped off, in the form of a thick opaque emulsion-like matter, of yellowish, orange, or gray colour; and in all cases it shows, under the microscope, abundant granular elements and cell-structures, of the kinds usually found in inflammatory exudations in parenchymatous organs. In the collapsed lung, on the contrary, the section is comparatively smooth, having somewhat the appearance, as described by Laennec, of muscular flesh; it presents no trace of granulations, and yields, on pressure, or to the knife, only a semi-transparent bloody serosity, which, under the microscope, is seen to contain little or nothing besides blood-corpuscles, epithelium, and other portions of normal tissue, and possibly a small amount of pus from the interior of the bronchi.

In the *diffused* variety, the collapsed condition may be found affecting a more or less considerable portion of either or both lungs, usually at their posterior part, and passing quite gradually into normal tissue; the supple, dense, tough feeling being exchanged for the normal spongy, elastic crepitation; and the violet, or deep purple colour, shading off into the usual hue of the surface. Even in this form of the lesion, however, a tendency of it at some points to be circumscribed by the interlobular divisions may often be observed; this tendency being, so far as I have observed, quite characteristic of the bronchitic, as opposed to the pneumonic, consolidations.

In the *lobular* forms of bronchitic collapse, which often occupy the anterior edges, as well as all other parts of the lung, the affected portions are everywhere accurately and abruptly marked off by the interlobular septa, the portions so limited being various in

size and form, but always manifestly shrivelled, and sunk in below the level of the surrounding parts. This is peculiarly manifest when they occupy the anterior edges. When they are scattered through the lung, they communicate to the fingers, in feeling the organ externally, much the same sensation as clustered tubercles in the midst of crepitant tissue. This is the form which was so often described as "lobular pneumonia" in young children, till the experiments of MM. Legendre and Bailly clearly showed it to be nothing but a collapse of the air cells.[1] These observers were in fact the first to apply to this condition of the lung the same test as Jörg had long before used in respect to the congenital atelectasis, and to show that the lung affected with "lobular pneumonia" could be generally restored nearly to its natural condition by forcible inflation, which occasionally requires, however, to be continued for some time.

I have employed inflation in both the diffused and lobular form of collapse, as observed in adults, and as above described, with precisely the same results as those of Bailly and Legendre. I may state, however, that though this test is very useful in demonstrating the nature of the lesion, in a favourable case, to one not familiar with its character, I do not believe it to be applicable to the determination of the presence or absence of pneumonia in those mixed cases in which alone there is any difficulty. For I have observed that on the one hand, the partially pneumonic lung may be inflated when the affection is recent, and combined, as it frequently is, with bronchitic collapse; and, on the other, that in the latter lesion in its purest forms, complete inflation is often very difficult or impossible after the collapsed state has been of some duration. In fact, the lung then begins to undergo a modification in its nutrition and structure, which ultimately leads to permanent atrophy.

With regard to the combined forms of lesion above alluded to, they are, I believe, by no means of rare occurrence. The collapsed lung, especially in the incomplete and diffused form in which it most frequently presents itself in adults, may become the seat of a true inflammatory exudation into the air-cells, giving to the section an obscurely granular aspect. The exudation has in such cases the usual microscopic characters, but the shrivelled state of the lung, and the minuteness of the granulations, together with the state of the bronchial tubes, demonstrate the participation of these in the inflamed condition. Occasionally, also, the collapsed lobules are subject to an œdematous infiltration of fluid, when this state prevails in the other parts.

I think, however, that I have also observed the collapsed parts, particularly in the well-marked lobular forms of this lesion, to escape, to a certain extent, the inflammatory or œdematous condition prevailing in the parts around them. At least, I have observed cases where these remained comparatively dry, containing

[1] "Archives Generales de Medecine," 1844; and Legendre's work before cited.

only blood, when other parts were bathed with frothy serum or pus. This subject, however, requires further investigation.

In maintaining (as I have no hesitation in doing) that some degree of collapse of the lung is an almost invariable concomitant of bronchitis of a certain degree of intensity, it must not by any means be supposed that *complete* loss of crepitation is to be looked for in any part of the tissue in the majority of cases. The usual fact is, that the collapse is in the incomplete and diffused form; but I believe, nevertheless, that dulness of percussion during life from this cause, and complete lobular or diffused collapse after death, especially in the posterior parts of the organ, will be found to be much more common in the bronchitis, as well of adults as of children, than is commonly supposed. Of the truth of these opinions, both recorded observations, and unrecorded recollections, appear to me to furnish no inconsiderable amount of evidence.

I insert here the following two cases, as good examples of collapse from bronchial obstruction,—the former in its diffused, the latter in its limited or lobular, form. The accounts during life are, in both cases, very inadequate; but they have, nevertheless, some points of great interest, apart from the lesion now immediately under consideration, which will render reference to them necessary in the sequel.

CASE III.—*Extensive Collapse of Posterior Portions of Lungs—Emphysema of Anterior Parts—Obstruction of Bronchi by Ropy Mucus—Death by Exhaustion.*—(P. R. 362.)

A woman, æt. about 30, died of exhaustion from the combined effects of Bright's disease and ulcerating cancer of the uterus.

The body, examined July 1st, 1850, was extremely pale, and the limbs, with the depending parts of the thoracic and abdominal parietes, were anasarcous. The abdomen was not remarkably distended, but a little fluid was found in the peritoneal cavity, and the surface of the membrane was studded over with small miliary granules. All the pelvic organs were more or less involved in cancerous disease, and small nodules, slightly larger than those on the serous membrane, were infiltrated into the muscles and fat of the abdomen. The kidneys were very pale, and presented an early stage of the waxy (non-granular) degeneration.

On opening the thorax, which appeared contracted and flattened laterally, the anterior edges of the lungs covered the heart more than usual. Both lungs were evidently emphysematous anteriorly. In the left lung large bullæ existed at the lower part of the upper lobe in its anterior prolongation; the dilated air cells forming a portion connected with the rest of the lung by a narrow process, composed of pulmonary tissue in part emphysematous, and in part completely collapsed and flaccid. The anterior edge of the lower lobe was also emphysematous. The pulmonary tissue at the posterior part of both lobes was perfectly collapsed and flaccid, tough, and of a violet colour; not containing enough of air to give the feeling of crepitation. The crepitant tissue passed very gradually into the collapsed. In the right lung there was diffused emphysema of the anterior edges, with here and there collapsed portions. Fully one-third of the posterior portion of this lung was completely collapsed; the rest more or less crepitant,—the non-crepitant tissue at some points shading off gradually into the crepitant; at others being pretty sharply divided from it by the interlobular septa. On inflating the lungs artificially, the collapsed portions, for the most part, admitted air pretty freely when considerable force was employed; some of them, however, even after several re-

petitions of this process, retained their collapsed appearance. On making incisions into the lungs, the dense portions were seen to be smooth on section, and of a deep mahogany colour, and contrasting remarkably with the brighter colour of the crepitating parts. The collapsed tissue yielded nearly pure blood to the point of the knife; the crepitating portions a frothy sanguinolent serum. Some of the denser portions sunk in water; other parts were indifferent; and many floated freely. A large quantity of tough, semi-transparent, stringy mucus existed in the bronchi of both lungs. The mucous membrane was of a purple colour. Heart healthy.

CASE IV.—*Lobular Collapse of Lung from Bronchial Obstruction—Interlobular Emphysema—Death from Tubercular Hydrocephalus.*—(P. R. 360.)

A child, aged about 5 years, much emaciated, died with symptoms of hydrocephalus. The state of the lungs not noted during life.

The body was examined June 30th, 1850. Inflammatory lymph was deposited beneath the arachnoid at the base of the brain, and very minute tubercles existed over some of the convolutions. The ventricles were distended with serum. The root of both lungs was occupied by enlarged bronchial glands, which, on being cut into, were seen to be infiltrated with crude yellow tubercle. The right lung was healthy, with the exception of partial and imperfect collapse of some parts of its tissue, and considerable redness of the mucous membrane of the bronchi. The enlarged bronchial glands in the left lung pressed upon some of the bronchi going to the lower lobe, so as distinctly to diminish their calibre, as ascertained by a probe. The upper and lower lobes were glued together by adhesions, and in the upper margin of the lower lobe was a rounded portion of condensed lung of the size of a walnut, which, on being cut into, showed a thick cluster of yellow tubercles, mostly confluent, and at some parts breaking down into pus. The anterior extremity of the lower lobe was completely collapsed and violet coloured, evidently sunk below the rest of the lung. The anterior end of the upper lobe showed a very few slightly enlarged air cells at the extreme margin; the interlobular septa were emphysematous over a considerable extent of this margin, even where the air-cells were normal. On inflating the left lung, it was found that the collapsed tissue could be imperfectly inflated by using considerable pressure, but subsided again when the pressure was removed.

In the last case, besides some general bronchitis, there was a special obstructing cause in the bronchi going to the collapsed lobules; their calibre being diminished by the encroachments of enlarged bronchial glands, and of the isolated tubercular mass in the lung itself, which was situated immediately in contact with the collapsed part, between it and the root of the lung. In both these cases, the inflation of the diseased parts was effected with difficulty, and in some parts imperfectly; showing that their nutrition was already becoming modified, and the state of permanent atrophy was supervening on that of temporary collapse.

I have now to advert to the observations of the authors who have described collapse of the lung as a state distinct from pneumonic consolidation, or pleuritic carnification, with the view of showing in how far their researches have tended to throw light on the question of its origin. Laennec described the diffused form of collapse only in connection with pleuritic effusion and compression; and there can be little doubt, from some expressions in his descriptions of the first and second stages of pneumonia, that he must, to a considerable extent, have confounded the two conditions, especially in their more

mingled and less characteristic forms. That he has not altogether overlooked the peculiarities of the lobular collapse, however, is evident from a passage in which he speaks of meeting with carnified portions of the size of a filbert or an almond in the midst of very crepitant pulmonary tissue. The occurrence of these he ascribes to "a slight inflammation in the first stage, the resolution of which, hastened perhaps by compression of the lung, has taken place in an irregular and imperfect manner."[1] This expression, while it shows that this great pathological anatomist had been puzzled to account for the lesion in question, will not be accepted as anything more than a hypothetical explanation of it. Very many writers, following Laennec for the most part in their pathology, have thrown this passage out of view altogether.

In 1829, M. Louis described the condition of the lung in a variety of cases of typhoid fever. These descriptions were repeated in his work on fever in 1841, in which the state of the lung in fevers was compared with that found in other diseases. The result of these inquiries was a most accurate description of the collapse of the lung as a state altogether different from pneumonia, and which was found not only in typhoid fever, but in a variety of other diseases; chiefly at the posterior part of the lung, but sometimes *disseminated* (lobular.) M. Louis offers no speculation or remark as to the origin of this affection.[2]

In 1830, Dr Alderson, in a paper on the "Pathology of Hooping-Cough,"[3] pointed out most clearly the distinction between the lobular condensation observed in that disease, and true hepatization of the lung. "In hooping-cough the lung is always dense and contracted, as if the air had been expelled, and from the throwing out of adhesive matter the sides of the air cells had been agglutinated together; while in hepatization the lung is less dense than in hooping-cough, and is rendered more voluminous than in its natural state."[4] The inflation of the lungs would probably, in some cases at least, have clearly disproved this supposed agglutination of the air cells. The state of the bronchi is carefully described by Dr Alderson. In one case, for instance, "most of the tubes were filled with a light yellow secretion, which, in the greater number, had assumed a concrete form, having very much the character of fibrine; in others, it was in the form of a thick puriform mucus; where it occurred in the concrete form, it adhered, though slightly, to the lin-

[1] De l'Auscultation Mediate, vol i., ch. v., art. 1, sect. 366.

[2] Recherches sur la Gastro-enterite, I. 361; and Recherches, &c., sur la Fievre Typhoide, I. pp. 328-334. It is worthy of remark that, from the observations in the preceding pages on the Edinburgh epidemics of 1847, as well as from Dr Jenner's recent careful descriptions of cases observed in London (Monthly Journal, Feb. 1850, p. 115, et seq.), this form of pulmonary lesion does not appear to be peculiar to any type or form of continued fever, at least in this country.

[3] Medico-Chirurgical Transactions, vol. xvi. p. 78.

[4] Loc. Cit. p. 91.

ing membrane of the tubes."[1] It will not be questioned that these descriptions concur entirely with the views already laid before the reader in this paper, as to the origin of the lesions in question.

The researches of Jörg, in 1832,[2] gave a new impulse to infantile pathology, by showing the frequent occurrence in new-born children of a state of deficient expansion of the pulmonary lobules, having all the appearance of being congenital, but often persisting for some time after birth. This state, which Jörg called *atelectasis*, was identical in its characters with the collapse described above, and, like it, was often lobular in its distribution. It was figured as a disease of the new-born child by Cruveilhier,[3] who, however, understood nothing as to its real nature. Jörg was the first who pointed out the effects of inflation in this form of pulmonary affection as contra-distinguished from pneumonia. He ascribed the collapse to various causes, but particularly to weakness on the part of the child, and to the circumstances of a too precipitate birth, which he conceived, somewhat fancifully, to militate strongly against the establishment of respiration. In this last view he has had few supporters.

In 1838, MM. Barthez and Rilliet published a monograph on infantile pneumonia, which afterwards became the basis of the extended treatises in their great work on the diseases of children.[4] While these authors describe with great care and general accuracy the appearances of the carnified lung, whether lobular or diffused, they fall into the error, at that time universal in France, of considering it as a form of pneumonia; and their reference to the researches of Jörg is so slight as to render it probable that they were not aware of the important facts established by him. An interesting remark, in relation to the present subject, is, however, made by MM. Barthez and Rilliet, viz.:—that bronchitis, especially of the smaller bronchi, is a frequent concomitant of the pneumonia of children, *especially the lobular form* (collapse); the connection of bronchitis with the lobar pneumonia being more rare.[5] Similar observations had been made by M. Fauvel and other authors.

Rokitansky, writing in 1842, does not allude to any of the researches hitherto mentioned; nor does he appear to be sufficiently aware of the distinctive marks and real nature of collapse of the lung. In his description of *lobular*, *typhoid*, and *catarrhal pneumonia*,[6] however, the reader will trace many of the characters of this condition. In regard to the last of these affections, he says, "It is always lobular, concurring with catarrhal affection of the bronchial ramifications leading to the diseased lobules, and occurring frequently in catarrhal attacks in children, especially hooping-cough and catarrhus suffocativus." * * "The lobuli affected are

[1] Loc. Cit. p. 85. [2] De Pulm. Vitio Organico; Leipz. 1832; and Die Fötus-Lunge im Gebornen Kinde; Grimma 1835.

[3] Anat. Pathologique, livraison 15, plate 2, fig. 1.

[4] Traité Clinique et Pratique des Maladies des Enfans. 1843.

[5] Op. Cit., vol. i. p. 75. [6] Path. Anat. vol. iii.

blueish-red, dense, and rather tough; the walls of the air vesicles are swollen, so as to obliterate their cavities, and contain, when less swollen, a sero-mucous, slightly frothy secretion; there is no trace of granulations." Rokitansky also notices the sinking in of the affected lobules, which he ascribes to an emphysematous condition of the surrounding parts.[1]

In 1844 were published the important researches of MM. Bailly and Legendre, before alluded to, which demonstrated the identity of the "lobular pneumonia" of children with the congenital collapse, or *atelectasis*, of Jörg. These authors also describe the catarrhal affections of infants as often attended with this change. But it is singular that, notwithstanding their own application of inflation of the lung to show the nature of this lesion, they consider it as produced, in some instances, by distension of the blood-vessels causing closure of the air cells. It is clear that under such circumstances inflation could not effect any considerable change.

The greater number of these authors, in so far as concerns the diseases of children, are passed in review by Dr West in his late carefully elaborated, and, at the same time, original lectures.[2] The descriptions of Dr West well deserve to be read, on account of their clearness and accuracy of detail; and his views as to the frequent connection of the collapse of the lung with bronchitis, and its dependence, in many cases, on that affection, harmonize so closely with my own, that I can scarcely regret the late period at which I became acquainted with them,[3] as it enables me to point out more clearly the identity of the conditions in the adult with those in the child. "In the child," says Dr West, "nothing more is needed than a copious secretion of mucus into the bronchi, or a feeble condition of the vital powers, to prevent the air from freely entering the pulmonary vesicles, and thus to induce the collapse of a large portion of the lung."[4] I have already endeavoured, in part, to show that in the adult the same causes are capable of producing the same effects; and I shall hereafter explain more at length what I conceive to be the exact mechanism of this change, both in the one and in the other. Dr West adduces three cases, examined by Dr Baly, of lobular collapse of the lung in the adult, in persons who died in a state of great exhaustion from fever and dysentery. In two of these cases there was much dyspnœa and distinct signs of bronchitis. The third is said to have presented no complication, and is considered by Dr West to be the "result of simple debility." I shall refer to this point in the sequel.

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[1] Path. Anat., vol. iii., p. 106.

[2] Lectures on the Diseases of Infancy and Childhood: Lond. 1848.

[3] Dr West's researches have come to my knowledge only since this paper was read, in its original form, to the Medico-Chirurgical Society.

[4] Op. Cit. p. 183.

On reviewing the whole of the facts here presented to the reader, I think that the frequency of collapse of the pulmonary tissue, both in the adult and the child, must be considered as established, and its connection with bronchial obstruction rendered at least extremely probable.

I have yet to discuss the exact mechanism by which the collapse is, under these circumstances, produced, and the predisposing and other causes which determine its occurrence; but this subject will be treated after the next effect of bronchitis has been considered, as the mutual connection of all the primary results of bronchial obstruction can then be more advantageously discussed.

(*To be continued.*)

ARTICLE IV.—*Case of Chronic Empyema treated Successfully by Paracentesis.* By A. PEDDIE, M.D., F.R.C.P., Physician to the Minto House Hospital and Dispensary, Edinburgh, &c.

ROBERT NEILSON, aged 26, picture-frame maker, near Denny, was admitted to Minto House Hospital on the 27th October 1846, being recommended by Dr Alison to Professor Syme for the operation of paracentesis thoracis. He stated that, while in Dublin in the beginning of January of the same year, after complaining for some weeks of slight cough, he was suddenly affected with what he understood to be a pleurisy of the left side. For this he was cupped, repeatedly blistered, took mercury and hydrodate of potass, and was sent back to Scotland in the end of February, very weak and much distressed in breathing. In the month of June he was advised to sea-bathing, which he tried without benefit for two months. Various practitioners in the country were consulted, and the general opinion appeared to have been, that he was affected with "a hardness of the lungs," which blistering and time only could remove.

On admission his general symptoms were,—very considerable debility, great breathlessness, and palpitation on the slightest exertion, and these to a distressing degree, accompanied with cough when attempting to lie on the right side. His nights were very restless, but not feverish; his pulse was 80—of ordinary character, the tongue clean, the appetite good, and the bowels regular. The external aspect of the chest to the eye, as well as measurement, showed a decided enlargement of the left side, considerable filling up under the clavicle, and great bulging of the inferior and lateral parts, which was particularly apparent when viewed from behind. The intercostal spaces generally were completely filled up, and almost prominent in the lateral region; and inspiratory efforts, although very conspicuous in the right, produced no expansive movements in the left side. There was also slight fulness in the upper part of the abdomen. Percussion over the side was universally dull, but especially stone-like beneath the mammary region; and not the slightest feeling of vocal vibration was communicated to the hand. Auscultation gave a weak but rough and somewhat bronchial respiration above and immediately below the clavicle. It became less audible as the mammary region was approached, where, and underneath, it was entirely absent. It was also very faintly heard in the upper and back part of the axilla; and, posteriorly, from the shoulder to the lowest part of the thorax, and to an extent of about four finger breadths from the spine, there was a purely bronchial respiration audible. In approaching this limit a slight but distinct friction sound was heard. The voice-sounds furnished a degree of bronchophony in the summit of the lung, and along the

vertebral portion already noticed; but in no part was œgophony heard. On the right side of the chest there was considerable dulness on percussion over the whole anterior surface, but particularly great between the sternum and mamma downwards. Here, also, respiration was heard indistinctly; but very clear and puerile in all the other parts of the chest. The heart was found to be lying between the sternum and mamma on this side, and its apex distinctly seen striking about an inch below, and external to the nipple. The impulse and sounds were normal, with the exception of a very slight prolongation of the second sound, but not amounting to a bruit. It being considered that paracentesis, cautiously performed, held out the only prospect of recovery, Mr Syme, on the 31st October, drew off 21ʒ of very thin, greenish-coloured pus, from which a little serum separated on standing for a short time. Again, on the 7th November 50ʒ of a similar fluid were removed; and on the 14th, 34ʒ—the chest being now apparently emptied. The instrument employed was a trocar, about $\frac{1}{18}$th of an inch in diameter, and about $2\frac{3}{4}$ inches in length. The parts selected for puncture were different points, between three and four inches external to the nipple in the sixth intercostal space; and the patient lay on a sofa, with the trunk of the body overlapping its edge a little, and the left elbow resting on a chair. None of these operations were followed by the slightest local pain, or any general disturbance. There was much relief afforded to the breathing; and the respiratory murmur in the upper part of the chest was much improved in character, and a little in extent. He could exert himself moderately, and lie in bed with comfort, and even continue for a short time on the right side. The heart, also, was now found to beat much nearer the sternum. No general treatment was pursued until after the last tapping, with the exception of restricted diet, and an occasional purge with the compound powder of jalap. Now, however, a large blister was applied over the side, and gr. ij. of the iodide of potass given twice a-day. He was dismissed on the 17th November—to continue the medicine, and return again in a few months.

Was re-admitted on the 7th April 1847—improved in general appearance, but again complaining much of dyspnœa and palpitation. Mr Syme tapped him on the 9th, taking away 40ʒ, and on the 15th removing other 26ʒ. On both occasions the situation formerly punctured was made choice of, and the fluid evacuated was perfectly purulent, and the relief obtained was as great as formerly. Dismissed on the 19th.

Was admitted again on the 7th July of the same year, when he was found to be very much in the same state as when first brought under treatment, but having decidedly increased in strength, and presenting a healthier aspect. On the 8th paracentesis was again performed by Mr Syme, and 79ʒ of pus removed. From the smallness of the canula the stream continued to flow for nearly an hour, and as it became feeble the position of the body was altered so as to drain off every drop from the pleural cavity. On the 13th he was dismissed in all respects very much improved. The fulness of the infra-clavicular region was not now apparent, and the respiration there was much more natural, and the heart's pulsations, although still considerably to the right of the sternum, were fainter, as if the organ had retired considerably from the parietes of the chest.

Was re-admitted on the 29th September, in very much the same condition as on last admission. On the 30th 112ʒ of pus were removed—the stream continuing to flow for nearly one hour and a-half; and on the 1st October, as he was free from all uneasiness, and desirous to return home, he was accordingly dismissed.

Returned again on the 1st February 1848, very well in general health, but feeling his breathing much embarrassed, and being much distressed with palpitations. 72ʒ were now withdrawn—the pleural sac being quite emptied; and he returned home on the following day. Paracentesis was on this occasion performed in the seventh intercostal space, in the most lateral part of

the chest; and the pus drawn off was observed to be browner than formerly.

From this period up to the 4th of January last, I performed paracentesis on him four different times, draining off, on the 18th May 1848, 57ℨ; on the 19th August 1848, 37ℨ; on the 27th February 1849, 67ℨ; and the 4th January 1850, 26ℨ.

Thus he was operated on twelve different times, and in all 621ℨ of pus—equal to about 4 gallons—were taken from him. On each successive admission there was a manifest improvement of his general health and strength; he could undergo a very considerable amount of fatigue, and on the six last occasions he returned home on the day following the tapping, without the slightest inconvenience or injury. The changes, too, in the configuration of the chest, more especially in the disappearance of the infra-clavicular fulness, were very marked; and when last seen, the intercostal spaces had much the same aspect as those of the opposite side; and there was now some degree of movement of the side, during respiratory efforts, very different from its previously paralysed condition. More healthy respiration, also, was heard anteriorly, as low as the edge of the pectoral muscle, and also along the back near the spine. The heart now lay close to the sternum on the right side, and its impulse and sounds were perfectly normal. In the three last tappings I found that considerable force was necessary to introduce the trocar, owing, apparently, to the thickness and hardness of the pleura. This was particularly the case in the last operation; and, although the canula of the trocar measured two and a-half inches in length, it seemed short enough for the depth of parts to penetrate. I may remark, also, that the fluid drawn off, although secreted in greatly increased quantity from November 1846 to September 1847—at which time it was going on at the rate of 56ℨ per month, from that period until the 4th January last it came gradually down in quantity until it had arrived at a ratio of little more than 2ℨ per month. It is interesting, also, to remark, that while the fluid was sero-purulent at first, and afterwards became pure pus, that at the last tapping a considerable amount of serum separated from the pus. This change, viewed in relation to the progressive improvement of Neilson's health in all respects, made me feel confident that a cure would soon be complete; and, accordingly, I am glad to report that, in a statement which I had from him a few days since, he says that he considers himself quite recovered from the affection; that he has felt so strong and well since he was last under treatment, that he recently entered into the bands of wedlock; and is convinced that no farther operative interference will be necessary. He states that he is now capable for any ordinary exertion, can lie on either side without oppression of breathing; and does not consider that there is any material change in the configuration of the chest—unless it be a slight depression of the shoulder, and tendency to stoop, which, however, he states was the case before the inflammation occurred. Of course, when a cure in this case is said to be complete, I admit that there is a lung, of which, perhaps, less than one-third only is fitted to perform any part in the function of respiration; that there is an enormously thickened pleura, with strong adhesions connected therewith; and that there is a dislocation of the heart from its own compartment of the thorax, to which it can never return. Notwithstanding all this, I regard Neilson's case as satisfactorily cured, seeing that the functions of respiration and circulation are now carried on compatibly with existence; that the strength is re-established, and fully adequate for the duties of his calling; and that, instead of feeling life as a burden, he is now in a condition, and is seeking, to enjoy it.

The above case has been thus minutely detailed, as its interest and value appear to me to depend on its being so. It differs widely in several respects from hitherto recorded cases of empyema—so far as I am aware. In those detailed by Dr Hamilton Roe, in his ex-

cellent paper on "Paracentesis Thoracis,"[1] &c., the operation was performed at a comparatively early date, excepting in one case, which was complicated with phthisis—of which the patient died after the lapse of several months. The sum of Dr Roe's experience accords with that of Laennec and others. He says,—"In any instance where paracentesis was employed in an early stage of empyema, or inflammatory hydrothorax, it was successful, and failed to cure only in those where it had been long delayed." (P. 234.) The duration of the disease must doubtless be viewed as a main obstacle to cure from paracentesis, or any means; for not only are the thoracic viscera themselves subjected to serious injury in the way of compression, atrophy, displacement, and adhesion, but the power of life must ere long become undermined from the presence of a noxious fluid, as well as from long-continued derangement of the functions of circulation and respiration. Relief by operative procedure, may thus be sought for too late. Neilson's case, however, is remarkable for its chronicity; and in spite of the existence of ten months between the pleuritic attack and the first tapping, and its continuance during a period of three years and two months, recovery has fortunately been accomplished. The most promising feature in the case was its being a simple pleural accumulation, uncomplicated with other thoracic disease; and there can be no question, but that had an operation been earlier performed, the affection would have been brought sooner to a satisfactory termination. Regarding those cases of hydrothorax, inflammatory or mechanical, which may or may not, at an early period, be treated by paracentesis, it is not at present necessary to give an opinion; but it appears plain that, as a general rule, whenever pus is detected in the pleural sac, it ought to be evacuated in this way; and that the operation be repeated from time to time, as the breathing begins to be embarrassed in consequence of a fresh accumulation. Should there be a decided prominence or protrusion at any one point of an intercostal space, and more especially if attended with pain or tenderness and discoloration, indicating that the matter is seeking an outlet for itself, the best practice is to allow it to point well, allowing nature to perform her own work, unless the symptoms of dyspnœa, &c., are urgent, when an artificial opening must be immediately made. Even in such circumstances, however, it is advisable to draw off the fluid by tapping in the first instance, rather than by incision, as it is impossible to predict what the diseased membrane will bear in the way of a wound inflicted on its surface. In cases complicated with pneumothorax the case is different, for much greater freedom may be used; although it is perhaps most judicious to make the incision small at first, and dilate more freely a few days afterwards. I have in this manner treated successfully several interesting cases of

[1] London Medico-Chirurgical Transactions, vol. xxvii.

this serious affection, in which there was reason to believe that the fistulous communication with the lung had been formed by an empyema bursting through its substance. In Neilson's case it was considered proper at first to withdraw only a portion of the fluid at a time; but it was soon apparent that the whole might, with safety, be evacuated at once; and this practice I should not hesitate to adopt in any future case, with the precautions to be afterwards noticed. In reflecting on the history of the case, the impunity with which the various operations—twelve in all—were practised, and the manifest relief derived from each, it is impossible to avoid the conviction of the unlikelihood, or rather the utter hopelessness, of expecting that a cure could have been accomplished by the long-continued use of blistering, iodine, and mercury, which would have been trusted to by some practitioners. The main objection to paracentesis-thoracis has been the apprehension of air finding an entrance to the pleural sac, and thus exciting violent inflammatory action. To avoid this supposed danger in Neilson's case, all due precautions were taken. These consisted, as I have already noticed, in the employment of a very small sized trocar and canula, so as to make it impossible that any large rush of air inwards could take place, and to insure more certainly the immediate closure of the wound; in prohibiting conversation during the flow of the fluid, so as to maintain equal and easy inspirations, and prevent any suction power being exercised from within; in the change of position during the operation,—turning the body over, cask-like, when the stream was becoming feeble; and in withdrawing the canula the instant the stream became interrupted. I agree with Drs Townsend[1] and Roe in thinking that this source of danger from paracentesis,—the admission of air during the operation,—has been much exaggerated, which is proved by abundance of surgical experience, both in operations with the trocar, when air was known to have entered freely, and with the lancet and bistoury in cases of pneumothorax, when its exclusion was not sought. The experiments of Nysten and Speiss also sufficiently prove that air introduced into the pleura is invariably removed by absorption in the course of a few days. Dr Roe, however, says, "It is clear that if we evacuate more fluid than can be replaced by the expanding lung, air *must* fill the vacuum; but as this does not seem to be attended with either danger or inconvenience, I think it better to evacuate the whole of the fluid at once with the trocar." (P. 225). Now, admitting the truth of the well-known saying, that "nature abhors a vacuum," he keeps out of view altogether the extent to which the collapse of the thoracic parietes on all sides by atmospheric pressure will, in such circumstances, assist in diminishing any space which the partial replacement of its contents, besides the expansion of the compressed lung—small though it may be—and the ascent of

[1] Cyclopædia of Practical Medicine, art. Empyema, p. 49.

the diaphragm, cannot accomplish. All this is the more likely to occur, provided paracentesis is performed with the precautions already recommended.

Some surgeons have treated cases of empyema by leaving the canula or a catheter in the wound. Dr Roe objects to this fatal practice, "because it allows a *continual* ingress and egress of air" to the pleural sac, and thereby excites inflammatory action. Now, although this source of danger must be very great in any case, uncomplicated with pneumothorax—or in which nature has not herself prepared for a fistulous opening, the main danger would arise from the direct and continued irritation of the instrument left in the wound, acting as a foreign body by pressure on a membrane predisposed to diseased action. It is strange that this latter explanation, rather than the former, did not occur to Dr Stroud, when remarking on a case published by him, in which a fatal result followed the use of an elastic gum catheter after paracentesis.[1]

It is from the infliction of a similar injury that bad consequences generally follow the old method of performing paracentesis. It consists in making an incision in the intercostal space, drawing aside the skin, and then penetrating the pleura with a common hydrocele trocar. It is a clumsy expedient, possessing an appearance of scientific ingenuity, with the view of valving the internal part of the opening, and obtaining a better closure of the wound. Unfortunately, however, it often happens that the wound either does not heal by the first intention, or soon breaks open again; and when that occurs in a bad subject, or where there was no preparation made, as I have already remarked, for a fistulous opening, mischief is almost inevitable. The danger, however, is not from air admitted during the operation, but partly from its admission subsequently and constantly, and more especially from the violence done to the pleura by the operative procedure itself. This method contrasts ill with the simple and safe practice adopted in Neilson's case, in which, although twelve times tapped, on all these occasions there was an immediate closure of the trifling wound, and in none the slightest approach to inflammatory action.

A remarkable feature in Neilson's case was the extent to which the heart was displaced, for its apex was found to beat fully an inch external and inferior to the right nipple. That this should not have occasioned a greater change in its action and sounds is unexpected, although quite in accordance with the observations of Laennec and others in cases of less extreme dislocation.

This case also points out in a very clear light the sure marks which characterise a pleural accumulation, and distinguish it from other diseased pectoral states. The displacement of the heart alone, viewed in connection with the general symptoms, was quite sufficient to prove the existence of a large effusion in the left

[1] Medical Quarterly Review, vol. 1, p. 184.

sac; but the nature of the case was made still more evident by the general enlargement of the chest on this side discoverable by the eye; the fulness of the infraclavicular region; the disappearance of the intercostal spaces generally; and their rounded, although not prominent, aspect at the lower and lateral part of the chest. Then there were the very significant signs of immobility during respiration; and the total absence of vocal vibration. No disease could present more precise indications than the above; and although the remaining physical signs furnished by percussion and auscultation were the least important, as they always are in this affection, they lent their evidence to confirm satisfactorily the diagnosis. It may be asked, however, was there anything in Neilson's case, before paracentesis was performed, to lead to a conjecture regarding the quality of the fluid—whether it was purulent or serous; or, can we in any instance distinguish an empyema from a hydrothorax? In this instance the sign of stony-hardness communicated by percussion might be considered as some test of the consistence of the fluid present; and the very well filled up, even roundish although not decidedly prominent condition of the intercostal spaces, might be considered as indicative of the same, if we credit the statements made in recorded cases of this affection. But, in addition to these diagnostic guides, I would draw attention to the circumstance of the duration or chronicity of the affection, as suggestive of the quality of the pleural contents, and state my conviction that this, with other signs, may be found a valuable fact to aid in the diagnosis. In some attacks of pleuritis a purulent product is formed at once,—within a few days or weeks: such may be designated as cases of acute empyema. They occur generally in unhealthy, strumous subjects; the attack is severe from the first, attended with distinct rigors, and much fever and dyspnœa; and the fluid is not formed in such quantity as to occasion a very obvious enlargement of the affected side. These are the instances in which, if they do not fall under the observation of a discerning practitioner, and paracentesis is not early performed, an eschar takes place at some part of the pleura-pulmonalis, and the fluid, bursting through the lungs, converts the case into one of pneumothorax; or else it finds its way through an intercostal space, and points and discharges spontaneously; or the latter mode of vent-finding may follow the former. In the more chronic cases of empyema I do not doubt but that the fluid accumulated has often been purulent from the first; but I think it probable that in the greater number of instances the membrane which in the beginning poured out serum, now takes on the action of secreting pus. This I think may be inferred from the history of cases in which paracentesis had been repeatedly performed, and in which the first operation, early had recourse to, produced serum; the second, serum, turbid with albuminous matter floating in it; and the third, sero-purulent matter, or true pus. Of course the rapidity of this change will depend

much on the constitution of the individual and other circumstances; and that it occurs in instances in which tapping can have had no share in altering the nature of the secretion, I have no doubt. Very few instances indeed, I believe, could be adduced in which paracentesis, long delayed after the existence of a pleural accumulation was positively ascertained, produced anything but purulent matter. In regard to Neilson's case, I am decidedly of opinion that in the first few weeks or months of the pleuritic effusion it was serous, but gradually, from increased distension of the diseased membrane, its morbid action was increased likewise, and the secretion became more and more purulent. The fluid drawn off at first was to some extent sero-purulent; and what was taken last—the thorax having been long freed from extraordinary pressure, and the powers of the constitution having been somewhat restored—had a still greater proportional amount of serum in it. On the whole, therefore, I am of opinion that the fact of the duration of a pleuritic effusion is, taken in conjunction with other signs, of considerable value in the diagnosis of its quality.

I have only now, in concluding these observations, to remark that in all probability, the progress of pathological change and recovery in Neilson's case happened in the following order: Layer after layer of adventitious membrane was formed on the pulmonary and costal surfaces of the pleura,—particularly of the latter,—adding progressively throughout the whole course of treatment to their thickness; that after each successive tapping, agglutination to some extent took place from above downwards; and that this, with a slight re-expansion of the lung, the ascent of the diaphragm, and a very complete collapse of the thoracic walls, effected obliteration of the entire cavity, with the exception of the supra-mammary and vertebral portions formerly described, which I have little doubt were walled in by stout adhesions at an early period of the affection.

ARTICLE V.—*Cases in Surgery.* By JAMES SYME, Esq., Professor of Clinical Surgery, Edinburgh.

POPLITEAL ANEURISM.

P. F., aged 33, was admitted into the Royal Infirmary on the 25th of April last, suffering from aneurism of the left popliteal artery. Five weeks before admission, he had first become aware of a swelling in the ham, with some stiffness of the knee, and pain extending down the leg. He continued his occupation of "street musician" until the end of a month, when he was obliged to relinquish it, on account of the increased stiffness of the joint, and pain of the limb, and at the same time observed a strong and constant pulsation in the seat of enlargement.

He became much easier after admission, from lying quietly in bed, and also, as it appeared, from the pressure of a bandage applied from the toes up to the knee. Indeed, he experienced so much relief, and the pulsation became so feeble, that some hopes were entertained of spontaneous recovery. But, in less than a week, the tumour, which nearly filled the popliteal space, began to enlarge, extending outwards, under the tendon of the biceps, and it was deemed inexpedient to delay the operation longer. It was, therefore, performed on the 15th of May, under the influence of chloroform. The artery was exposed and tied without the slightest difficulty, and with the loss of certainly not more than half a teaspoonful of blood.

On awakening from his sleep, the patient found himself perfectly free from uneasiness of any kind, and so continued until the ligature separated, which it did on the 10th of June. He was dismissed well on the 14th.

This case is related, not as presenting any new feature, but as tending to confirm the statements made, and frequently repeated, in this Journal, with regard to the safety and advantage of the ligature for the treatment of popliteal aneurism. The femoral artery, on the occasion just mentioned, was tied by me for the nineteenth time, without any bad effect resulting from the operation; and the perfect comfort of the patient subsequently to the operation, certainly presents a remarkable contrast when compared with the torment inflicted by pressure, no matter how applied, provided it be sufficient to impede the circulation adequately to cause coagulation.

The "Dublin Medical Press," of 10th July, contains the following characteristic paragraph, in an editorial article, referring to two cases of aneurism treated in London, and in which pressure had been followed by death :—

"The truth, however, must be told. The London and Edinburgh surgeons, either from sheer ignorance, or, what is worse, from sheer jealousy, deny the unfortunate sufferers from aneurism, who are so unlucky as to fall into their hands, the invaluable resource which compression affords. In the very teeth of overwhelming evidence in favour of this practice, they still, as we hear, continue to cut down on and tie arteries in cases which, if in Dublin, would, in all human probability, be cured by compression. They do not, forsooth, believe the accounts published of the success of the practice in Dublin, and they deny that the compression can be made effectually, or borne by the patient : and why ? Just because they do not know how to do the thing. We do not blame the two Scotch professors for their scepticism,—national vanity stands between them and many other improvements; but we do blame the hospital surgeons of London, who should know better,—and we most earnestly beg of them, for the poor patient's sake, to put the next case on board the steamer, and send it over to us in Dublin."

With regard to the invitation here so kindly offered, I am really at a loss to see what advantages could result from crossing the Irish Channel, in compliance with it. So far as is known to me, there has not been any fatal effect from ligature of the femoral artery for

aneurism in Edinburgh, during the last eight or ten years, in the practice of all the surgeons—four or five in number—who have had occasion to perform the operation. The average length of time required for maintaining the pressure in Dublin—where, it must be supposed, "they know how to do the thing"—being no less than upwards of thirty days and nights, the relative amount of suffering may be ascertained, by comparing this dreadful ordeal with the case just related, which affords a fair specimen of the procedure employed, and its effects. The risk of life and limb attendant upon pressure will only appear when all the unfortunate cases of its employment are published.

In the last number of this Journal allusion was made to the small amount of contributions to surgery which of recent years have emanated from Dublin. It would seem, indeed, as if the surgeons of that city, to whom their own science and art had been incapable of furnishing legitimate offspring, had been induced to adopt a rejected bantling, of French and English parentage; and, not satisfied with lavishing their affection upon this foundling, felt inclined to quarrel with every one not equally blinded to its defects.

OBSTRUCTED VAGINA, WITH RETENTION OF THE MENSTRUAL DISCHARGE.

A. D., aged 19, was admitted on the 10th of February. It was stated that, twelve months before, she had suffered two severe attacks of hæmatemesis, and had been confined to bed for two months by the weakness so occasioned. About five months before the time of admission, she noticed a tumour in the lower part of the abdomen. It was the size of a fist, firm, and seated above the pubes, rather inclining to the left side. Since then she had complained of pain in the back and lower part of the belly, with regular monthly exacerbations of these symptoms, and at each period had observed a distinct increase in the size of the swelling.

Upon examination it was found that the orifice of the vagina was obstructed by a dense thick membrane, through which a fluctuation on the inner side could be distinctly perceived. The abdominal tumour in size, situation, and consistence, resembled the uterus about the sixth or seventh month of pregnancy, and at the left side had a projection, somewhat larger than an egg, which seemed to be composed of a firmer substance. Dr Simpson, who had the kindness to see the patient at my desire, thought that this tumour might depend upon a fibrous growth of the uterus; but did not regard it as affording an objection to evacuation of the retained discharge, which unduly distended the organ.

On the 12th, I pushed a bistoury through the obstructing membrane, which was extremely thick and tough, and then guided the knife on my finger so as to enlarge the opening to the full extent of the vagina. Pressure being now made on the distended uterus, induced the contents to issue in the form of a semiconsistent

stream of reddish brown coloured matter, to the amount of at least four pounds. When no more could be pressed out, injections of water were thrown in both then and frequently afterwards, so as to wash out the remains of the accumulation and also the discharge, which continued for several days. Under this treatment the patient quickly recovered without any unpleasant symptom. The os uteri, which at the time of the operation was felt to be dilated as it is after parturition, soon contracted; all trace of the abdominal swelling disappeared; and the patient was dismissed well on the 7th of March.

The point of interest in this case seems to be the illustration which it affords of the advantage attending a very free division of the obstructing membrane, and the use of syringing with water subsequently. In similar cases a typhoid fever of alarming degree, and even death, has resulted from punctures made with a view to evacuation; and I feel indebted to the professor of midwifery for impressing upon me the importance of preventing any of the impurities from being retained. The knife is certainly much preferable to a trocar, and should be used with such freedom as to insure complete division of the dense membrane, which effectually resists the finger, so as not to admit of being torn.

FORMATION OF A SUBSTITUTE LOWER LIP.

The operation formerly in use for restoring the lower lip was one of the most unsatisfactory in the practice of surgery—so much so, indeed, that few practitioners felt inclined to try it more than once, if they did so at all. The principle of its performance was to obtain integuments sufficient for the purpose by dissecting a flap from below the chin, and turning it round, so as to occupy the vacant space. The ordinary result of this proceeding was sloughing of the transplanted part, which, in the event of escaping that danger, constituted a most unseemly and uncomfortable substitute for the absent part,—presenting no resemblance of the natural lip,—affording no protection against the dribbling of saliva,—and being a constant source of annoyance, from the beard causing irritation by growing in a wrong direction. Some years ago I proposed another method of proceeding, which simply, easily, and effectually accomplished the object. The following case, when taken in addition to those already published, may tend to increase the confidence which has been placed in this operation.

W. M., aged 58, from Perthshire, was admitted on the 27th of May, on account of a malignant-looking sore of the lower lip, which was in great part destroyed, and thoroughly diseased throughout the portion that remained. The disease had appeared about fifteen months before, and been subjected to operation in a provincial hospital, but subsequently returned.

The patient having been placed under the influence of chloroform, I removed the diseased parts, and made the dissection requi-

site for constructing a new lip, precisely as described in my original account of the operation,[1] and with the same satisfactory result hitherto experienced. The patient was able to permit the manipulations of the barber within a week, and was not only relieved from the disease, but provided with a lip quite similar to the natural feature, and perfectly efficient for all its duties in articulation, mastication, and closure of the mouth.

ARTICLE VI.—*Case of Malformation of the Intestinal Canal.* By GEORGE J. NICOL, M.D., Aberdeen.

MRS W., residing in Forbes Street, was delivered, on Tuesday, April 2, of an apparently healthy female child, which I was called to visit on the 4th, as there had been no evacuation of any kind from the bowels, though medicine and an injection had been administered. It was reported to me that the enema had been introduced with great difficulty. The child had been vomiting what had the appearance and smell of meconium. On examination, I could discover no appearance of hernia, or other ailment, and the anus was in every respect natural. Having introduced a female catheter into it, and finding no obstruction in the rectum, I directed the nurse to inject as much lukewarm water, with soap, as she could, and instructed her to let me know the result. In the course of the evening she informed me that there was great difficulty in getting the water to pass up into the bowels. Next morning, Dr Keith was kind enough to accompany me to see the child, and suggested that we should introduce a male catheter, and inject, by means of a syringe attached to it. This was accordingly done, the catheter having been introduced fully four inches; but the warm water was returned by the side of the instrument immediately,—showing that there was some obstruction higher up than we could reach, which, on post-mortem examination, was found to be the case.

The child lived till the morning of the 7th April—that is, about five days altogether—and continued to vomit up feculent matters, she having taken the breast frequently for the first three days. The countenance acquired a livid appearance for two or three days before death.

On the 8th, I made a post-mortem examination, and found considerable inflammatory redness and distention of the small intestines, and slight adhesions of their peritoneal surface. In tracing them along near the lower end of the ilium, the distension ceased, and on cutting into the gut it was found to be impervious, the mucous membrane apparently stretching across, and causing complete occlusion; the whole of the cœcum and colon, and also the lower extremity of the ilium below the obstruction, being quite rudimentary, and pervious to a small probe only, the rectum being rather larger, so as to admit a good-sized male catheter; there was only a little inspissated mucus contained in this part of the intestine, which had a cord-like feel, and was altogether about two feet long. I should suppose this had been a *lusus naturæ*, but I can discover no malformation exactly similar recorded in the works of Rokitansky or of other authors.

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[1] Contributions to the Pathology and Practice of Surgery.

Part Second.

REVIEWS.

A Practical Treatise on Diseases affecting the Skin. By the late ANTHONY TODD THOMSON, M.D., &c., completed and edited by EDMUND A. PARKES, M.D., &c. London, 1850. Pp. 440.

THIS work is a systematic treatise on diseases of the skin, in which the classification and divisions of Willan are followed in all their minute and very complex ramifications. The author was evidently anxious to communicate all the information that was known on the subject up to the time he wrote, and the editor has very ably carried out his intentions in this respect in the latter portion of the book. In an appendix, also, Dr Parkes has condensed some of the results arrived at by Dr Gustav Simon, of Berlin, whose investigations into the minute structure of the skin under various morbid conditions have so much tended to elucidate our knowledge of the morbid anatomy and pathology of this texture. The chief character of the present volume, however, and the one, indeed, which will give it value in the eyes of the profession, is its suggestions as to treatment with the illustrative cases in which cures were effected. The editor informs us that, "for more than thirty years Dr Thomson had paid special attention to diseases of the skin; he had observed them with great care and accuracy, and had learned to treat them with singular success;" and that, "at the close of his active and useful career, it was his greatest wish that his experience should be made useful in aiding his professional brethren in the great object of all practical men, viz., the safe and rapid cure of disease."

When we examine into the nature of Dr Thomson's treatment of skin diseases, we cannot fail to remark, that it is almost entirely of an empirical nature, a circumstance which leads us to conjecture that, however successful he, with his practical tact, may have found it to be, others will not derive from it the same amount of benefit. We shall notice, however, a few of the many modes of treatment he has recommended.

In the dropsy which follows scarlatina, we are told that the urine generally, when submitted to the action of heat and nitric acid, is found to contain much albumen, which the author regards rather as the consequence of a general inflammatory condition, than as any special state of the kidneys. Now, we think this involves a great error in pathology, because a microscopic examination of the urine under such circumstances will usually exhibit, in addition to a large excess of uric acid salts, numerous casts of the tubes, or

other proofs of structural changes in the kidneys themselves. The efficacy of diuretics, we are told, has been doubted, but the author has found them useful, and in many cases they certainly are so, but we are not told how to distinguish these cases. For our own part, we are of opinion that when the uriniferous tubes are blocked up by exudation, or an epithelial desquamation, diuretics, by increasing the amount of fluid behind these obstructions, favours their separation. At all events, it is exactly when the kidney is in this condition, not only in scarlatina, but in various other diseases, that we have found diuretics beneficial, and, consequently, consider that the rational indications for their use should never be neglected.

When speaking of the management of the variolous eruption, Dr Thomson alludes to a former statement of ours (*Monthy Journal* Jan. 7, 1848, p. 518), in which it was said that we had confirmed the good effects ascribed to mercurial ointment, thickened with starch, as an ectrotic, that is, a remedy which prevents pitting. We are not told that the author has ever tried it, and yet he says that, "after all the experiments which have been made with the view of effecting this desirable object, no topical application has yet been found to prove always successful." We can assure our readers, notwithstanding, that further experience has only confirmed our favourable opinion of this practice. In two or three cases we have observed salivation produced, and now order the mercurial ointment to be diluted with an equal part of lard.

Dr Thomson retains the distinctions between porrigo favosa and porrigo lupinosa,—telling us that the first consists of flat straw-coloured irregular pustules, and the second of yellow favous spots, which furnish a viscid, pus-like fluid, which oozes out and concretes into dry, circular, whitish-gray crusts, &c. We are certain that the two forms of porrigo here noticed are identical; that the favi are dry from the commencement to their termination, and that, although pustules may be occasionally present as a result of irritation, they are in no way connected with the peculiar eruption itself. In 1843, we had the pleasure of breakfasting with Dr Thomson, and then communicated to him for the first time the discovery of the vegetable nature of this disease. We shall not easily forget the marked incredulity with which he received this information, so wholly different from all his preconceived notions on the matter; and it is, therefore, with great pleasure we observe that he has since investigated the subject for himself. He says (p. 118), "I have examined the crusts under the microscope, and am now perfectly satisfied of their vegetable nature." Six illustrative cases of the disease in question are given, in four of which, we are told, that it commenced with *pustules*. A fifth case is said to be a doubtful one, and the sixth commenced with white scurfy scales, followed by the peculiar crusts of the disease. Of these six cases the last appears to us to be the only one which is well characterised. They are all said to have been cured in periods varying from seventeen

days to three months, by various kinds of ointment applied externally, as well as by an internal treatment referable to particular symptoms. The syrup of the iodide of iron is recommended in these cases. We cannot avoid the impression, that some of the individuals here said to be cured in such short periods of time laboured under impetigo, and not favus, of the scalp. This error in diagnosis is continually made, especially by those who hold that the disease commences in pustules. It was made even by Willan and Bateman, as any one may convince himself by examining their forty-first plate.

With regard to scabies, Dr Thomson says, "If there is any article of the materia medica that can be truly termed specific, we may venture to consider sulphur as such for the cure of scabies" (p. 137); and again, "no remedy but sulphur can be depended upon; and, when it is properly employed, every day's experience proves that no case can long withstand its curative influence," (p. 139). Without denying the good effects of sulphur, we think it might be easily demonstrated that several other remedies are equally efficacious; for instance, soft soap, simple lard, and a solution of the chloride of soda, which have also the great advantage of being more cleanly, and not giving rise to the odious smells which accompany the sulphur cure. We must confess, also, that, seeing the gross Hahnemanic delusion is principally founded on the supposed specific virtues of this drug in scabies, we have infinite satisfaction in showing by our practice that itch may be quickly, pleasantly, and permanently cured without having recourse to it.

In the external treatment of pompholyx, the author has found no plan equal to that of puncturing the bullæ, gently pressing out their contents, and then pencilling the parts over with a solution of nitrate of silver, in the proportion of a drachm to a fluid ounce of distilled water, acidulated with ten or twelve minims of dilute nitric acid. The application should extend at least half an inch beyond the base of the discharged bulla (p. 178). We have generally found that a local treatment in this disorder is of comparatively little avail—from the circumstance, that no sooner is one crop of vesicles cured than another appears. The constitutional treatment spoken of by the author, viz., purging, followed by mild diaphoretics, is much more to be depended on.

The papular diseases are treated under the heads of lichen, strophulus, and prurigo, each of which is divided into numerous varieties and described in reference to their symptoms, causes, diagnosis, &c. We are told that the principal treatment must be constitutional, assisted more or less with tepid and emollient baths. A quotation is given from Bateman (p. 211), who says, with regard to the treatment of prurigo, "I have seen considerable benefit derived from the internal use of the oxyginated muriatic acid (*acidum nitro-hydro-chloricum*), both the eruption and the itching yielding during its exhibition. It may be taken in doses of a fluid drachm,

and increased gradually to three times the quantity, in water or any agreeable vehicle." We have also found the nitro-muriatic acid very serviceable in papular diseases, having been led to its use by the frequent discovery of oxaluria in such cases; and we can assure our readers that, if they will examine the urine microscopically, the detection of oxalates will often lead them to the successful treatment of these very troublesome eruptions.

Our experience completely coincides with that of the author, in his condemnation of unctuous applications in cases of impetigo (p. 226), and the same may be said with regard to eczematous eruptions. Combined with an appropriate constitutional treatment, he recommends externally tepid water, decoction of bran, or the following lotion:—℞ *Acid Hydrocyanici diluti*, ʒiij; *Sp. Vini Rect.* ʒv; *Aquæ Rosæ* ʒvij. *M.* We think that keeping the parts moist is an essential part of the local treatment, which ought never to be neglected. The *linimentum aquæ calcis* is a favourite remedy; but we have only lately seen a case of sub-acute eczema of the legs which withstood this liniment for between two and three weeks, while the smartings and itchings were not relieved, and which was immediately alleviated and rapidly cured by the continuous alkaline lotion. Indeed, we are inclined to think that no kind of greasy application is useful in these cases—a circumstance which renders their accurate diagnosis the more imperative.

In psoriasis, the author advises a constitutional treatment, combined with the tepid bath. In none of the varieties of the disease has he seen much advantage from topical applications (p. 294). In this, our experience differs from that of the author; as in many of these disorders, as well as in lepra (in which we observe it is recommended), the tar ointment has always appeared to us the most valuable remedy we possess. The great objection to it is undoubtedly the uncleanly nature of the treatment, and perhaps it would be well to give the internal treatment by pitch, either in the form of pills or infusion, a further trial. This we propose doing. The good effects of arsenic are now so universally acknowledged as to require no comment. Nothing is added to our knowledge of the treatment of pityriasis and icthyosis.

One of the most obstinate diseases we are called upon to treat is undoubtedly lupus. Dr Parkes informs us that with regard to this disease Dr Thomson was extremely successful in temporarily and sometimes even permanently, arresting its ravages. Local applications were sparingly used, but reliance was chiefly placed on three medicines, viz.,—iron, iodine, and arsenic, which were generally used at the same time. Mercury, in the form usually of biniodide, was also often given. Cod liver oil was frequently used, and quinine and other tonics were occasionally employed. When a patient first came under treatment, if the general health had been at all lowered, and if there were any anæmic symptoms, cod liver

oil and iron, especially the iodide, were given for ten or fourteen days,—the diet being nutritious, and other usual means adopted to improve the tone of the system. Afterwards, the alteratives were commenced. The biniodide of arsenic was given in doses of from one-twelfth to one-sixth, or even one-fourth of a grain. In such large doses, however, it often produces gastrodynia, and when this has once occurred, the stomach is extremely intolerant afterwards of the smallest dose. Therefore, as the utility of the medicine can only be looked for after some considerable period, Dr Thomson considered it safest to keep to the smaller doses, and to continue them regularly for a long time. If any gastrodynia came on, the medicine was at once left off, and opium and hydrocyanic acid administered. The cod liver oil and the iron were sometimes continued with the biniodide. Conium was found useful if there were pains in the tubercles; and even if not, appeared sometimes to have a good effect; so that it was frequently combined with the biniodide in the form of pill. If the biniodide could not be borne in any form, the liquor arsenicalis of the London Pharmacopæia, in small doses of from four to eight minims, was given, and iodine rubbed in over the healthy skin with a view to promote absorption (p. 310). We have given Dr Thomson's treatment of this disease more at length on account of the incurability of the disease, than from any confident hope of its proving ultimately successful.

Acne in the adult is certainly another very obstinate disease, and we turned to the treatment of this disease in the hope of finding some useful suggestions of a novel character, but were disappointed. In the syphilitic eruptions, mercury and iodine are the remedies principally depended on; but here, also, we find nothing that need detain us.

We can confidently recommend Dr Thomson's posthumous work to the perusal of practitioners, perfectly satisfied that, when treating many of the harassing and obstinate diseases of the skin which they may meet with, it will furnish them with valuable hints and suggestions, that none but one who has had great experience in their management could have supplied.

The Diseases of the Breast, and their Treatment. By JOHN BIRKETT, Assistant-Surgeon to Guy's Hospital, &c. London: 1850. Pp. 264.

THIS treatise, although only just published, is said to be the dissertation to which the Jacksonian prize for 1848 was awarded by the Council of the Royal College of Surgeons of England. We say *said to be*, because, during the two years which have elapsed since the prize was obtained, we cannot suppose that the author

has been idle, and neither subtracted from or added to his original essay. As at present published, it is an attempt to systematise all the known diseases and abnormal conditions of the breast, and, as may easily be supposed, the execution falls far short of the comprehensive plan, several of the sections being very cursorily noticed, and containing no additions whatever to our existing stock of knowledge. The author seems to have laboured throughout under an excessive fear of being tedious. He apologises for it in the preface, and every now and then hints at the impossibility of entering into this or that subject for want of room, length of the essay, &c. &c., as if he were bound down to complete his subject within so many given pages. In fact, numerous important points are dismissed with a most unsatisfactory and even startling brevity, while at the same time a degree of authority and knowledge are assumed to which we are little accustomed. Short scraps like the following are common :—

"*The Duration of the Cyst.*—The cyst may continue unchanged for a long time.—[What information this sentence is intended to convey we are at a loss to imagine.]

"*The Axillary Glands.*—The lymphatic glands in the axilla are unaffected, even in some cases in which the integuments are ulcerated.

"*The Treatment.*— The removal of a part, or of the entire gland, is demanded.

"*The Progress.*—A similar disease may return after two years, the sufferer being pregnant in the interval. But this disease never contaminates the tissues around, and is perfectly harmless.

"*Result.*—The result of excision is favourable."—Pp. 92, 93.

At page 220, we find the following sentence :—

"*Diathesis and Temperament.*—Individuals of a strumous diathesis are most liable to cancer,—that is, I believe that in them cacoplastic deposits are more readily developed, and hence their more frequent occurrence. But it is quite impossible to state, with any degree of accuracy, what appearance of the woman betokens a liability to the complaint. Dark and fair,—florid and pallid,—the corpulent and the thin,—the apparently healthy, and the decidedly unhealthy,—all present instances equally severe and alike uncontrollable. Who, then, are exempt ? In the present state of science,—the truth must be told,—the surgeon cannot say."

No series of statements could be made more opposed to our impression of what really exists than is here given, and yet the author has not thought it worth while to favour us with any facts or arguments in their support. We have no doubt that the statement he makes in the preface is quite correct,—namely, that opportunities on a large scale have occurred to him, through the kindness of many friends, and his connection with Guy's Hospital, for examining as minutely as possible morbid growths. But what science at present requires is not imperfect systematic works, made up from the limited researches of individuals, but accurate and well observed facts, which may hereafter serve as a foundation for a wide and perfect generalisation. It is our opinion, that, at present, all sys-

tems are premature, and that works of this character are only valuable from the well-recorded observations they contain. It is in this point of view that we can confidently recommend Mr Birkett's publication, containing, as it does, amidst a large number of quoted cases, some very valuable original ones, together with good histories of special lesions, well illustrated by drawings. His accounts of cystic diseases and of hypertrophy of the breast are particularly valuable.

Throughout the work there are scattered some very useful practical hints, which will recommend the book to surgeons. Of these we have only room for two examples :—

"Most of the ill effects resulting from abscess—such as their consecutive formation and sinuses—occur for want of proper local support; and, therefore, in all cases in which the mischief appears to be deep, this plan should be adopted. I can testify to the advantage accruing from its application; and others, who have pursued the practice extensively, are equally ready to give it their support. Nor are poultices to be long continued, but only a small piece of wet lint is to be placed over the wound. Even in the first stage of the attack, before there is any suppuration, strapping the breast up with plaster, and then bandaging it, prevents further mischief."—P. 50.

"*Of the Incisions and Dressings.*—In all operations upon the mammary gland, for the removal of new growths, the direction of the incisions is a matter of some importance.

"As a general rule, the long axis of the wound should incline as much as possible downwards and outwards when the patient is in a recumbent position.

"The edges of the wound should also be adapted so as to prevent any tension upon the newly cicatrised parts.

"When any tumours of an innocent nature are removed from the gland, the incision should be made in the direction of a line radiating from the nipple as from a centre ; for, by so doing, a transverse division of several ducts may be avoided.

"As regards dressing the wound, the edges are to be well adjusted as much as possible by position, and which may often be accomplished by placing the arm of the same side across the lower part of the chest. By this means very little strapping is required ; for it is a great object to leave the extremities of the wound open. A bandage should never be placed *around* the chest of the patient ; for it generally gives rise to much annoyance, and as evening advances requires removal. After the vessels are secured by ligature, a fold of lint, wetted with cold water, should be placed over the wound, and this kept cool by the application from time to time of a second piece, also wetted, which may be removed and reapplied after immersion in fresh water.

"Unless secondary hemorrhage be profuse, pressure, with cold, will generally suffice to arrest it. Very gentle equable pressure may be kept up by a small flat sand-bag placed upon the flaps. The pressure of this, while it serves to adapt the divided surfaces, at the same time restrains bleeding."—Pp. 239, 240.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR ALISON.

NOTICE OF CASES OF PLEURISY AND PNEUMONIA IN THE CLINICAL WARDS OF THE ROYAL INFIRMARY IN SUMMER 1850,—BEING THE SUBSTANCE OF TWO CLINICAL LECTURES, DELIVERED ON 7TH MAY AND 18TH JUNE.

Lecture I.—7th May.

The most remarkable peculiarity which I observe in taking charge of the clinical wards in the beginning of this summer course is the large proportion of cases of well-marked pleurisy and pneumonia, left in the wards by Dr Christison,—no less than eleven, in forty beds. This is certainly much more than is usual in the practice of the hospital. I remember being surprised, long ago when a student here, by an observation of Dr Home, senior, in his clinical experiments, that the true pneumonia (under which head he included pleurisy) is a rare disease in this hospital. This, however, agreed with my own observation when a student, and most generally since I have acted as physician here, although in some seasons we have seen much more of such inflammations than in others. In general, I have seen, or at least distinctly recognised, a much larger proportion of such cases among the out-patients of the dispensary,—seen *early* after their complaints have begun,—than in patients brought to the hospital. But at present there can be no doubt that the cases of pleurisy or pneumonia, or the (very common) combination of the two, form the majority of the pectoral complaints that you see in the wards; and, whatever be the cause of this unusual number, you have a remarkably good opportunity of studying the phenomena and progress of these complaints, and may be aided in that study more by some general observations on this class of cases, illustrating the bedside reports, then by detailing to you individual cases.

In regard to the indications by which these have been recognised (before I took charge), and are still unequivocally marked, although the inflammatory symptoms have subsided in almost all, it is worth while to fix attention on those that are most practically important—*i. e.*, those which are most uniform in their occurrence and most easily distinguished, and which, therefore,—when taken in connection with more or less of the general symptoms of fever, cough, dyspnœa, and pain of chest,—enable us most confidently to pronounce on the texture within the chest which has been affected with inflammation,—a precision of diagnosis now very generally attainable, and contrasting strongly with the candid admission of Cullen, that, in his time, diagnostic marks to distinguish the bronchitis, peripneumony, and pleurisy from each other, or even from pericarditis or carditis, during life, were not to be depended on.[1]

For this purpose we trust chiefly to what are now generally, although somewhat incorrectly, termed the *physical* symptoms of chest complaints—*i. e.*, those observed by *auscultation*, *percussion*, and *manual examination* of the thorax. Those by which pleurisy may be distinguished, generally within a very few days after the inflammation has begun, are, perhaps, the simplest and most easily recognised; particularly because, in almost every such case, the disease being confined to one side of the chest, we have the opportunity of making

[1] First Lines, sec. 334.

comparative observations on the other; but even among them we can point out some as by much the most uniform and distinct, and, therefore, the most practically important. A young man is apt to fancy that he has acquired the most important information of this kind, when he has heard and learnt to distinguish the greatest variety of morbid sounds; but when he goes into practice he may be disappointed on finding, that some of those on which he has bestowed the most pains, seldom present themselves to his observation, and that, when he can hear them, he can draw no practical inference from them. What is most desirable is, that young men during their studies should familiarise themselves with the symptoms of this class, which are of most uniform occurrence, and form a judgment of their true import and comparative value.

In the case of pleurisy, there are three symptoms of this kind, easily observed in almost all those patients, and worth all others put together, as indicating that effusion or exudation exists in the cavity of the chest, and, if preceded or attended by febrile and inflammatory symptoms, that inflammatory exudation exists there. These are the *dulness on percussion* (greater when resulting from pleurisy than is ever observed from condensation of the lung without effusion into the sac of the pleura); *imperfect expansion* of that side of the chest on inspiration; and *suppression of the respiratory murmur*, excepting in the neighbourhood of the spine, where it is to be expected to be *tubular or bronchial*. There are three others, very distinct and satisfactory in some cases, and noted in several of those in the wards, but much less uniformly observed, requiring more peculiar conditions for their manifestation, and therefore less generally useful—viz., the modification of resonance of voice, termed *ægophony*, depending on the presence of a thin film of semi-fluid effusion between the layers of the pleura; the *friction sound* attending the acts of inspiration, and depending on the pleura pulmonalis and costalis, roughened by partial solid effusion, rubbing on one another; and the *absence of the vocal thrill*, felt by the hand applied to the chest when the patient speaks or coughs. Of these three, the last appears to me the most constant, and therefore the most valuable, particularly when going along with the dulness on percussion; as characterising the case of *fluid* effusion, separating the pleura pulmonalis and costalis at the part, and distinguishing it from the case of adhesion of these two membranes by *solid* lymph, over a condensed portion of lung;—in which case the dulness on percussion is attended with an *increase* of the vocal thrill, often very obvious in the upper part of the chest in advanced phthisis, as in several phthisical cases now in the wards. The enlargement of the side of the chest, or bulging of the intercostal spaces,—more or less general, according to the extent of the effusion,—is often very obvious and satisfactory in the advanced stage of the disease—at least of one of its forms; but is not to be looked for in the early stage, which, in all inflammatory diseases, is practically the important stage.

As to the special indications of peripneumony, whether with or without attending pleurisy, I think you will find the most important and practically useful to be that given by the *sputa—i. e.*, the appearance of tenacious and at first translucent sputa, thoroughly *stained* with blood, and thereby acquiring a considerable variety of colours in different cases, the reasons of which variety I do not pretend to know,—rusty, pink, purple, dark yellow or greenish,—all to be regarded as inflammatory; but the darker coloured, or more opaque, being generally the indication of the more advanced stage of inflammation. The *crepitous râle* is in some cases very distinct and satisfactory, but in many it is unperceived, or if perceived is faint and of short duration in the early stage of the disease, while it may be of long standing in the advanced stage, and during the convalescence; and in some it is not easily distinguished from the somewhat coarser sound of the subcrepitous râle, which may depend on conditions in the lungs, or minute branches of the bronchial, very different from inflammation. On these accounts this symptom has never seemed to me

to realize the very natural hopes of Laennec, as to its practical usefulness in detecting pneumonia, when not otherwise indicated.

The other indications that we have of peripneumony by auscultation and percussion—dulness on percussion, bronchial sound, respiration, or its complete suppression, and imperfect expansion of the affected side—are rather, in fact, what may be expected, and can hardly be distinguished from the effects of attendant pleurisy, so frequently complicating that disease.

In regard to all those physical or local symptoms of peripneumony, this observation, I am sure you will find practically important, that although we may rely much on them, particularly when grouped together, and watched in their progress, as indicating the *nature* of the disease, we cannot rely on them at all as indicating its *intensity*, its *probable tendency*, or the *extent* to which antiphlogistic remedies during it are demanded, or can be safely employed.

Take, for example, the pneumonic sputa,—they may be seen in full perfection in cases which are partial and slight, or which are attended with extreme debility, demanding stimulants; and may be absent in cases which are extensive and urgent, and will bear full evacuation. I remember having two young men—both previously in good health, and seen early—under my care at the same time, in pneumonia;—in one of whom we had the translucent sputa almost from the first, exactly resembling red currant jelly, but he was so feeble that he became faint the first time I set him up to examine his chest; and, although I thought it right to take some blood from him, he did not lose ten ounces, generally or locally. In the other, we never saw any tinge of blood in the sputa, but the other symptoms were well marked and urgent, and he bore four full general bleedings perfectly well; the blood was sizy, and the symptoms were relieved as usual after each bleeding. Both these patients recovered perfectly well; and, judging by the progress of the cases,—especially as regarded the state of the vital functions, the pulse, and breathing,—we had no reason to regret, either that the first was so little, or the second so fully bled; but if we had trusted to the sputa as indicating the nature and intensity of the disease, we should have thought that the first demanded the full antiphlogistic treatment, and the last little or none. And I have seen several, in which the peripneumonic sputa were peculiarly well marked, which I have thought it necessary to treat, from the time I first saw them (late in the disease) by stimuli only, and which have recovered remarkably well.

Again, as to the crepitous râle, I have seen many cases which seemed to me well marked by all the other symptoms, and which have done well, generally under the antiphlogistic treatment, in which I never could detect that symptom; and others, in which I was quite satisfied of its existence and extension through the lungs, while the other symptoms have either continued so mild, or been of such a nature, that no active antiphlogistic practice has appeared to be demanded or justified by that extension, and the whole symptoms have subsided without them. Particularly since I read the statements of Louis, as to the extension of the crepitous râle through the lungs, notwithstanding full bleeding, by which the pulse had been brought down,—from which he inferred that the idea of the power of blood-letting over inflammation had been a mistake,—I have attended to this point, and satisfied myself that he was quite correct as to the fact, but wrong as to the inference—I mean as a general principle; because, in these circumstances, when the pulse and breathing have decidedly improved, although the râle may indicate that the inflammation continues and extends for a time, we may be very confident that it will subside without further treatment. The reason probably is, that the inflammatory exudation, although extending, is modified in character, and so attenuated as to be easily re-absorbed,—so that no complete obstruction, or permanent disorganisation of the pulmonary substance, to any injurious extent, is effected. In these circumstances, all that is required is, to watch the patient carefully, and revert immediately to the bleeding, or other

antiphlogistic treatment, if the breathing becomes again more oppressed, and the pulse more frequent or *firmer;* under the conviction, given by experience, that the crepitous râle extending through the lungs in a case of recent illness, and in connection with a frequent and full pulse, and a laboured state of the breathing, implies much danger, which may be averted by such treatment; but that the same râle extending through the lungs, in connection with a more natural pulse, and an easy state of the breathing, or with dyspnœa, but a feeble or soft pulse, and damp livid skin, either implies no danger, and demands no active treatment, or is to be counteracted by very different remedies. And I may add, that this is exactly the line of conduct which our predecessors would have advised in those circumstances, before the crepitous râle was heard of, although they could not have spoken with so much precision as we can, of the nature of the changes then going on in the lungs.

I remember a case of a gentleman advanced in life, whom I saw some years ago, affected with urgent pneumonia, who was bled seven times, and the last bleeding seemed to me as clearly demanded (as indeed it was anxiously wished for by the patient), and as clearly effectual in relieving the dyspnœa, as the first; but the crepitous râle distinctly extended itself through the whole of the affected lung, and maintained itself, becoming only very gradually coarser, for a fortnight thereafter,—the convalescence going on, in all other respects, perfectly well. Last year I saw this gentleman in another attack of pneumonia, with the febrile symptoms and the characteristic sputa, and found the crepitous râle re-established over the greater part of the same lung as formerly; but the general symptoms were on this occasion, at least from the time I saw him, so typhoid, and the pulse so soft, that it was thought best, not only to dispense with bleeding entirely, but after a short time to give stimulants (of course carefully watching their effects) as in typhoid fever; I had the pleasure of seeing all the symptoms again gradually subside under that treatment, and that gentleman, although now past 70, is again in the habit of walking several miles daily. Now in that case, if we had regarded the crepitous râle as an index to the intensity or danger of the inflammation, or as a warrant for blood-letting, we should have thought active treatment required in the convalescence from the first attack, when no remedies were really required, and when it would pretty certainly have been dangerous; and should have resorted to bleeding in the last attack, when it would, to say the least, have been unnecessary. But all this does not prove, either that the bleedings, which appeared so signally useful in the early part of the first attack, were not really useful, or that the inflammation might not have been most beneficially combated by that remedy in the second attack likewise, provided it had been used within the first 36 or 48 hours. And in drawing any such negative inference, we should have opposed ourselves, without sufficient reason, to the belief as to the power of blood-letting over the early stage of inflammation, which has prevailed from the earliest age of medicine, and which is confirmed to most of us by numerous cases, carefully watched, and even by experience in our own persons.

If we regard the different indications that we now have—not strictly speaking of inflammation, but of *inflammatory effusions* in the chest—as I think we ought to do, as important evidence of the nature, but not of the degree of intensity, nor of the stage, nor proper treatment, of the diseased action there existing, we shall have less difficulty than we otherwise might have had, in reconciling such facts as we have now before us, with the general principles as to inflammatory diseases, which have been handed down to us from our predecessors, and on which we still confidently rely, although aware that they require modification and limitation from the progress of knowledge.

Thus, you may think it strange, that we should have so many unequivocal examples of these inflammatory diseases, without a single opportunity, in my colleague's practice or my own, of seeing the effect of general bleeding—which we nevertheless both regard, now as formerly, as being the most powerful remedy in this disease, at least in the form of it most generally described, and

with which we are all familiar ; and this consideration suggests several reflections which I think important.

1. You will observe that several of these cases, known to us with almost absolute certainty as effusions consequent on inflammation in the chest, would not have been recognised in former times at all, under their true denomination ; and this no doubt is part of the reason why these inflammations were thought so rare. Some of the cases now before us would have been called catarrh, some pleurodyne or rheumatic stitch, some incipient phthisis, some would have had no more definite name assigned to them than broken constitutions, threatened with hydrothorax ; or if, by tracing the whole history of the cases, their real nature had been made known, most of them would have been termed, and indeed correctly termed, the *sequelæ* of pleurisy or pneumonia, not the inflammations themselves. They would, therefore, have been regarded as, either by their nature or the stage at which they were seen, distinct from cases of true inflammation within the chest, and would not have been thought to demand any such active practice as is usually associated with those names. Nor would the uncertainty which undoubtedly would have then existed as to their real nature, have had any such injurious practical effects as you might suppose, because these patients are all now in such an advanced stage of disease as admits, and would have been long ago known to admit, of no active depleting practice ; and the chief benefit to be expected in them, must be from the antiphlogistic regimen,—particularly as regards rest, avoiding excitement, exertion, exposure to cold or wet, intemperance, and all other irritations, and from the use of a diet suited to their condition and the stage of their diseases,—and so giving fair play to the provisions of nature for the restoration of health ; all which would have been enjoined by a prudent practitioner, notwithstanding his uncertainty as to the exact nature of their complaints. The more accurate discrimination of such cases, which has been attained of late years, is satisfactory to our minds, and is often valuable with a view to prognosis, and occasionally it suggests useful practice, but is not so frequently available for the good of patients as you might suppose.

2. But you may further very naturally ask, whatever be the stage of disease in which those cases come under observation, if it be admitted, as I believe it may, that they have all been truly inflammatory in their commencement, how comes it, that none of them have been bled at the arm, either out of the hospital or in it, and yet almost all appear in a fair way of recovery ? There is a poor woman, M'Govan, brought several miles from the country some days ago, in the seventh day of disease, which seems to have been attended from the first with typhoid symptoms, as she was sent in as a case of fever, but whose case I regard as simply pneumonia, affecting almost the whole of the left lung, exhibiting all the usual local symptoms, with a pulse from 120 to 140, always soft and feeble, a cool skin, clammy sweats, livid lips, a dry tongue, and such marks of debility attending the dyspnœa as I have never seen recovered from, at this stage of the disease, under any other treatment than that which we have here adopted, viz., the strictly stimulating. This case we must expect to be fatal,[1] but all the other ten may be expected to recover, with some injury to the play of the lungs, and therefore shortness of breath, which will gradually diminish if there is no fresh attack,—although none of them have been bled. Such observations have been repeatedly made of late years, and have been thought by some to justify the conclusions : that these inflammatory diseases are not so dangerous as they have been represented; and that the active practice recommended for them in systematic works is quite unnecessary. These are points deserving grave consideration. Certainly it must be admitted, that since we have had the means of detecting effusions consequent on inflammation in the different parts of the chest,

[1] This woman died on the fifteenth day of her disease, but unfortunately permission to examine the body was refused.

we have recognised inflammations there more frequently, and in cases apparently requiring less active treatment, and doing well under a less active treatment, than we could have expected; and in many such cases we can satisfactorily observe—and I have no doubt shall be able in some of these cases in the wards to observe—the important and essential difference between inflammatory exudation, and those which constitute other organic diseases, tubercular, or fatty, or granular, or malignant deposits,—viz. that the former, when the inflammation has subsided, are so much more liable to absorption, than any of the latter,—that being the main agent in the natural cure of all lesions of structure produced by inflammation, such as you will here have the opportunity of observing, under which an organ is indeed seldom restored to its perfectly natural condition, but is very often permitted to resume the natural exercise of its functions in ordinary circumstances.

On this point—the efficacy of the provisions of nature for the cure of inflammatory disease—we have important facts in the experience of *homœopathic* practitioners,—for I presume we are agreed on this, that the treatment by infinitisimally small doses of a variety of medicines, all thought to be specifics, is in fact treatment by *placebos*, or is another name for what is usually called the *expectant* practice, admitted by all to be, in *some* circumstances, the best of all kinds of practice. We must admit, I think, that this practice has appeared, on fair inquiry, to be more frequently successful in inflammatory diseases than could have been expected—*i. e.*, the practitioners who have thought themselves justified, by that theory, in trusting more than we do to the provisions of nature, aided only by regimen, for the cure of such diseases, have had fewer deaths and better recoveries than we should have expected.

One reason for this you can anticipate from what I have already said. This theory has come into vogue only since the means of detecting the local indications of inflammation within the chest have been so much improved, as to make us sure of its existence, and of its being of some extent, in many cases where our predecessors would have overlooked, or could only have suspected it. In fact, you will remember that the terms pleurisy, pneumonia, &c., as defined by Cullen and others, have a *nosological*, not a strictly *pathological*, meaning; they are not applicable strictly to inflammatory effusions, but to those effusions taking place in connection with a certain group of symptoms,—pain, deranged function of the affected parts, and inflammatory fever. We now know that exactly similar effusions or exudations take place in what we call the more chronic or even latent forms of the disease (most common, I believe, in persons of the lower ranks of life, in whom all vital action is somewhat languid), with slight and insidious symptoms in their commencement; and that they take place also, in some cases (especially, as some have stated, in the upper part of the lungs), with a febrile re-action of the typhoid rather than the inflammatory character; and, of course, it is only by experience we can learn how far the antiphlogistic treatment—judged to be so useful and important in the strictly acute cases attended with inflammatory fever—is applicable to those more obscure and insidious forms of the same diseases.

Such cases as we have before us are not so important as you might at first suppose, in enabling us to judge of this question. Certainly, you are not entitled to infer, from such observations as these, that blood-letting has not the power which is usually assigned to it, of checking inflammation in its early stage, simply because you have not seen these cases in their early stage, and cannot, therefore, tell how far they could then have been recognised by their symptoms, nor what would have been the effects of any remedies on them. The first inference which I think it most important to impress on you is, that hospital practice seldom gives a fair opportunity of judging of the power of such remedies over inflammation; and this is quite in accordance with what I have very frequently observed formerly, particularly in comparing hospital and dispensary practice. I have little doubt that if all these eleven cases had been seen within the first forty-eight hours of their existence, some of them at least might have

been recognised, and if assiduously treated by general and local bleeding might have been so far controlled, that the effusions we now perceive might have been either altogether prevented, or so modified as to be hardly perceptible, and the patients have been restored to health by this time.

I have often quoted, in illustration of this remark on hospital practice, a case narrated by the late Dr Jackson, a young American physician, who was a very zealous and diligent student at one of the hospitals at Paris, and on which he dwelt with delight, as "the most superb he had ever seen." The case was one illustrating merely what most of us regard in this country as one of the simplest and most elementary principles in medicine—the power of blood-letting over incipient pneumonia; but Dr Jackson says, that although it was the twenty-first case of pneumonia that he had watched with the utmost care in that hospital, it was the first in which he had seen the power of this remedy distinctly exemplified; all the symptoms, local as well as general, although quite distinct and following their usual course, having in this case disappeared by the sixth day; but then he adds, that it was the first case he had seen, in which the treatment was begun within the first twenty-four hours. The inference from such a fact, as to this important deficiency in the practical information to be derived from hospital practice, is well worth your consideration.

But this leads me to take notice of another statement made by the homœopathic practitioners, which is certainly more staggering, viz., that under their plan of treatment the convalescence from inflammatory diseases is more rapid than under the depleting plan. This, they say, they can show statistically; and although I place little reliance on statistics in such an inquiry (on account of the numerous fallacies to which they are liable), yet I know that this observation has been made by intelligent and unprejudiced witnesses of this treatment—*e. g.*, by Dr Balfour, who saw it in Vienna; and a little reflection will enable us so far to understand it. For all must allow that, after full and repeated bleedings, whether for inflammatory or other diseases, convalescence is necessarily slow; and farther, that relapse is easily produced, inflammation being always easily excited in a weakened constitution.

But then you will observe, that a rapid recovery, particularly as regards the general symptoms of disease, up to a certain point of *general* health and strength, is not necessarily a perfect recovery; and that a slow convalescence, although attended with the inconvenience I have mentioned, may in many cases be the most complete, as in some, we are convinced, that it is the only possible kind of convalescence, of which the case admits. What we apprehend from a pneumonia, met by what we believe to be inadequate treatment, is, not pure exhaustion of the vital power, but such an amount of inflammatory effusion as may either cause rapid death by asphyxia, or lay the foundation of chronic and incurable organic lesion of the lungs, the precise nature of which may vary according to the constitution of patients. This local injury consequent on inflammation is quite compatible with a rapid recovery of a certain degree of health and strength—such as to enable a patient, for example, willingly to leave an hospital. The only perfectly fair way of determining this question by statistics would be, to have a certain number of patients treated from the beginning, in inflammatory diseases, by practitioners accustomed to the use of blood-letting in such diseases, and with the symptoms which they have been accustomed to regard as indicating it, by the expectant practice only; and then an equal number treated by the same practitioners, likewise from the commencement, by the depleting remedies; and farther, we must have perfect confidence, if not in the judgment, at least in the good faith, of the practitioners using (and not abusing) these active remedies. To such a trial I shall only say, that I should not wish myself, nor advise any of my friends, to be a party; and the moral reasons which make me say so, must, I think, for ever prevent any such statistical experiment being really tried by any trust-worthy practitioner.

What I have stated is enough to show that some of these cases, occurring in

former times, would have been differently named, although not, as to essentials, differently treated; and this, I think, may go far to explain the fact which I stated as presenting a difficulty,—the number of cases of thoracic inflammation now going on favourably, without affording you an illustration of the use of general blood-letting. But I must confess, that I do not think this explanation sufficient. I am strongly inclined to believe, from reflection on many other cases as well as these, that there has been a gradual change in the usual form and character of those inflammations, as occurring in the inhabitants of this country—and that, even although seen from the earliest period at which they can be recognised, they do not, in general, present the same intensity of local symptoms, nor the same amount of febrile re-action, as used to attend similar diseased actions in the same parts in former times; that the fever attending them is more frequently either slight, or in several respects of a truly typhoid character; and therefore, that they do not furnish the same indications for blood-letting, and do not admit of the same amount of depletion, without dangerous depression of strength.

On this point I do not speak with absolute certainty, but I can give you what I may call statistical evidence on it; for it not only has appeared to myself that there is a change in the usual character of inflammatory diseases within my recollection, but I have put the question to many experienced practitioners in different parts of Scotland and the north of England, and in every instance but one I have been assured, that the result of their observation agreed with my own, and that, judging by the same rules as formerly as to the cases requiring it, they have much fewer occasions to use the lancet for inflammatory complaints now, than they had thirty or even twenty years ago,—or than might be expected from the writings of Cullen, or even of Abercrombie, or from the well-remembered precepts of Gregory. Of course it may be said, that this is only to be ascribed to the gradually increasing caution of advancing years; but when I add, that the faith of all the gentlemen to whom I allude, in blood-letting for the cure of well marked and violent inflammation, is unshaken, and as they believe confirmed by experience, and that they think, as I do, that cases still occasionally present themselves in which it appears as distinctly demanded, and as clearly useful, as formerly; but that, in general, the symptoms on which we rely, as guiding us in its use, are either so slight as not to demand it, or of such a character as to limit or even contra-indicate it, before it has been carried to any considerable extent,—you may probably think this general inference from extensive experience deserving of attention.

We have instances before us which illustrate what I now say. Take, for example, the case of Michael Joyce, a lad of 18, of rather full habit, admitted into No. 1 the day before I took charge. We have evidence in him, certainly of pleurisy, probably of pleuro-peripneumony, affecting at the time of admission nearly half the right side of his chest, and the *lower* half, *i. e.* the part most liable to acute inflammation. We have dulness on percussion, impaired expansion, bronchial or suppressed respiration, friction sound, and muco-purulent sputa; and we know that up to the fifth day before admission he had been working in the fields, and then been disabled by rigors, dyspnœa, and pain of side. I am confident that, in former times, from this history and these local symptoms, we should have safely anticipated acute inflammatory fever, and an opportunity for full blood-letting; unless, indeed, the disease had made too much progress to admit of it; but in this case we have had the pulse 70 on admission, never above 80, always soft, the skin always cool, the pain and dyspnœa never urgent. He had twelve leeches and a blister, ordered by Dr Christison, with such relief that I have only thought it necessary to order eight more leeches, and already we have indications of beginning re-absorption of the effusion.[1]

[1] This lad had two returns of pain of side, with slight febrile symptoms, and extension of the marks of pleuritic effusion over nearly the whole of that side, for which

You are aware that Sydenham and other authors have written much on what they term the atmospheric constitution of different seasons, not only as affecting epidemics, but as modifying the usual character of sporadic diseases; and although there are many fanciful notions and minute distinctions, not now to be recognised, in those writings, I believe all reflecting practitioners of some standing will allow, that there is a foundation in nature for such observations. And I beg you to observe, that although Sydenham speaks of atmospheric constitution, he expressly says that he cannot ascribe these varieties of disease to *known* qualities of the atmosphere, having observed the same characters of certain diseases to prevail throughout whole years, and when the temperature varied from intense heat to the greatest cold within the memory of man. Therefore, he says, these varieties of disease must be ascribed to "*occult* qualities of the atmosphere,"[1] which is the same thing as saying, that he did not know what produces them; and I apprehend we must acquiesce in this conclusion likewise. But although we cannot assign the cause, we are quite certain of the fact, of such variations in the usual character of the *epidemic* diseases—as I believe every practitioner is aware, who saw as much as I did of the epidemic fevers prevailing here between 1815 and 1825, and can compare his recollection of those fevers with observations of those which have constituted the three great epidemics that have since prevailed here—commencing in 1826, 1836, and 1847; the typhoid character of all these epidemics contrasting strongly with the inflammatory character, and the local inflammatory symptoms which constituted the chief danger of the fevers of the preceding ten or fifteen years—such as those described by the late Dr Duncan and Dr Welsh in this town, and by Dr Bateman in London. And I may add to this another observation, of which I am equally certain, and which I have long thought important, as unequivocally showing that such variations in epidemics are not to be explained by season or sensible variations of the atmosphere—viz., that the inflammatory epidemic of continued fever observed from 1815 to 1818, was coexistent with a more typhoid form of measles than we have ever seen since; —the eruption in the bad cases of that epidemic having been late, livid, partial, rapidly fading, and attended and followed by stupor, retching, dry tongue, &c.—such cases being easily depressed by evacuations, and obviously benefited by stimulants, and, when the dyspnœa was not urgent, very often recovering under their use.

Now, if epidemic diseases are thus subject to an influence, unknown to us except by its effects, which modifies their symptoms and tendency so remarkably at different times, there is nothing unwarrantable in the supposition that the usual tendency of *sporadic* diseases likewise,—and particularly the febrile reaction excited by those local changes to which we give the name of inflammation,—are liable to similar variations, demanding much modification of the treatment; and I need not say, that if this principle is true, there is no other more worthy of your attention and recollection.

Next, as to the practice to be employed, in addition to the antiphlogistic regimen, in cases of those inflammations which, either on account of the stage in which they are seen, or the general symptoms they present, are not judged proper for general blood-letting, I need hardly say that local bleedings and blisters, or other counter-irritants, are the next remedies to be thought of; and you have had, and probably will have, various opportunities of observing their good effects. But it is easy to perceive that both may be too much relied on, or carried too far,—the former because they may so far depress the strength, and the latter because they may cause so much pain and irritation, as to interrupt and retard, rather than promote, the gradual progress of the natural

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one leeching and two blisters were again directed; but the pulse never reached 84, and was never full or firm; and he was free from complaint, and almost free from indications of any disease in the chest before the 15th of June.

[1] 1st Epistle on Epidemic Diseases, § 5.

changes on which recovery in these cases must mainly depend. The same observation applies to the antimonial solution, which, given in the manner recommended by Laennec, *i.e.*, *altera quaque hora* (although in much smaller doses), I have been accustomed to regard as the most powerful and manageable auxiliary to, and sometimes substitute for, the loss of blood in these inflammations,—and even to the still smaller doses of antimony or ipecacuan with morphia, which are usually the most effectual palliatives for the irritating cough, but retard the recovery of the stomach, in these complaints.

The next question is as to the use of mercury, most generally in the form of calomel and opium, which many practitioners regard as of signal use in pneumonia, and more especially in pleurisy. I must confess that I have so often given this, or seen it given, and watched its effect as carefully as I could, at the time when the mouth became affected (which is the only time when you can be sure of distinguishing the specific effect of this remedy), without being able to perceive any advantage from it,—nay, in some cases where the irritation excited by it has seemed to me manifestly hurtful,—that I have no such faith in it as many profess, and think it necessary to be cautious in using it, and watch carefully for any of its untoward effects,—particularly in hospital patients, often of feeble constitution, and soon to be exposed again to cold and hardships. But in the pleuritic cases, when there is no peculiarity of constitution to contraindicate it, I generally think it right to make a cautious trial of it.

In the cases of peripneumony in the advanced stage, or with the typhoid symptoms well marked and urgent, I am certain that by far the most important remedies are the stimulants, in small doses, carefully watched and gradually increased,—wine, brandy, and different preparations of ammonia and æther. This kind of practice is more frequently required in the advanced stage of mere bronchitis, both acute and chronic; but I have seen several of peripneumony, in which it seemed as clearly demanded, and as decidedly useful, as in any case either of bronchitis, or even of typhus itself, in which I have ever used it; and although the case of M'Govan seems too far advanced,—*i. e.*, the effusion probably too great, and the febrile symptoms attending it of too intense a character, to be benefited by this or any other remedy,—yet it is very possible that we may have other opportunities of witnessing its successful employment.

Lecture II.—18*th June.*

You have had opportunities, in the last six weeks, of watching the progress, not only of the cases of pleurisy and pneumonia formerly mentioned (all of which, except the fatal one of M'Govan, have gone on favourably), but likewise of two cases of well marked pleurisy—sub-acute and chronic, and one case of well marked pleuro-peripneumony—which have been admitted since; besides several cases of decided phthisis, and several of bronchitis with asthma, the diagnosis of which has been easy, excepting one case of that kind (Mrs Kerr), in which there has been pretty violent hæmoptysis, and strong suspicion of a phthisical cavity in the upper part of the left lung. I say suspicion, because there is no doubt of general and habitual bronchitis with asthmatic paroxysms in this case, and where these exist it is not uncommon, in the lower ranks, to have repeated attacks of hæmoptysis which never pass into phthisis,—unconnected therefore, I presume, with tubercular deposits, but probably connected with apoplexy of the lungs, as in many cases of disease of the heart, particularly of the mitral valves. And although the sounds in the upper part of the left side in this woman are not to be distinguished from the resonance of voice, bronchial respiration, and cavernous râles, of tubercular condensation and ulceration, yet we know that in severe cases of bronchitis affecting this part, especially if there be any dilatation of the bronchi,—in connection with condensation of lungs from any other cause,—the sounds elicited may be for a time exactly those which I have

mentioned. From this cause we know that some cases of inveterate bronchitis and asthma have been mistaken, even by practitioners thoroughly experienced in the use of the stethoscope, for cases of *phthisis confirmata*.

One of the cases of pleurisy recently admitted is a woman named Hutchinson, æt. 35, admitted 31st May. This case is chiefly remarkable for the extent of pleuritic effusion, shown distinctly by the usual marks, occupying, from the time of admission, the greater part of the right side of the chest, and obviously displacing the heart towards the left axilla (though without protuberance of the intercostal spaces), while the pulse is little affected, and her breathing, when she is at rest, apparently easy. The disease in her, nevertheless, appears to be of short duration; and at the time of admission, she had ℥xij of blood taken, which was sizy (though without any marked aggregation of the coagulum), leeches and a blister, with relief to the pain and sense of tightness in the chest. Since then, the only remarkable fact presented by this case has been, a return of the pain, followed by a decided extension of the pleuritic effusion (as you will see by the Reports) at the time when her mouth was distinctly affected by mercury. The very same thing happened in the case of the lad Joyce, formerly mentioned; and I regard these two cases, therefore, as favouring the incredulity which I formerly expressed as to the utility, at least as to the importance, of mercury, given so as to affect the system in cases of this kind.

Of course you will understand that when we see inflammatory symptoms easily produced, or reproduced, in a person under the influence of mercury, we are always apprehensive of these becoming speedily connected, in one way or another—we need not at present inquire how—with tubercular deposits and scrofulous disease; knowing as we do, that in this climate such disease is a very common consequence, in certain constitutions, of the agency of mercury, especially if the patient is exposed to cold and wet during or immediately after its effect on the system. In this case of chronic pleurisy it might be supposed that this danger is merely imaginary, because it has been stated that the complication of chronic pleurisy with phthisis is very rare. Dr Blakiston, in particular, speaking from the result of extensive hospital practice, says he had traced the history of above 50 cases of chronic pleurisy, without finding that any of them terminated in phthisis. This observation is well worth attention; and certainly I have seen many cases, where the general symptoms were so much like phthisis that they have been thought decided examples of that disease, but in which chronic pleurisy has been detected, either during life or after death, and where no tubercular deposit ever took place; and I believe you will hardly ever see any very large pleuritic purulent effusion in persons in whom, at the same time, tubercles are in an active state. But I have seen quite enough to convince me that the complication of the two diseases often happens in two ways. 1st, That pleuritic effusion, of various degree, extending sometimes over much of one side of the chest, takes place as an intercurrent inflammation in the course of phthisis—sometimes at an early period of the disease, sometimes late—and may either be re-absorbed, or remain till death, without materially altering the course of the disease; and 2d, that after pleuritic effusion has been chiefly or entirely re-absorbed, the patient, although previously of strong habit, may be left in a state of debility, in which he may readily pass, from slight causes, into tubercular disease. And this last fact in particular is sufficient to show us, that in treating chronic pleurisy by mercury, we ought always to have before our eyes the danger of the patient falling into phthisis, if exposed, even slightly, to its usual exciting causes, during, or for some time after, the mercurial course.

The case of George Sutherland (æt. 29), admitted 22d May, is the only distinct one we have seen of pleuritic effusion (on the left side), besides presenting the usual indications formerly detailed, so distending that side of the chest as to cause bulging of the intercostal spaces, and such displacement of the heart, that its pulsations could be most distinctly felt, and even seen, to the right of the sternum; and the liver, somewhat swelled and tender (as it most generally

is in such cases, especially in young persons), was pretty certainly forced downwards, and was felt dull on percussion 1½ inch below the margin of the ribs. It is important to observe that in this case, as in almost all that I have seen, where the effusion ultimately became very great, it never was attended by acute symptoms. His complaint commenced in August last, with a fit of hæmoptysis (and he has now somewhat suspicious sounds at the apex of the *right* lung); since then he says he has suffered repeatedly a good deal from cough and dyspnœa, but seems never to have been confined to bed, and says, positively, he never had pain of side, and always could lie on either side. His pulse, on admission, was 88, and he suffered a good deal at times from cough and dyspnœa.

Now in this case, there was no such urgent dyspnœa, nor appearance of immediate danger, as I should have thought requisite, some years ago, to justify the paracentesis. But since I have been acquainted with the experience of Dr Hughes, and Mr Cock of Guy's Hospital, as to this operation, and since I have known it performed by Mr Syme—particularly in one case which I recommended to him some years since, and in which it has been repeated many times—with the simple precaution of using *a very small canula, not* $\frac{1}{10}$*th of an inch in diameter*, so as to allow the fluid to dribble off very slowly, and to withdraw the little tube and close the orifice as soon as the fluid begins to drop, so as to provide against the entrance of air into the chest, and to be sure that the fluid taken off is only as much as had *distended* that side of the chest and compressed and dislodged the heart and lungs,—I have been so convinced of the safety of the operation itself, when skilfully performed on this plan, that I have thought it sufficient to justify our advising it, to find the intercostal spaces bulging out, the heart manifestly displaced, and palpitation and dyspnœa easily excited by any exertion. At least, if these phenomena occur in a case which has been slowly making progress, and is apparently unaffected by a fair trial of the usual internal remedies, it may always be feared, that even if absorption does spontaneously take place, it will be so slow, and attended with such debility, and probably hectic fever, as to imply much danger. And although I have known various cases of very good recovery, after such collections of matter within the chest have made their escape by ulceration, either externally or through the lungs, yet the event of any case in which such ulceration take places, must be regarded as very precarious; whereas if the fluid distending one side of the chest, and threatening such mischief, can be safely drawn off, it may be hoped that the absorption of what remains may take place more rapidly, and the danger attending any such ulceration be averted.

Accordingly, after consultation with Mr Syme, and with the staff-surgeon here (as this man is a soldier on sick leave), the left side of the thorax was tapped by Mr Syme, in the way above described, between the fifth and sixth rib, about half-way between the middle of the sternum and spine, and sixty-six ounces of sero-purulent fluid drawn off, in forty minutes. Since the aperture was closed, there has not been a drop of fluid evacuated, nor any complaint of pain or dyspnœa, nor febrile symptom; but after the operation, the left side of the chest still measured $1\frac{1}{10}$ inch more, from the spine to the middle of the sternum, by a line drawn horizontally across the nipples, than the right; and the sound of the heart, although now distinctly heard to the left of the sternum, was still loudest immediately below the centre of that bone. He had been taking a Plummer's pill at night, and gr. 1 ss. of iodide of potassium three times a day before the operation, and this was continued in the hope that the requisite absorption might be thus promoted.

[On the 4th July, *i. e.* thirty days after the operation, the two sides of the chest measured at the same line as before, were exactly equal, and in the upper part of the left side of the chest the sound on percussion was somewhat clearer and the sound of respiration distinctly more natural; but in other parts there was still much dulness on percussion, and the sound of respiration only tubular; and the sound of the heart was still heard loudest beneath the middle of

the sternum. His pulse was now natural, tongue clean, appetite good, the swelling of the liver diminished, and the tenderness there gone; he had no cough or dyspnœa, and could walk several miles without suffering in any way. His mouth was somewhat sore, and his breath tainted, but he had no salivation from the calomel. He was then dismissed, by desire, with cautions as to his conduct,—and certainly with a very fair prospect of the absorption requisite to a complete recovery going on satisfactorily; and we have good reason to believe that this process has been materially expedited by the operation, and perhaps somewhat by the treatment here adopted.]

The case of J. Gayner (æt. 25), admitted 7th June, on the fifth day of pneumonia, with pleurisy of the upper half of the left lung, is a still more remarkable one, particularly as illustrating what I have stated of the typhoid nature of the febrile reaction, frequently observed as attending such inflammations at present, and the modification of practice thereby required. This man had been much exposed to cold, wet, and fatigue in working at a breakwater at Leith, particularly during the night, and had been seized with rigors on leaving work on the 3d instant, the fifth day before admission; he had had much cough, with febrile symptoms, and great weakness from that time, and his sputa had been stained with blood before admission; but at the time of admission there were no such sputa, and not much cough, although the cough became urgent and the sputa quite characteristic very soon after; his pulse was 100, full and soft—his aspect so much that of typhus, that I thought the diagnosis doubtful, and the indications in the left side of the chest were *slight* dulness on percussion in the upper part of the left side, and a crepitous râle heard distinctly on full inspiration, and attended with slight resonance of voice, in different parts of that region, but not extending lower than the inferior angle of the scapula. He had twelve leeches that forenoon, and as his breathing seemed more oppressed,—and as I found he had passed through typhus fever in the hospital two years before, making him less liable to another severe attack of that kind,—he was directed to be bled at the arm to ℥xij in the evening, but fainted after ℥vi had been taken. After the bleeding his pulse was 84, and weak. This blood showed a distinct buffy coat, but the coagulum was not firm, being easily torn by its own weight when lifted on a pin. After this time he has had no more bleeding, but two more leechings, making thirty-two leeches in all; he had two blisters to the side, and was put one day on the use of the antimonial solution in doses of only $\frac{1}{6}$ gr. every two hours, which caused, however, sickness and vomiting, and so much general depression, that it was very properly stopped in my absence; and since then his symptoms, as you will see by the Reports, have distinctly exemplified the following points, illustrating several of the statements I formerly made:—

1. The fever has had, all along, the typhoid form; the expression of his countenance has been exactly like that of the man lying opposite to him, who has had unequivocal typhus—he has had the same drowsiness and indifference to surrounding objects—his pulse has always been soft, even small, his tongue dry, first in the centre, afterwards throughout, his lips incrusted—his skin has never been hot, generally moist, and he has shown more indications of general weakness than many of those patients in pneumonia whom I have seen, on former occasions, sent into the hospital as cases of fever; for many of these have borne evacuations better than was anticipated, and improved in strength rapidly after repeated, although moderate, bleeding; whereas this man bore evacuations ill, and had so obvious depression of strength, that he has been allowed ℥iv of wine daily since the 10th instant, and a mixture containing syrup of squill, with spiritus ammoniæ aromaticus; and there has been neither heat nor flushing, nor increase of cough, or of pain after it, and his pulse has come down from 96 to 84 and 78 within three days after this was allowed. The last leeching and blistering, however, have been ordered since this allowance of wine was begun.

2. You will observe that, even after most of the evacuations I have men-

tioned, there has been a gradual extension of the inflammatory effusions, just as described by Louis. The sound on percussion, almost natural at first, became quite distinctly dull in the upper part of the left side, both anteriorly and posteriorly, and this was attended with absence of the vocal thrill; and the sound of respiration became faint, and mixed with somewhat of the crepitous râle gradually changing to subcrepitous, almost to the base of the lung, while the sputa remained distinctly pneumonic, even after the last leeching and blistering. By these marks we were well assured of the extension of inflammatory exudation both in the lungs and on the pleura; and, by the way, I may remark that these unequivocal marks of *pleuritic* effusion are nearly enough of themselves to set aside the idea of typhus or epidemic fever, during the continuance of which I am sure you will find that true pleurisy hardly ever exists, although particular forms of peripneumony and bronchitis are common.

3. You might observe here, that, *co-existent with this extension of the inflammatory exudation, there was abatement of the general symptoms,* particularly as to the essential points, the state of the pulse and breathing, the pulse coming down below 80, and the breathing, never in this case marked as above 30 in the minute, becoming less frequent and easier, while yet extension of the effusions was going on. I think this implies that the inflammation, although extending, was of slight intensity, and this is confirmed by the improvement which has since taken place, and I hope will be by that which, unless I am much mistaken, you will find is to follow. This we cannot ascribe to the use of the antimony, for it was discontinued after two small doses. Neither do I think we can ascribe it to the use of calomel and opium, for, although he took this in small doses (12 gr. of calomel in all) on the 11th and 12th, this was discontinued on account of diarrhœa, with very slight soreness of mouth; and besides, the improvement as to the pulse and breathing had begun before the mercury was used. The abatement of the symptoms may have been referable only to mildness of the inflammatory attack; but judging by the local symptoms, I can certainly say this, that many cases are fatal in which the portion of lung obviously disqualified for its functions is less than that which appeared here to be involved. I think it, therefore, reasonable to suppose, that as it certainly was the object, so it was the effect, of the evacuations practised in the first three days that he was in the house (although much smaller than in many cases, where the febrile reaction is stronger, is necessary for this purpose), so to modify the inflammation, that the effusions consequent on it, although extending considerably thereafter, did not produce such condensation of the lung as they otherwise would; and were liable, subsequently, to the rapid absorption which I think is already in progress, and of which, unless I am much mistaken, you will see further evidence by watching this case.

If this case had occurred to me twenty-five years ago, I should have practised the same evacuations on his admission, and after that, being warned by the typhoid nature of the symptoms, and further by the diminished frequency of his pulse and improved breathing, to suspend such treatment, I should, without putting my ear to his chest, merely have watched and prescribed for the state of his general symptoms; and finding them gradually to improve under the cautious use of wine and ammonia, should have regarded the case as an illustration of one of the best established and most easily understood principles in medicine—the power of blood-letting—suited in its extent to the state of the general symptoms,—to check inflammatory effusion and its effects on the lungs, even as late as the fifth day from the attack; although affording less striking evidence than we should have had from using the same means on the second or third day,—and although used in a case where the general symptoms were such as to demand the use of stimuli almost immediately after. The information which we now have enables us to speak more precisely as to the changes going on in the lungs; and requires us to alter our language so far, as to use the term modify or moderate, instead of check or subdue, the inflammation, as applicable to the power of blood-letting on the fifth day of

the disease ; but this is a change of language which a little reflection would have shown to be right, even independently of such more precise information, and when the whole history of the case is fairly considered, I think the practical inference is not materially altered.

4. It is probably more practically important to observe, that in this case, as in many others which I have seen of late years, where the pulse has been soft and the febrile symptoms typhoid, there has been a gradual abatement of all the symptoms of the peripneumony—first of the general, and then of the local—under the use of wine and ammonia, begun as early as the seventh day of the disease, while the pneumonic sputa were in full perfection, and the dulness on percussion and crepitous râle *still extending* over the side; and that in this case, just as in that of typhus fever complicated with pectoral inflammation, there is no inconsistency in using the local bleeding and blistering for relief of the cough, pain of chest, and dyspnœa, at the same time that these stimuli are cautiously given to maintain the heart's action and promote the expectoration ; in short, that the state of the general symptoms—the degree and the kind of the febrile reaction that is excited by any inflammation in these parts—known to be remarkably various in connection with nearly similar states of inflammation, in different individuals,—must be our guide in the administration of general remedies. This conclusion is quite in accordance with what has been stated as to such cases of typhoid pneumonia by Dr Stokes and others ; but it is of so much importance to know, not only that such typhoid cases, requiring early modification of the practice, may occur, but that they are especially to be looked for at certain times and places, that it is right to lose no opportunity of impressing this principle upon you.[1]

During the progress of this case I have had another strikingly similar. The patient is a gentleman in this neighbourhood, whom I have seen along with Dr J. Moir, in well-marked pneumonia of the lower lobe of the left lung, who had been only once cupped in the beginning of the disease, and in whom the inflammation, never very extensive, was attended throughout with so soft a pulse and so typhoid an aspect,—cold, clammy state of the skin, and sickness and vomiting,—that we thought it necessary to allow first wine and then brandy, omitting all depleting remedies, by the end of the first week of the disease ; and have had the satisfaction of finding that the general symptoms have improved, and the inflammation gradually subsided, under this treatment.

And although we must confess much deficiency in our pathological information on this as on other subjects, yet it is easy to see a justification of the deviation of our practice in such cases of pneumonia, attended with typhoid symptoms, from the usual practice where the reaction is stronger and more permanent, if we merely attend to the *mode of fatal termination* which may be observed in these cases of typhoid pneumonia ; for it may be clearly perceived in some of them that death takes place in a manner obviously distinct from the death in the more sthenic form of the disease ;—the state of the circulation and of the nervous system, for some time before death, distinctly shows that the patient does not die asphyxiated, but exhausted, and often comatose, or nearly so,—in a manner resembling the usual fatal termination of typhus, rather than of any disease confined to the chest. And farther, on dissection in such cases, I

[1 In this case the improvement of the general symptoms continued progressive, and the sounds in the upper part of the left side of the chest went through changes corresponding very exactly to what is stated above as common, where there has been condensation, from any temporary cause, of the upper lobe ; *i. e.*, we had dulness on percussion below the clavicle, and over the spine of the scapula, with bronchial respiration, resonance of voice, and subcrepitous râle, changing to mucous, and then to a sound not to be distinguished from cavernous ; so that for some days the sounds were just those of advanced phthisis, but gradually the sound on percussion became clearer, the respiration more natural, and the râle dwindled away ; and at this date (9th July) the sounds are nearly natural in these parts.]

have repeatedly seen the hepatized portion of the lung, although quite of the usual appearance, so much less extensive than in other pneumonic cases, that the impediment to the arterialization of the blood thus presented could not be regarded as an adequate sole cause of death. We have no more precise mode of expressing this than by saying, that the shock given to the constitution by a given amount of inflammation in the lungs is greater and more dangerous in some persons, or in persons under certain circumstances, than in others. And that this should be so we can easily understand, because we know that the effects of the shock given by injuries of the whole body, as in cases of concussion, are very various in different persons; and again, what is still more applicable here, that in cases of peritonitis from cold, acting on a system previously healthy, where the sympathetic affection of the circulation is the immediate cause of death, the extent of inflammation which shows itself on dissection is extremely various. It is quite in accordance, therefore, with what is observed in the most analogous cases that we know, to suppose that the constitutional fever excited by pneumonia will be of a very different character, and imply different kinds of danger, in different cases.

Thus these observations on the peculiar form in which this well-known and important disease may present itself, tend only to an additional confirmation of a practical rule which is generally acknowledged, and which is indeed the main reason for our cultivating the pathology, as well as studying the nosology, of diseases,—although not perhaps so generally acted on as it should be: that we must, for the good of our patients, prescribe, not for the names of diseases, but for the nature and tendency of their most urgent symptoms. And in regard to the treatment of pneumonia, when attended by a kind of febrile reaction, in itself dangerous, and likely to be aggravated by depletion, the most important practical rule to be deduced from what has been stated is, not that blood-letting and other antiphlogistic remedies have no power over such inflammation, but that there is a danger involved in such inflammation which such remedies do not counteract, but after a time distinctly increase; and therefore, that it is peculiarly important in all such cases to recognise the inflammation, and apply that kind of treatment *as early as possible*, in order that it may be *as small as possible*, and *yet effective* in moderating the inflammatory process.

[A few days after this lecture was delivered, I saw, with Dr Dunsmure, another striking example of pneumonia, affecting the upper and middle lobe of the left lung, attended with unusual depression of the circulation, and particularly with so much nausea, vomiting, and faintness, that blood-letting appeared inadmissible in the beginning of the disease; and twelve leeches applied on the eighth day had no further effect than relieving the pain of side. This case was fatal on the eleventh day, and showed on dissection the usual appearance of hepatization and softening in nearly half the left lung. The case was further remarkable as an example of phthisis of the right lung, the symptoms of which had abated so completely under the use of the cod liver oil, four years before death, as to enable this man to carry on an extensive business as an auctioneer; and he had been exposed to cold, wet, and fatigue, in conducting a sale some miles from town, some days before the fatal attack. The uneasy sensations in the chest, during his fatal illness, were all referred to the left side, in which the crepitous râle, and the dulness on percussion were distinct; but it further appeared on dissection that two irregular cavities existed in the upper part of the right lung, quite empty at that time, and surrounded by small clusters of tubercles, all of them in the hardened, or nearly cretaceous state. The tubercular disease, therefore, had stopped short, after passing into ulceration; and considering what was observed as to other cases of pneumonia at the time, it could hardly be supposed that the peculiarity of the fever attending the fatal pneumonia of the left lung was connected with that inert diseased state of the right.—W. P. A.]

Part Fourth.

PERISCOPE.

MEDICINE.

CURE OF SCIATICA BY CAUTERISING THE EAR.

[We had recently occasion to allude to M. Robert's practice of curing sciatica by cauterising the dorsum of the foot, and when we afterwards saw in the "Gazette des Hôpitaux," of the 2d July, a case of "Névralgie Sciatique guérie par la cauterisation de *l'oreille*," we thought the printer had probably mistaken *orteil* for *oreille*. We now, however, find that M. Malgaigne did actually apply the red hot iron to the anterior part of the right helix, and that his patient was *cured* forthwith; nay more, that similar success has attended the same practice on two more recent occasions at the Hôpital St Louis. The following account of the antiquity of the practice is extracted from the "Gazzetta Medica Italiana," No. 17, 1850, and may perhaps interest some of our readers.]

Dr Lucciana of Bastia relates in the "Journal des Conaissances Médico-Chirurgicales," 1st May 1850, a radical mode of curing sciatica, popularly practised in Corsica, and consisting in the application of a red hot iron to the ear, and exactly on its helix. The cauterisation cures the sciatica instantaneously, or at least effects immediate improvement. The operation is in Corsica uniformly performed by the farrier, and the inhabitants, when affected with sciatica, lose no time in applying for his assistance. In confirmation, Lucciana adduces some cases of sciatica which he had in vain attempted to cure in the hospital of Bastia by other therapeutical methods, and which yielded to the farrier's cautery, as if to a charm.

In relation to this kind of cure, and to its results, we recommend the perusal of the article "Reumatismo" in the "Instituzioni Chirurgiche di Monteggia," parag. 499, in which mention is made of a priest "who cured sciatica by a small burn behind the anti-helix." The editor of the journal from which this article is taken, adds other authorities in support of the energetic method of cure revived by Lucciana. He cites Mercatus, who recommends, in sciatica, "to open the vein in the middle of the ear with a hot iron," and who probably took the hint from Hippocrates, who advised, "in fluxes descending along the thigh, to open the veins behind the ear." Zacutus Lusitanus, in the second book of his "Praxis Medica Admiranda," has an article entitled "Ustio venarum retro aures ischiaticis utilissima," and professes himself well satisfied with the results which he had himself obtained by this method of cure in certain obstinate cases. He also tells how a traveller, who had long been resident in Japan, in his presence cured an obstinate sciatica, after it had resisted scarification of the skin behind the ears, by cauterising the same parts several times within the space of two hours with a hot brand of lignum vitæ.

ELECTRICITY IN PARALYSIS OF THE BLADDER. BY M. MICHON, SURGEON OF LA PITIÉ.

This application of electricity is mentioned in some of the older writers, and especially Chopart, but in terms which show that they attached very little value to it. It is not mentioned in many modern works; but, according to M. Michon's cases, it appears to be an efficacious remedy.

Case I.—Claude Teroschier, aged 67, a pedlar, was admitted on the 9th October 1848. Eight days before he had been drinking wine moderately in

the evening, he returned home in good health; he was disturbed in the night with desire to make water and go to stool, but was unable to accomplish either. He was seen by a surgeon next day, who drew off with the catheter a considerable quantity of urine, ordered twenty-five leeches to the perineum, and stimulant frictions over the hypogastrium. He was subjected to this treatment for eight days, had the water drawn off night and morning, but never in the intervals passed a drop of urine. When admitted under M. Michon, he had absolute impossibility of passing urine without the aid of the catheter, had no fever, and no pain beyond the uneasiness caused by the distension of the bladder. The day after admission the bladder was again emptied by means of a large catheter, which passed freely. There was no kind of stricture or obstruction, and the prostate was ascertained by the finger to be sound. The urine flowed from the catheter with a feeble jet, which gradually diminished as the bladder became emptied. It was necessary, in order to evacuate it, to make pressure on the hypogastric region. The patient's account of himself did not tend to show that there had been any previous want of power in the bladder; but M. Michon was inclined to suppose that it had been gradually losing tone from the man's advanced life, and that complete paralysis had followed from over distension on the night of his seizure. He was ordered demulcent drinks, laxative enemata, and the catheter twice daily. Two days after, the patient having suffered much from repletion of the bladder, M. Michon left the catheter in, to enable it to be evacuated more frequently without risk of injury to the urethra from less skilful hands. Injections of tepid water into the bladder, discharging the urine by the catheter every three hours, Vichy water, blister to the hypogastrium, cantharides liniment to the belly and thighs, were all tried without success. On the 6th November the patient had rigors and vomitings; the urine became turbid and ammoniacal, with a copious puriform deposit. The catheter was removed, and the febrile symptoms relieved by baths and emollient poultices. The urine remaining ammoniacal, acidulous injections were used, with the effect of slightly improving it, but the retention was as complete as ever. As the patient, towards the end of November, lost ground, M. Michon resolved to have recourse to other means, and tried the effect of electricity.

The method of applying the electricity was as follows:—A silver catheter was introduced into the bladder, and a female catheter introduced into the rectum, and made to rest upon the recto-vesical parietes; each of these catheters was connected with one of the poles of an electric machine, which was put at first in gentle action, and then continued briskly for two or three minutes. The patient experienced no pain, he felt only some slight prickling; the urine had to be drawn off twice in the succeeding twenty-four hours. The electricity was re-applied next day in the same manner,—the patient felt the prickling more acutely, but only in the rectum,—he felt nothing in the bladder. A third application of the electricity was made, the same sensations were experienced, but, as on the two former occasions, ceased immediately afterwards. On this night he felt a desire to make water, and accomplished it easily without the aid of the catheter. Next day he was up, and showed the urine which he had passed during the night. From that time he required no more catheterism, but the electricity was applied again for three successive days; it produced the same effects, only that the sensations were more acute, and that during its application the urine was projected from the catheter at each electric stroke. During the intervals the urine was passed at his will, and without difficulty; it rapidly lost the ammoniacal odour, and became limpid. The fever disappeared, the patient regained his strength, and after remaining in the hospital for three weeks without any urinary disorder, was discharged cured. He presented himself again in three weeks, at the request of Dr Michon, and was found to be quite well. It is proper to state, that during the application of the electricity the catheter was not allowed to remain in contact with one single part of the bladder, but was moved gently over the inner surface of the organ.

Case II.—A few days after the above patient was discharged, there was admitted into the same ward E. Poulard, aged 65, a labourer, tall and muscular. He had been a soldier, and twice wounded, but his health was quite good. He never had suffered from gonorrhœa or syphilis. He stated, that in 1844, when employed in sweeping the streets, he had been exposed to a continuous rain for two whole days, after which he was seized suddenly, and without any other appreciable cause, with complete retention of urine. He had no medical aid for two days, and made no water all that time. The practitioner who saw him did not draw off the water, but merely ordered a bath. He made water in the bath, and from that time he felt relieved, and returned to his work. But the retention was not completely removed, he made water with difficulty, and only drop by drop, for five or six days, and then was admitted into the clinical hospital with complete retention. He was discharged in fifteen days, much improved, and furnished with a catheter to relieve himself when necessary. He did so for eight days, and then became so well as to be able to dispense with it, and returned to his work. He remained quite well till December 1848, when he experienced a general prostration and uneasiness, for which he could not account. On the 1st January 1849, after a temperate breakfast, he wished to make water, but found that he could not do so. A surgeon relieved him several times with the catheter, and sent him to the Pitié Hospital on the 30th January, at which time he presented the following symptoms :—There was complete impossibility of making water ; there was not the least symptom of paralysis of the trunk or extremities ; the catheter passed easily into the bladder ; the prostate was felt from the rectum to be diseased ; the right lobe was hypertrophied and indurated, and the rest of the gland rather enlarged ; there was no fever. He was ordered to take taraxacum with nitre, and to have the catheter passed thrice daily.

It was resolved to treat him by electricity, care being taken first to ascertain that the retention was real and complete. On three successive mornings the bladder was found distended, and forming a swelling above the pubes ; the patient could by no effort of his will pass water. Each morning there was drawn off by the catheter about two pints of clear limpid urine ; it did not issue in a jet, but fell from the end of the catheter. On the 9th January the electricity was applied in the same way as in the former case. The first application lasted for three minutes ; the patient experienced sharp pain in the rectum, and some prickling in the penis. He required the catheter at night, and the urine flowed from the instrument without any jet as before. The electricity was re-applied next day, and the catheter was again needed, but the dresser who drew off the water thought that it issued from the instrument with a greater power than before, although there was less urine in the bladder. During the night the patient had frequent calls to make water, and accomplished it on several different attempts without the catheter. The next day, on repeating the electricity, the bladder, though previously emptied, expelled with some power the few drops of urine which it contained. The patient made water freely at night, and required the catheter no longer. The electricity was applied for a fourth and last time. The whole duration of its application was four times of three minutes each. He was kept in hospital nineteen days longer, remained well all that time, and was discharged cured.

M. Michon gives the account of two other cases from the practice of Dr Monod. One of these was in an old man of 99. M. M. first applied the electricity to the surface of the hypogastrium—that was done twice without effect. He then applied it by a metal catheter in the bladder, connected with one pole, while the other was applied over the pubes. Some effect followed from this, but he ultimately adopted M. Michon's method, of one catheter in the bladder and another in the rectum, and in five days the action of the bladder was restored. The patient, however, did not bear the electricity very well, and at the end of the fifth application experienced some symptoms of cystitis.

M. Monod's other case was in a woman who had retention after poisoning

by charcoal vapour. The paralysis of the bladder had lasted six days, and it was cured by a single application.

According to M. Michon, the cases to which the electricity should be most applicable are those of essential paralysis of the bladder, but such an affection is comparatively rare, the retention and paralysis being commonly traceable to enlarged middle lobe of the prostate. But even should it only be applicable to senile paralysis, or paralysis from over-distension, it would still be an useful addition to the ordinary means of treatment, and the above cases warrant its being tried when these have failed.

M. Michon appears to think that the electricity acted here not directly on the bladder, but on the sciatic and sacral nerves, which supply the greater part of the pelvic organs. Both he and M. Monod seem to hold that the electricity is of no avail if applied to the surface of the body, and that to render it efficacious it must be passed into the interior of the organs.—*Bulletin de Thérapeutique*, April 30, 1850.

[We had an opportunity in one case of trying the electricity, and we succeeded perfectly, by sending the current through the pelvis from side to side. We employed a common galvanic coil machine. The patient, who was advanced in life, had been drunk, and sleeping in the open air in winter. There was neither stricture nor diseased prostate, but the patient remained for about a week quite incapable of passing water, and required the catheter twice daily. After three applications of the electricity to the pelvis, for about half an hour each time, the power of the bladder was completely restored. In another case where we tried it, it failed; but the patient was about 90 years old, excessively feeble, very restless and intractable, and had diseased prostate.]

STRYCHNIA IN PARALYSIS OF THE BLADDER. BY M. LECLUYSE, OF TOPERINGHE.

M. L. was called to a man of 68 years of age, who, after a day of excess, had retention of urine, which led to complete paralysis of the bladder. After trying all the ordinary methods, such as permanent catheter in the bladder, copaiva, turpentine, stimulating diuretics, cold douche, and cantharides, he gave strychnia internally. It was pushed as far as one grain in the day, and produced tetanic spasms in the muscles of the trunk and limbs, but left the bladder as thoroughly paralysed as ever. After fifteen days abstinence from all treatment, he resolved to apply the strychnia in the form of injection into the bladder. For this purpose he dissolved six grains in alcohol, mixed this with a pound of water, and ordered four injections of two ounces each to be introduced through the catheter, after previously emptying the bladder. At first the bladder seemed insensible to the contact of the liquid, but after four or five days, the patient found himself wet with urine, though the fixed catheter was well closed, and he supposed that this was due to a fissure in the instrument. It was withdrawn for the purpose of being examined, and to the astonishment of both patient and surgeon, the urine flowed freely and well, and the patient recovered completely, with no other inconvenience than some momentary irritation, probably caused by the irritation of the catheter.

M. L. thinks himself warranted, from the length of time that elapsed between the internal administration of the strychnia, and the application of it by injection into the bladder, in concluding that the former had nothing to do with the cure. —*Bulletin de Thérapeutique*, April 30, 1850.

[It seems to us to be doubtful if the strychnia injected into the bladder had any greater share in the cure than might have been due to the effect of the injection of the water alone. It is not likely that strychnia, merely suspended in water, when injected into the *bladder*, would be capable of producing much effect. The following case will show how strychnia acts when injected into the bladder in a soluble, and therefore absorbable, form.]

PARALYSIS OF THE BLADDER—INJECTION OF SULPHATE OF STRYCHNIA—SYMPTOMS OF POISONING.

A man, aged about 48, of good constitution, had paralysis of the bladder consequent upon a debauch. He remained three days without treatment, and the medical man who saw him, finding the urine dribbling away, mistook the case for one of incontinence of urine, and prescribed some trifling remedies. The patient was admitted into the Hôpital Beaujon under M. Robert, who, finding that besides retention from paralysis, there were symptoms of cystitis, such as pain in the hypogastrium and kidneys, and purulent urine, ordered a bath, leeches to the perineum, and the use of the catheter twice daily. Having thus relieved the more urgent symptoms, he proposed to adopt M. Michon's practice of applying electricity, but not having an apparatus at hand, deferred it. In the meantime, M. Lecluyse's plan of injecting strychnia having been mentioned to him, he resolved to try it, but was advised to use it in smaller doses. He ordered three grains (twenty centigrammes) in six ounces of water. The strychnia not being soluble in this amount of water, the pupil in charge acting upon the principle—*corpora non agunt nisi soluta*—substituted sulphate of strychnia far the alkali itself. The injection was given; in a few minutes afterwards, the *interne* who had charge of the case, observed convulsive movements coming on, with opisthotonos, and especially spasms of the thoracic muscles, which alarmed him much. The bladder was immediately emptied, and washed out with tepid water. The symptoms did not become worse, they gradually diminished, but for some days the patient still experienced spasms of the thoracic muscles. The most remarkable fact is, that in spite of the severity of these symptoms, which indicated a great intensity of action on the part of the strychnia, the bladder did not recover its tonicity, but remained paralysed. The only appreciable change in this respect was, that it was remarked on the third day, when the catheter was introduced, that the urine flowed with a little more force than before, but this was a very insignificant amelioration. The spasmodic affection of the chest continued, the patient was seized with bloody expectoration (black mucous sputa, differing from those of pneumonia, and appearing to result from a sort of asphyxia), symptoms of urinary fever soon came on, and the patient died. There was no dissection.—*Bulletin de Thérapeutique*, May 15, 1850.

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

MILK IN THE BREASTS OF A MALE INFANT. BY DR BATTERSBY.

Bonetus[1] gives various instances, quoted from different authors, of virgins and men with milk in their breasts. In Captain Franklin's "Journey to the Polar Sea," is noticed an old Chipewyan, who, on losing his wife in childbirth, had put the infant to his breast, and earnestly prayed that milk might flow, and had actually been happy enough to see sufficient produced to enable him to rear the child. The Indian was now old, but the left breast still retained the unusual size acquired by nursing.

A parallel instance is recorded by a Bishop of Cork. His lordship had given half-a-crown to a poor Frenchman above seventy years of age, who made the best return he could by showing his lordship what he knew must be a curiosity —two very large breasts with nipples larger than the Bishop had ever seen in a woman, and related that his wife dying when his child was two months old, he endeavoured to pacify it at night by putting it to his breast, and at length milk actually came, so that he suckled and brought it up.[2]

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[1] Medicinæ Septentr. Collatitia. Pars altera, p. 163, fol. Genevæ, 1686.
[2] Phil. Trans., vol. xli., p. 813.

The fact of males giving milk is now well established. Dr Dunglison quotes the case of a coloured man, who, in 1837, was shown by Professor Hall to his class. The man was aged 55, and had large, soft, well-formed mammæ, rather more conical than those of the female, and projecting fully seven inches from the chest, with perfect and large nipples. The glandular structure seemed to the touch to be exactly like that of the female. He had officiated for several years as a wet-nurse in the family of his mistress. Great difficulty was experienced in arresting the secretion of milk when no longer required. His genital organs were fully developed.

Adult males of other mammalia have been known to furnish milk. Blumenbach has described a he-goat which it was necessary to milk every other day for a year; so that, according to Dr Elliotson,[1] to say with Virgil, *mulgeat hircos* is not tantamount to calling a man a fool.

In Blumenbach's Physiology, by Elliotson, we read that "occasionally newborn infants of either sex have been known to furnish milk," but no instance of this is quoted. Neither does Bonetus give any case of a *male* infant thus disposed. He says, we rarely read of infants having milk in their breasts manifestly by a perverted order of nature, nor do I remember meeting with any other example of this, except in the writings of the very ingenious Cardanus, *Lib. xii. de Subtil*, in an infant in its first month with the breasts manifestly swelled, and giving out milk copiously from the nipples. Likewise another case of a little female infant, three months old, pouring forth milk from its conspicuous little breasts.—*In Joach. Camerarius de illustr. Norimb. Med. Exempt.* I may here, adds Bonetus, remark as a very rare fact, that it has happened to me to observe, not long ago, a little girl, aged two weeks, whose nipples afforded during eight days a fluid rivalling milk, *liquorem lactis æmulum*.[2] The infants here spoken of were all females.

In explaining the cause of abscess of the breast of infants, Dr Maunsell states that "the breasts of infants, both male and female, contain at birth a secretion *somewhat resembling milk*. Accordingly, the attendants frequently set about pressing the part until inflammation, and occasionally an abscess, is produced."[3] Dr B. believes the common impression amongst medical men of the present day to be similar to Dr Maunsell's—that is, that this secretion is not milk, but merely resembles it. Dr Carpenter states that "a fluid, which is *probably mucus*, may be pressed from the nipple of many persons."[4]

The infant in the following case was well formed, and the chemical and microscopical examination of the milk, made by my friend Mr Moore, afforded the best evidence of its resembling so closely the milk of the adult human female, as to leave no room for doubting them to be precisely identical.

James Kernan, aged three weeks, was brought to me on the 3d of last May by his nurse, who informed me she received him from his mother eight days before. She then found the breasts swollen and hard, and with milk in them. She obtained from them "the least little drop," she stated, which had the taste of milk. The breasts have since become smaller, but are still the size of large walnuts, with the soft hardness peculiar to the breasts full of milk. They are quite round and moveable, seem tender to the touch, and are rather red on the surface. The nipples are of a size proportionate to that of the breasts. The child has some bronchitis.

7th.—Dr B. could press some milk out of the breasts.

12th.—Dr B. withdrew, by gentle pressure on the breasts, nearly a drachm of milk. It was precisely similar in appearance to woman's milk. Having left it with Mr Moore, Dr B. received from him the subjoined note:—

"My dear Sir,—Many thanks for the specimen of milk, which is most interesting, as it closely resembles that from the adult female. It is alkaline, throws

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[1] Physiology, p. 833.

[2] Maunsell and Evanson's Diseases of Children, p. 184.

[3] Physiology, p. 666.

[4] Loc. cit., p. 164.

up a cream, is not rendered viscid by caustic potash, nor does it coagulate when heated. Viewed under the microscope, it is seen to consist of normal milk globules of various sizes; on the whole, smaller than those contained in ordinary human milk, floating in a colourless fluid. It contains a very few colostric bodies, and some epithelium.—Yours very truly, "Wm. D. Moore.

"F. Battersby, Esq., M.D."

It is to be regretted that the presence of caseine was not here determined. The quantity of milk submitted to examination was so small that many experiments could not be tried, and Mr Moore, convinced from its microscopic characters of its being milk, directed his attention to ascertain whether the colostric bodies seen under the microscope were sufficient in amount to give the fluid the chemical properties of colostrum. The fact of heat producing no coagulation, and of the fluid remaining unaltered on the addition of caustic potash, proves the absence of the colostric character. Of the fact of its having been true milk, Mr M., from extensive observations on the subject, has not the least doubt.

I was not again allowed to express any more milk. The breasts soon subsided, no abscess ensued, and he is growing up a healthy little child.—*Dublin Medical Press*, 17th April 1850.

NEW OPERATION FOR OVARIAN DROPSY. BY MR I. B. BROWN.

Mr I. B. Brown brought before the Westminster Society a new operation, which he had lately performed in ovarian dropsy. In doing so, he was particularly anxious to be understood as never having condemned operative surgery for the relief or cure of this disease; he always considered that his plan of pressure should be first tried, wherever applicable, and after that and other remedies had been unsuccessful, it was not only justifiable, but advisable, to have recourse to operation; but the nature of that operation was the great point. He believed that the plan he was now about to submit to the Society, coupled with another which he would also mention, would be found of practical utility, and be of less danger to the patient than the formidable operation for ovariotomy. He would not, on this occasion, attempt to enter into the very interesting case in which this operation was performed; he would do so fully hereafter, in the series of his cases which he from time to time should place before the profession, whether successful or unsuccessful; he would merely remark, that this patient was considered by himself, and also by Mr Fergusson, as having only a few days of life remaining, and this operation held out reasonable hopes of prolonging her life, if not even of arresting effectually the disease itself. The operation was successful in prolonging her life for some weeks; and its whole course clearly showed, that in cases free from other serious disease, this operation was calculated to cure many cases of ovarian dropsy. He (Mr Brown) observed that this operation differed materially from Mr Bainbridge's, inasmuch as that performed by him required the patient to be in the prone posture for many months; whereas this enabled the patient to be on her back, on water cushions, which added very greatly to her comfort, and also enabled the nurse to dress and clean the wounds without fatigue or annoyance to the patient, as well as enabled the surgeon to apply pressure over the cyst, by placing strips of plaster from the opposite side of the abdomen, and bringing them firmly over beyond the incision and open wound. He placed the patient on the edge of the bed, in the horizontal posture, and then drawing an imaginary line from the umbilicus to the superior spinous process of the ilium, and dividing it into thirds, he made an oblique incision from above downwards, and from within outwards, through part of the middle third and into the outer third.

The oblique incision was about three inches. He (Mr Brown) dissected carefully through the muscles and fascia down to the peritoneum; through this could be seen the whitish shining coat of the cyst; he then made another smaller incision at right angles to the first, and dissected down to the peritoneum; this incision was about an inch and a-half in length. At the point of

the angle a large-sized trocar was introduced, and the fluid drawn off. The canula being kept in, the peritoneum was divided, and reflected back; then the cyst was stitched by sutures to the aponeurotic tendon of the external abdominal oblique muscle, carefully avoiding the other structures; these sutures were nine in number, and were so arranged as to secure completely the cyst on all sides to the tendon of the muscle, and prevent any escape of the fluid from the cyst into the peritoneal cavity; the canula was now withdrawn, and by means of a pair of scissors the cyst was divided between the sutures; a pledget of lint, soaked in oil, was then introduced into the wound, and changed occasionally, and some adhesive straps placed across the abdomen, to keep up gentle pressure. Not a single bad symptom, referable to the operation, followed. The second operation was performed in a similar manner, and in the same position, but a piece of the cyst was cut out, the fluid being first evacuated, and then the remaining portion of the cyst was allowed to return to the peritoneal cavity, the external wound being closed; pressure was applied over the whole abdomen by means of adhesive strapping and flannel bandage. This operation was intended to imitate spontaneous rupture, and was only to be recommended where the fluid was not of an irritating character.—*Provincial Jour.*, April 17, 1850.

TOXICOLOGY.

POISONING WITH DARNEL GRASS, *Lolium Temulentum*. BY DR EITNER, OF STEINAU.

The work-people of a farm complained to the authorities that they had suffered from the use of certain barley meal,—after taking which, in the form of pottage and dumplings, they had all become so ill as to be for some time unfit for their work. When they complained to their master, he treated the statement as a pretence for escaping from labour. The meal, which was submitted to Dr Eitner for inspection by the authorities, showed a good many black specks, but had not otherwise any unusual appearance. When chewed dry, in small quantity, it caused a feeling of burning in the mouth and throat, and confusion in the head. The proprietor was ordered not to give any more of this grain to his work-people, but he disobeyed this injunction. Thereupon the people, in a body, came and complained that they had again all been ill, after eating dumplings made of it. The symptoms which had attacked them each time after its use were severe vertigo, headach, anxiety and general uneasiness, followed by trembling and vomiting, accompanied by a small irregular pulse. The people had been for some hours, less or more unable for work. Under these circumstances, the author, accompanied by a policeman, went to the place to inspect the grain from which this meal had been taken. He found a large store of thrashed barley, so copiously mingled with darnel that at least every tenth grain was of this grass, which was readily distinguished by its longer ears from the barley, but from which it could not be separated by sifting or other mechanical means. To separate the darnel from the barley by picking was too tedious and expensive a process. The meal was, therefore, confiscated as an improper article of food. It was intended to have thrown out the whole meal upon the dunghill or into the water; but, at the request of the proprietor, and upon his assurance that he could give it without injury to his swine, he was allowed for this purpose to soak it in water, in the presence of the authorities, in feeding troughs, upon the belief that swine, from their strong digestive powers, can eat even narcotic substances, such as hyoscyamus, without injury! With respect to the grain, he was allowed either to use it solely as fodder for his cattle, or to pick out the darnel from it, and, before using it further, to submit it to the inspection of a miller, authorised for this purpose. He used it as fodder, not wishing to employ it any more as meal. The millers in the neighbourhood were further enjoined not to grind grain which was mixed with darnel seeds. From this date no more complaints were heard about this matter.

The noxious qualities of darnel grass partake more of the narcotico-acrid, than narcotic character. According to recorded experience, the symptoms are sometimes manifested by nausea, vomiting, inflammatory affection of the gullet, stomach, and bowels, and febrile action,—at other times, languor, cramps, tremors, paralysis, apoplectic attacks, delirium, and death, sooner or later, according to circumstances. In these respects the action of darnel is somewhat analogous to that of ergotised grain, and both are apt to occur in very wet seasons.

It appeared probable that, in the above cases, serious results would have followed, but for the interference of sanitary police measures.—*Casper's Wochenschrift*, 9th February 1850.

POISONOUS EFFECTS FROM ZINC.

It is now some time since it was proposed by M. Leclaire to use oxide of zinc as a substitute for white lead, with a view to avoiding the dangerous effects of the latter on the workmen. There could be little doubt that, in point of salubrity, oxide of zinc was an improvement on carbonate of lead; but it was still a matter worth determining to what extent the oxide of zinc was itself free from objection, and whether or not some precautions were necessary regarding its use.

M. Flandin endeavoured to determine this experimentally. He rubbed animals over with ointments of oxide of zinc, of carbonate of lead, and sulphate of lead; and whilst he found that the two last always produced poisonous effects, he observed that the animals rubbed over with the oxide of zinc continued to enjoy their usual health. The following facts, however, show that the innocuousness of oxide of zinc must not be admitted so decidedly as Flandin supposes.

I. *Poisoning by Oxide of Zinc, used as a substitute for White Lead.* By Dr Bouvier, of the Hôpital Beaujon.—A man, aged 42, a labourer, entered on 19th April with all the symptoms of metallic colic. He had been employed for the fifteen previous days at a white colour manufactory, along with five other workmen, in barrelling oxide of zinc, and in repairing casks which had contained that substance, during which operation they were exposed to an atmosphere loaded with the powder of the oxide. From the commencement of their work, he and his comrades experienced colic and a repugnance to food, and the wine and brandy which they took to excite their appetite were disgusting to them, and did not remove the clammy taste which they had constantly in their mouths. This man could not continue long at work. After ten days of this employment he was seized with vomiting, severe colic, accompanied by constipation, which persisted and increased in intensity so much, that he rolled on the floor in agony. On the day of admission he continued to vomit, and to suffer severe abdominal pain. The vomited matters were bilious, and he rejected all his food almost immediately after swallowing it; he had had no stool for five days; the belly otherwise was natural, the tongue whitish; he had no appetite, was free from fever, but was sleepless. The next day, 20th April, the bowels were opened by two ounces of sulphate of magnesia, and the painter's purgative clyster, as used in La Charité—[this consists of twelve ounces of wine, with six of oil]. The free evacuation of the bowels, and the administration of two and a-half grains of opium, were followed by cessation of vomiting and diminution of the pain. From this time to 26th April, after having taken from six to twelve grains of gamboge daily, and using frequent clysters and medicated baths, he became convalescent, and was dismissed cured on 4th May.

From the whole history of the case, there appeared to be no doubt that this man suffered from a genuine zinc colic. To ascertain that it was really oxide of zinc which had caused the illness, M. Bouvier collected, by washing the surface of his body, the metallic particles which adhered to it, and found them to consist of oxide of zinc. We are not entitled from this to say, that the oxide of zinc is as noxious as white lead; but it shows, at least, that some

precautions require to be taken by those who work with it, in order to preserve their health.—*Compte-rendu de l'Académie des Sciences*, May 1850, in *Bulletin de Thérapeutique.*

II. *Zinc poisoning observed in the Workmen employed in twisting Galvanised Wire.* By MM. Landouzy and Maumené, of Reims.—The iron wire employed for securing the corks of champagne is sent in bundles of one to ten kilogrammes to workmen called *tordeurs*, who, by a dexterous manœuvre, cut and twist from ten to twenty threads of wire at a time. These wires are then made up in packets of one kilogramme, and after being beaten with a bit of wood to make them even, are packed in bundles. Although this sort of work had been followed by the same workmen from eight to fifteen years under very bad hygienic circumstances as regards ventilation, they never had experienced any evil effects from it till the beginning of January 1850, when the so-called galvanised wire (which is iron wire covered with a layer of zinc) was substituted for the common iron wire; and soon after, the workmen began to complain of the taste as of a sweetish powder in the throat, an incessant tendency to cough and spit, shiverings, and general malaise. The whole of the people employed in this branch of industry—two youths, two women, and two men—were affected by symptoms which were referable to zinc. Four had symptoms of general depression, with sore throat, swelling and ulceration of the tonsils, inflammation of the palate, white pellicles on the gums, salivation, fœtid breath, colic and diarrhœa. In one, the colic and diarrhœa were the only symptoms observed; in another, the colic was accompanied by nausea, tenesmus, and obstinate constipation. The wires with which they worked had been made hurriedly and carelessly, and were covered with a dusty powder, which escaped abundantly during the twisting, and especially the beating, of the wires. This powder consisted of zinc, oxide and carbonate of zinc, alloy of zinc and iron, iron, and oxide of iron. It contained no trace of lead.

These symptoms seem to have subsided readily, without treatment, on abandoning the occupation. With one exception, all the work-people returned to their work in from three to six days.

That the symptoms were due to the exposure to the dust appears from the circumstance that, in fifteen days more, the same work-people, in the same hygienic circumstances, resumed the same work, with the same galvanised iron, but free from all dust, and none of these phenomena manifested themselves which were formerly observed.—*Gaz. Méd. de Paris*, 1st June 1850.

Part Fifth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

Session XXIX.—Meeting XV.—*June* 19, 1850.—James Syme, Esq., President, in the Chair.

CASE OF LARYNGO-TRACHEITIS TERMINATING FAVOURABLY AFTER TRACHEOTOMY.

Dr James Gillespie read an account of a case in which he had recently performed tracheotomy successfully.

(This case appeared at p. 25 of our last Number.)

The *President* said there could be no doubt that this patient's life had been saved by the operation performed by Dr Gillespie, and he believed that this pleasing result was partly due to the employment of a large tracheotomy tube. He added, that the large tube used in Edinburgh, and infinitely superior to the

smaller canulæ which some surgeons recommended, but which he himself considered dangerous, was first made for Mr Liston, who used it in a case of œdema glottidis. Dr Gillespie had acted wisely in withdrawing the tube at the proper time. When too long kept in the wound, it cannot be afterwards dispensed with.

Dr Bennett suggested, that as both the cases alluded to by Dr Gillespie had occurred in pregnant females, possibly the peculiar nature of the exudation might be connected with the plastic condition of the blood which was known to exist during pregnancy.

Dr Maclagan and *Dr Moir* also took part in the discussion.

PATHOLOGICAL STATES OF THE LUNG CONNECTED WITH BRONCHITIS.

Dr W. T. Gairdner read a paper on the pathological states of the lung connected with bronchitis.

(The first part of this Communication appears at p. 122 of our present Number.)

After some remarks by the *President* and *Dr Bennett*, it was agreed to postpone the discussion on this communication till the next meeting of the society.

EVULSION OF TENDONS.

The *President* read the following case, recently communicated to him by Mr Gerard, surgeon at Troup, near Banff:—

"Troup, Banff, March 12, 1850.

"Dear Sir,—In the March Number of the 'Monthly Journal of Medical Science,' I observe your notice of the above injury, and beg to inclose the circumstances of a case which lately came under my care, and which fully agrees with your experience of the absence of those consequences which theory would lead us to expect to follow such an accident.

"John Laird, a middle-aged labourer, while turning a grindstone with his left hand, on which he wore a thick worsted glove, or 'mitten,' suddenly felt his thumb fixed around the iron handle, and 'too tightly,' as he said, 'to be convenient.' He immediately attempted to stay the velocity of the machine, and, 'to his astonishment,' discovered the injury, to dress which I was sent for. On visiting him, he drew from his pocket the second phalanx of his left thumb, very neatly separated at the joint, and having attached to it the tendon of the flexor longus,—the muscular fibres which had given way being traceable at the point of separation. From the time of the injury to the healing of the stump (which was effected by the warm-water dressing alone) he never complained of the slightest uneasiness in the forearm; nor did pressure along the course which the tendon had occupied, produce any feeling of tenderness.—I am, dear Sir, yours truly,

"James Gerard, Surgeon.

"Professor Syme."

URINARY AND SALIVARY CALCULI.

The *President* exhibited two curious specimens of urinary and salivary calculi.

The urinary concretion had been sent to him by Dr Paul, of Elgin. It was remarkable for the beautiful arrangement of the concentric layers of which it was composed.

The salivary calculus, which was of unusual size, had been sent by Dr John Traill, of Arbroath, who likewise had furnished the following account of the case:

"Arbroath, May 24, 1850.

"A stout healthy man, aged 35, by trade a blacksmith, applied to me about ten days ago for a tumour at the angle of the lower jaw. Examined externally, it seemed to occupy nearly the usual position of an enlarged tonsil; was of considerable size, and of stony hardness; but on passing the finger into the mouth its situation was felt to be under the root of the tongue, and within the ramus of the jaw. The man had been sensible of its existence for upwards of ten years, during which time it had gradually increased in size. In general it caused but little inconvenience, but on several occasions had become enlarged

and painful. These attacks had always been relieved by leeching and other treatment.

"My first impression was, that the tumour was of a malignant nature; but on more careful examination I thought I detected a rough point, and on making an incision the nature of the disease became at once evident. Although I made a very free opening with a probe-pointed bistoury, I found much difficulty in removing the calculus, and was finally obliged to break it up with strong forceps. A good deal of its substance was, in consequence, lost, and when entire the stone must have weighed above three drachms, and measured about an inch and a-half in length. A small flat stone lay in a smooth cavity on one side, perhaps at the orifice of the duct.

"The calculus seemed to occupy the situation of the body of the sub-maxillary gland, altogether posterior to the duct of Wharton."

ATROPHIED PANCREAS.

Dr W. T. Gairdner exhibited a specimen of remarkably atrophied pancreas from the body of a man who had died of diabetes. The duct was of the usual length, but no glandular tissue remained except a piece of the size of a small hazle-nut. A similar coincidence had been observed by Bouchardat, who, on theoretical grounds, considered that diabetes was essentially a vitiation of the pancreatic secretion. This view might be too exclusive, but there was now little doubt that diabetes was generally connected with derangement of the duodenal digestion, of which this case might be considered an illustration. Another circumstance in connection with this subject might be mentioned, though not bearing directly on this individual case. It had been lately rendered nearly certain by the researches of Bernard and others, that the assimilation of fatty matters is much influenced by the perfection of the duodenal digestion, and particularly by the supply of pancreatic fluid. Now, curiously enough, it happened that Dr Bright had long ago pointed out (Med. Chir. Trans., vol. 18) that the fæces in many cases of atrophied pancreas were characterised by the presence of much fat. This observation had not received the attention which was probably due to it, especially when taken in connection with the fact, established by Dr Percy's analyses, that in diabetes, also, the alvine evacuations were generally overcharged with fatty matters. Dr G. thought that these remarkable coincidences were of a nature to call strongly for renewed observation, with the view of determining the true relation between the presence of undigested fatty matter in the fæces, and diseases depending on deranged duodenal digestion.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION IX.—MEETING V.—*April* 24—(*Continued*).

A communication was read from Dr Imlach of Liverpool, being the account of a

CASE OF TRISMUS NASCENTIUM.

I was called, November 30, 1849, at 1 A.M., to see a male child, aged seven days. The history of the case is the following:—The child is the ninth of healthy parents. It was born on Friday 23d, after a very hurried labour, the umbilical cord being twice round the neck, so tightly that the infant did not breathe till the midwife had allowed some blood to escape from the divided cord. On the Monday following (the third day after birth) the cord separated in the usual manner, and the child progressed well till yesterday (Thursday, 29th), when, about 11 A.M., the mother perceived that it could not take her nipple into its mouth. It swallowed, however, greedily when fed with a spoon. It cried a good deal, which was attributed to flatulence, and accordingly it got a dose of aniseseed. Bowels freely moved, urine passed frequently, no vomiting. About 4 P.M. it had a sort of a fit, after which it became more restless and fretful. At midnight it got worse, and the mother perceived it could not open its mouth.

I found that, when crying, the mouth was contracted in a very peculiar manner, and the face became very livid, the breathing very quick, about 90 in a minute, the hands clenched with the thumbs in the palms of its hands, and the child apparently in great agony. The jaws were partially open, so that I could introduce the tip of the forefinger into its mouth, but no force I deemed safe to use could separate the jaws further from each other. I immediately put the little patient under the full influence of chloroform, in the usual manner, with it sprinkled freely on a handkerchief. It screwed its face about at first, then raised its head into the hollow of the handkerchief, and attempted to suck. It soon became quite quiet, however, and the breathing fell to 60 in a minute. The pulse I could not count satisfactorily enough to state its rate. I could now get the tip of my thumb into its mouth, or put my finger easily into it. A sinapism was applied for five minutes to the nape of the neck, a grain or two of calomel given dry, followed by a little castor oil, as the last motion was curdy and sour smelled. After keeping the child under chloroform for nearly an hour, I left it still asleep.

Half-past 9 A.M.—Has slept a good deal, and been pretty quiet till an hour ago, when it began again to scream most mournfully. The face is remarkably puckered about the angles of the mouth and cheeks, and the infant has a spasmodic way of drawing its breath or coughing, accompanied with great lividity of face and neck. The hands are not so firmly clenched as they were during the night, and the feet present no appearance which one does not usually see in so young an infant when washed or otherwise disturbed. The jaw is as firmly fixed as ever, and there does not seem any remarkable rigidity about the neck or spine. But when the child is moved in any way it starts convulsively, and a fit ensues. It has had three natural-looking dejections, and passed urine once. Upon examining the umbilicus, I found it red and excoriated. This, the nurse assures me, was not the case yesterday, and she attributes this to the child's extreme restlessness at times. A linseed meal poultice was ordered to the navel—and I put the child under chloroform for half an hour.

Noon.—The child has had several of its usual screaming fits, but has slept a good deal since last visit.

4 P.M.—Has not slept since one o'clock ; is very restless, and swallows with increasing difficulty—part of the milk given being usually vomited, or at least regurgitated along with some tough viscid phlegm. Rigidity of the mouth as before. No change in the symptoms. Ordered repeated small doses of sp. camphoræ. Bowels open, and has made water. Chloroform again given to full sleep for half an hour.

8½ P.M.—Much the same. Has only had the camphor twice, and the nurse thinks it did not swallow it Has had two very long and severe fits of screaming, with such lividity that suffocation seemed almost inevitable. Chloroform given again for half an hour.

10½ P.M.—Has slept quietly, with occasional startings, ever since last visit, till about ten minutes ago. Symptoms continue much the same. Skin warm and moist, and the head perspires copiously. I gave it several teaspoonfuls of milk, which it swallowed pretty well, but vomited again immediately ; has passed water. Respirations still 80 or 90, and the countenance becomes very livid as a fit of screaming comes on, but the hands are not so much clenched as before. Chloroform again given for half an hour, during which time the breathing is reduced to 60 in the minute, and the mouth can be opened a little by the finger.

1st December, 8 A.M.—The child slept more than an hour after last visit, then awoke screaming, and continued to do so for two hours with scarcely any intermission. During this time the mouth was firmly shut, and the respiration at times so difficult that death was momentarily expected. When this had abated milk was given, a few drops at a time, and a drop or two of tr. camphoræ given three times ; but this produced so much convulsive choking and

spluttering, that the nurse ceased to give it. The infant is evidently weaker; the compression of the mouth, and other symptoms, continue the same. I gave chloroform again for quarter of an hour, during which time I could open the mouth without any trouble.

After this the infant remained at rest for an hour or two, and then the fits recurred very frequently, though less strongly, and the child made gradually less and less noise during them. There was also now a very disagreeable putrid smell about the child. After this time it never swallowed anything, nor opened its eyes at all.

2d December.—Child much the same, and the smell very disagreeable; there was some bleeding or blackish discharge from the umbilicus. As the little patient was evidently sinking, it was not disturbed till after its death, which occurred on the

3d December, at 8 A.M.—Ninety-three hours after the rigidity of the mouth was first observed by the parents.

Assisted by Dr Watson, I made a post-mortem examination of the child, twenty-six hours after death. The body was very rigid, the hands strongly clenched, and very considerable sugillation all over the back and dependent parts of the body. The face showed nothing remarkable. The rag which covered the navel was covered with a considerable quantity of fetid dark blood; and although the general skin was unaltered, the cuticle covering the corrugated part of the umbilicus was separating from the cutis, which last was dark and evidently gangrenous.

On the peritoneal surface also of the navel there was a very dark part, about the size of a shilling; but there was no appearance of any peritoneal inflammation. The umbilical vein was filled with a clot of red blood about an inch and a-half long, and seemed healthy. The left umbilical artery contained only a little fibrine; but the artery on the right side was much diseased. On slitting it up, the firm inner coat was observed to terminate suddenly, about three quarters of an inch from where the canal was obliterated; and this last part was filled with a yellow tough sloughy matter, in which large quantities of irregular pus globules could be seen under the microscope. The surrounding cellular tissue, even where it was dark, did not appear to contain any pus. The small intestines were tinged, of a dark brown-white, the large ones and the stomach seemed very white and pale. The liver was healthy, and so was the heart.

The upper parts of the lungs were of a natural pink hue; the posterior portions were much congested, and had many very dark points on them. The congestion, however, in both lungs was exclusively confined to the dependent portions, and they showed nothing like apoplectic effusions when cut into.

The head and spine were not examined.

The points in this case which chiefly interested me are,—1st, The inflammation of the umbilicus (the effect, possibly, of some injury at the birth of this child) having ended in gangrene; 2d, The occurrence of trismus and other convulsive symptoms; and, 3d, The very evident relief of these distressing symptoms by the free inhalation of chloroform.

The condition of the right umbilical artery was certainly very peculiar. Arteries leading to gangrenous parts are usually filled with fibrinous and bloody coagula, only occasionally adherent to the inner walls of the tube. In the present instance a yellow pultaceous slough was all that could be found, in place of the two inner coats of the right umbilical artery, which terminated as abruptly as if they had been cut through before their solution into this pulpy yellow slough.

Authors say very little about the treatment of trismus nascentium. They tell us this disease is "invariably fatal," seldom extending beyond twenty-four or thirty-six hours in duration, and not alleviated by anodynes, or by tobacco enemata, or any treatment they have seen adopted.

MEETING VI.—*June* 12.—DR PATTISON in the Chair.

A communication was read from Dr Christie of Dundee, being a

CASE OF TWINS, WITH DROPSY OF THE AMNIOTIC BAG OF SECOND FŒTUS TO A LARGE EXTENT—EXTENSIVE ANASARCA—UTERINE INERTIA, &C. &C.

Having been previously engaged to attend Mrs C. on her approaching confinement (her first), I was requested to visit her on the evening of Saturday, 2d March. I found her anasarcous to an enormous extent—the areolar tissue of the whole body, up even to the eyes, being loaded with serum. In this state she had been for a month, gradually getting worse—her rest being interfered with by difficulty of breathing. The urinary secretion was greatly deficient. I ordered her a dose of Pulv. Jalapæ Comp. On Sabbath forenoon I found her more comfortable—the medicine having acted powerfully, and relieved considerably the feeling of distention. I ordered a very weak solution of supertartrate of potassa as drink. In the evening of Sabbath I was again sent for, and found labour begun—the os uteri being dilated to the size of a half crown. Vertex presenting, and parts well lubricated. Fearing the occurrence of convulsions from the suppressed state of the urinary secretion, I bled her to a moderate extent (the blood being afterwards found to be much buffed and cupped). The labour progressed slowly but steadily, until 2 A.M. of Monday, when a small female child was expelled. On giving the child to the nurse, I found that the abdominal tumour was not perceptibly diminished, and, therefore, expected a case of twins; but, on examination, I could discover no presentation —neither could I, by an ordinary examination, trace the insertion of the umbilical cord. I gave a cordial, and waited an hour, when no pains having occurred, I applied the binder, used friction, and gave, first, one dose of ergot, and then three others at intervals. Still no pains. I now suspected (three hours after delivery of first child) that I might have to do with a case of dropsy, complicating pregnancy, in which idea I was countenanced by no motion of another child having been felt by the mother—no fœtal sounds being audible—no presenting part, and a most distinct feeling of fluctuation being present on external tapping. Being anxious to have another opinion in the case, I requested the kind assistance of my friend, Dr Gibson, who at first inclined to the latter opinion. We decided that I should pass my hand into the uterine cavity to ascertain if the placenta were adherent, and to elicit any further information I could. Accordingly, having put our patient fully under the influence of chloroform, I passed my hand into the uterus, guided by the umbilical cord, on to the very fundus—passing a very tense and resisting body. I found the placenta detached, but closely compressed against the anterior wall of uterus by this fluctuating body, but which did certainly *not* convey to me—so great was its size and distention—the idea of a bag of membranes. Immediately on removing the placenta, however, a bag of membranes, with the head of a fœtus, could easily be felt. I ruptured them, and the head came down so freely (although the pelvis was deficient in space) that I could not manage to turn as we afterwards wished to do. The discharge of liquor amnii was to a very *great extent*—greater than I ever remember to have seen—saturating the bed and running over the floor. The uterus now resumed its expulsive efforts, though by no means very briskly; and, after a labour of three hours, a male child (dead, and partly decomposed—evidently having been dead some time) was born. The placenta came away quite naturally—the uterus, no doubt, now obeying the stimulus of the ergot previously administered. The kidneys almost immediately resumed their wonted functions; and, after a couple of doses of smart aperient medicine, the anasarca disappeared, and she recovered favourably.

Now, it occurs to me as singular, that the labour, so far as regards the first child, progressed so favourably, and on its birth the uterine action became so completely suspended, notwithstanding the administration of repeated doses of

ergot, until the evacuation of the liquor amnii (the cause, apparently, of the anasarca and the uterine torpor at same time.)

It is also to be remarked, that no flooding occurred after the separation of the placenta of first child, although the uterus still remained uncontracted.

The presentation, too, of both children by the head, is not according to the usual rule in such cases.

CHLOROFORM IN PUERPERAL CONVULSIONS.

Dr Keith stated some particulars of a case of puerperal convulsions treated by chloroform. A lady, about the end of the seventh month of her first pregnancy, was suddenly seized with a convulsive fit, after suffering more or less for some days from headach, and slight feverish symptoms. The first fit occurred three hours before Dr K. saw her, and in the interval she had three others. She had been bled, and the bowels had been freely opened. The fits ceased for a time, but after four hours they again recurred, and four followed each other in the course of less than an hour, without any return to sensibility during the intervals. The bleeding was repeated very copiously, but seemed to have no effect in stopping the fits, or even in rendering them more moderate. Just as the fourth fit was coming on the chloroform was administered. It had an immediate effect in restraining the convulsive efforts, and she soon became perfectly quiet. Dr K. had ascertained, on first seeing the patient, that the os uteri was quite shut, and there had been no sign of labour coming on up to this period. Now, however, from the motions and expression of countenance, while she was still under the chloroform, he was led again to examine as to the state of the uterus, and found that regular contractions had come on. The action of the chloroform was kept up, the labour went on steadily, and in six hours she was delivered of a still-born fœtus, without any recurrence of the convulsions. She slept for nearly an hour after the termination of the labour, when she awoke in a confused state, but perfectly rational, and almost free from headach. She made a most favourable recovery ; indeed she had not a single bad symptom from the time that the chloroform was administered. The urine was highly albuminous, becoming almost solid on exposure to heat. The coagulability gradually went off, and after a fortnight there was scarcely a trace of it.

ALLEGED ILLIBERALITY OF THE EDINBURGH MEDICAL CORPORATIONS.

The recent act of liberality, by which the Glasgow Faculty of Physicians and Surgeons have surrendered their exclusive right of practice in the four Scotch counties under their jurisdiction, and have conceded to all other regularly qualified practitioners the privileges enjoyed by their own licentiates, has given occasion to some comments in a London journal, partaking of that almost studied ignorance, which, on this side of the Tweed, has often been justly charged against English journalists and English medical corporations, as to the constitution, history, and practice of the medical institutions of Scotland.

The editor of the "London Medical Gazette," quoting in his number for the 13th ultimo, the late proceedings of the Glasgow Faculty, and borrowing the censorious propensities of another English contemporary, roundly rates the Edinburgh Royal College of Surgeons, as well as the University of Edinburgh, for their "strikingly contrasted illiberality," and their "very rigorous enforcement of questionable privileges," through means of which, "if the Edinburgh M.D.'s are prosecuted in England, the English members and licentiates are persecuted in Scotland." "The Edinburgh Corporations," he adds, "have now a fair example before them in the Act recently obtained by the Glasgow Faculty. If they fail to follow this example, so far at least as to concede a right of examination on the production of an English diploma or license, and still stand upon their ancient privileges, they must not complain if

the Apothecaries' Society occasionally put in force *their* rights against the Edinburgh graduates. It is merely an enforcement of the *lex talionis*. The London Colleges have taken the initiative in liberally throwing open their examinations to the possessors of British licenses or diplomas. The Faculty of Glasgow have gone still farther: they have instituted an *ad eundem* title; and we think there would be less envy and jealousy in the profession if this plan were more generally adopted. Under a fair dispensation of medical law, the license of one duly constituted college, should suffice for a title to practise throughout Great Britain and Ireland." P. 67, 68.

Medical reform is a hopeless prospect indeed, if those who direct public opinion will not inform themselves in the simple elements of the subject. The editor of the "Gazette" is not aware, it seems, that the College of Surgeons of Edinburgh has neither enforced, nor threatened to enforce, its privilege of exclusive practice for a century at least, and has lately surrendered even the form of a privilege in a new charter; that the Edinburgh College of Physicians has also for more than a century waived entirely the exercise of its exclusive privilege; that the University of Edinburgh has no exclusive privileges to waive or abandon; that the persecution of "English members and licentiates in Scotland" is a wild dream of his own, so far as the Edinburgh Institutions are concerned; and that practitioners licensed in England may for a century past have settled, and may continue to settle here, as free and uncontrolled as they may settle in the wilderness. We are far from desiring to detract from the praiseworthy conduct of the present Glasgow Faculty. But when they are merely correcting the errors of their predecessors, it is too bad that they should be held up as setting an example to the Edinburgh College of Surgeons, who set them and others that very example fully one hundred years ago. In point of fact it was the application of that College for a new charter, containing, among other changes, the abandonment of the privilege of exclusive practice, which led to a similar application on the part of the Glasgow Faculty.

With this altogether imaginary grievance, the editor has oddly and intricately mixed up a totally different charge, complaining that the Edinburgh medical authorities do not admit to examination for their diplomas the members and licentiates of England on the same liberal footing on which Scotch licentiates are received by the corporate bodies of London. But he appears to be unacquainted with the real state of the case. The London College of Surgeons and Apothecaries' Society may quite well admit to examination a licentiate or graduate of Edinburgh; because practitioners so qualified have undergone the whole curriculum of study required by the London bodies, and something more. The Edinburgh College of Surgeons cannot, without manifest injustice to its own licentiates, admit a London apothecary or surgeon to examination; because his education is relatively defective in some respects. And where is the hardship,—the "persecution"? An English apothecary or surgeon may practise with his London diploma, uncontrolled by any Scotch institution throughout Scotland, whether in town or country. The Edinburgh College does not force him to join it; if he seek, therefore, what is merely an additional honour in his case, is the College to be accused of persecution if it requires additional proof of merit?

There is more semblance of truth in the complaint, that the University of Edinburgh will not admit the medical education received at the great hospital schools of London as qualifying for examination for the Edinburgh degree of doctor of medicine; and to the editor of the "Gazette" it naturally seems odd, as it must to many others, that, while these schools are not recognised, the University recognises, nevertheless, the schools of University College and King's College in London. We wish very much that the editor had inquired into this anomaly, and aided the University of Edinburgh in its endeavours to get rid of it.

The University will not admit the London hospital schools, simply because, under its existing government, it cannot. It does not admit them, because, in

face of the expressed wishes and votes both of the medical faculty and of the senate, it has been prohibited by its legal superiors from doing so. An old and still standing law of the University, requires, that candidates for graduation shall study in the schools of universities possessing the right of conferring degrees. This is obviously a simple mode of insuring that the teachers shall be of high standing. It is, of course, easy to raise the cry of illiberality on account of the restriction. But it is not so easy to prove that a university which confers a title of honour, but conveys with it no other exclusive privilege except the right of wearing it, may not with great public advantage, and without any very particular private hardship, practise much more reserve in recognising schools and teachers, than corporate bodies who convey with their diplomas important exclusive rights or monopolies. It would be easy also to prove that serious injury to medical education has resulted from the opposite rule of some important corporate bodies, who, with a reckless liberality, have recognised all medical schools, even the most insignificant.

Every restrictive rule, however, like that of the Edinburgh University, must be apt to bear hard in some quarters. Accordingly, in the present instance, it has fallen heavily on the students of the great London hospitals. The University law necessarily admitted University College and King's College, when the charter was passed which expressly acknowledges these two colleges as integrant parts of the University of London. The Medical Faculty of the University of Edinburgh, perceiving the injustice and inconvenience of recognising these colleges, and not the older and equally eminent schools of the greater London hospitals, immediately recommended to the Senatus Academicus, that these schools should be placed on the same footing with university schools. The Senate passed a resolution in conformity with the recommendation of the faculty. But the resolution was annulled by the magistrates of the city, who were unhappily found, about five and twenty years ago, to possess the power of controlling the proceedings of the Senate, and who declined to pass the resolution in question, without additional measures, so fraught with danger, that the Senate unanimously refused to concur in them. There the matter still stands. If the editor of the "Gazette" and his London friends will aid the University in carrying out its intentions, by prevailing on Sir George Grey to send down a royal commission of visitation, with power to enforce the recognition of the London hospital schools,—which would be one regular course of procedure,—we can only assure him that the Senate of the University would consider themselves under great obligation. Or if he will obtain the admission of the hospital schools into the charter of the London University, to which they surely have at least as good and obvious a claim as to admission in their present state to the privileges of the University of Edinburgh,—the matter would be settled at once, and satisfactorily to all parties.

Meanwhile, let it not be forgotten, that the University of Edinburgh compels no man to take its degree. This degree has been in great request for a century. But, except the title, it conveys no professional privilege whatsoever which may not be attained in various other ways, more cheaply and more easily by every British subject. And it is a new day with us, when the University of Edinburgh is asked, from England, to grant on easier terms that, which, for some years past, she has been incessantly though falsely accused of granting with too great facility.

VARIETIES.

INTERNATIONAL COPYRIGHT.—At the third meeting of the American Medical Association, held at Cincinnati in May 1850, the report of the committee on an *International Copyright Law*, with a memorial ordered to be prepared at the last meeting of the Association, was read and accepted ; and the memorial ordered to be signed by the officers, and transmitted to Congress. The committee consisted of Dr G. B. Wood and Dr Hays, gentlemen favourably known

in this country by their important contributions to American medical literature. We are glad to find that the American Association has formally expressed its disapproval of the system of pirating editions of foreign works of merit, whereby authors are deprived of a legitimate source of profit, while the public is often presented with inferior and mutilated editions of their works.

Ethnological Museum of Philadelphia.—This museum, collected by Dr Morton, contains 1,468 crania, of which 867 are human, and 601 belong to different animals. Of the Caucasian race there are 226 crania; of the Mongol race, 8; of the Malay race, 34; the primitive American race, 410; the Negro race, 107; mixed races, 25. Of the skulls of insane persons, 2 belong to the Anglo-Americans; 1 to an Englishman; 1 to a German; 1 to an Irishman; 2 to Mulattoes; and 2 to Nigroes. Lastly, there are 7 crania of idiots belonging to different races. Each cranium in this unique collection is accompanied by an exact measure of its capacity in cubic inches.—*L'Union Méd.* June 18, 1850.

Guillotin and the Guillotine.—Guillotin, a very amiable physician, highly esteemed by his contemporaries, was associated with Franklin and Bailey in the royal commission on the subject of magnetism, and supported the plan laid before the National Assembly for the foundation of the Academy of Medicine. He was also charged by Louis, the Perpetual Secretary of the Academy of Surgery, to consider and render more perfect the instrument of decapitation proposed to the Constituent Assembly; and hence the unfortunate notoriety which has been attached to his name. Medical men, however, should seize upon every proper occasion to protest against the *injustice of this celebrity.*—*Gazette des Hôpitaux*, June 4, 1850.

Physicians and Apothecaries.—At the late meeting of the Pennsylvania State Medical Society, the following resolution was adopted:—Whereas it is asserted and confidently believed by a portion of the public, that it is the practice of some physicians and apothecaries to enter into a collusive arrangement, by which the former are to receive a percentage upon all prescriptions sent to the latter, and in this way bring dishonour upon the medical profession; therefore, Resolved, That this society regards all collusion between physicians and apothecaries, whether with a view to pecuniary profit or patronage, as opposed to every principle of that moral code which the profession have adopted; and that no physician, known to be guilty of such a collusion, should be entitled to the confidence and professional intercourse of medical men.—*Boston Medical and Surgical Journal*, May 22, 1850.

Knighthood of Dr Charles Hastings and Dr Robert Carsewell.—Her Majesty has recently conferred the honour of knighthood upon these gentlemen, the former of whom is well known and highly respected as an accomplished physician at Worcester, and as the President of the Provincial Medical and Surgical Association; while the latter has well earned the distinction conferred upon him by his laborious and successful investigations in pathological anatomy.

Chair of Surgery in Glasgow University.—Dr James Adair Lawrie, of Glasgow, has been appointed Professor of Surgery in the University of that city, in room of Dr John Burns, who perished in the wreck of the Orion steamer, off Portpatrick.

Medicine at Rome.—The Pope has just established an Académie of *Orthodox Medicine*, of which he wishes himself to be patron and protector; while its members are to be physicians distinguished for their scientific attainments and devotion to the Catholic faith.

Cholera.—Three cases of malignant cholera are said to have been recently admitted into St Bartholomew's Hospital, and two or three others are mentioned in the Registrar-General's weekly report.

The London Medical Times.—This journal has commenced a new series by exactly copying the "Lancet" in its size of paper, type, and getting-up generally, a species of plagiarism very unusual among scientific periodicals. The adornment of the exterior of the journal has not been accompanied with commensurate improvement of its contents.

Longevity.—Jean Kolombeski, a native of Ostrowa in Poland, entered the French army in the year 1774, when forty-four years of age. He served in all the campaigns of the Revolution and Empire, in different infantry regiments, was wounded in 1814, and on the 21st May 1850 was admitted to the Hôtel des Invalides of Paris. Having been born on the 1st March 1730, Kolombeski is now in his *hundred and twenty-first year.* He has served in the army for *seventy-five* years, and has shared the dangers of *twenty-nine* campaigns. He enjoys good health, is strong and well-built, and looks like a man of seventy or eighty. The governor of the Invalides has ordered that in future all soldiers of a hundred years of age shall, on admission, be ranked as officers, in testimony of respect to their great age, and the long services they have rendered to their country.—*Gazette des Hôpitaux.*

MR ORR'S RECLAMATION.

In the "Dublin Medical Press" of July 17th, Mr Orr reclaims against the review of his "Principles of Surgery," which appeared in our July Number. We see no reason to abandon the objections which suggested themselves as to the form, nature, and merits of the work; and our antipathy to the "medicated drawers" is rather increased by the author's description of their salutary effects. "Sponginess of the gums and tenderness of the teeth, on pressing the jaws, together with a brassy taste, and moderate mercurial fœtor on the breath, are quite sufficient indications that the mineral is producing a satisfactory effect." These are the principles of the *modified mercurialists;* for our opinion regarding them, Mr Orr may consult with advantage our May Number, p. 460, *et seq.*

In "Justice to Ireland," we must dispel other delusions of Mr Orr. Professor Miller has had no connection whatever with this Journal, except as a contributor of a case of Inguinal Aneurism (p. 318, April No.) With the opinions which former editors of the Journal may have formed and expressed regarding Professor Miller's "Principles of Surgery," or regarding any other subject, the present conductors have nothing to do; their responsibilities commenced on the 1st January 1850. They consequently feel that they may, without inconsistency, abide by the verdict which the reviewer has pronounced upon Mr Orr's duodecimo.

BOOKS RECEIVED.

Observations and Essays on the Statistics of Insanity. By John Thurnam, M.D. London: C. Gilpin. 8vo.

The Medical News and Library. June 1850. (American Journal.)

Gout: Its Causes, Cure, and Prevention. By Abraham Toulmin, M.D. London, 1850.

The Diseases of the Breast, and their Treatment. By John Birkett, F.R.C.S. London, 1850.

A Practical Treatise on Diseases affecting the Skin. By the late Anthony Todd Thomson, M.D. Completed and Edited by Edmund A. Parkes, M.D. London, 1850.

On Consumption, Indigestion, &c. By G. Calvert Holland, M.D. London, 1850.

Cases Illustrative of the Cure of Consumption and Indigestion. By G. Calvert Holland, M.D. London, 1850.

The Chrono-Thermalist. July, 1850.

The Bath Waters: Their Uses and Effects in various Chronic Diseases. By James Tunstall, M.D. London, 1850.

The New York Journal of Medicine. May, 1850.—(We hope that this exchange will, under the new arrangements, go on regularly.)

Braithwaite's Retrospect of Medicine. Jan. to June, 1850.

Harvey on the Enlarged Tonsil and Diseases of the Throat. London, 1850.

Präger Vierteljahrschrifft für die Praktische Heilkunde. Band i. bis iv. Prag., 1849.

Annual Report of the Royal Edinburgh Asylum for the Insane. 1849.

Observations on Chronic Hydrocephalus. By Francis Battersby, M.B., &c.—(From Edin. Medical and Surgical Journal.)

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr Nimmo of Dundee, and Dr Kilgour of Aberdeen. Others have been privately acknowledged.

Part First.

ORIGINAL COMMUNICATIONS.

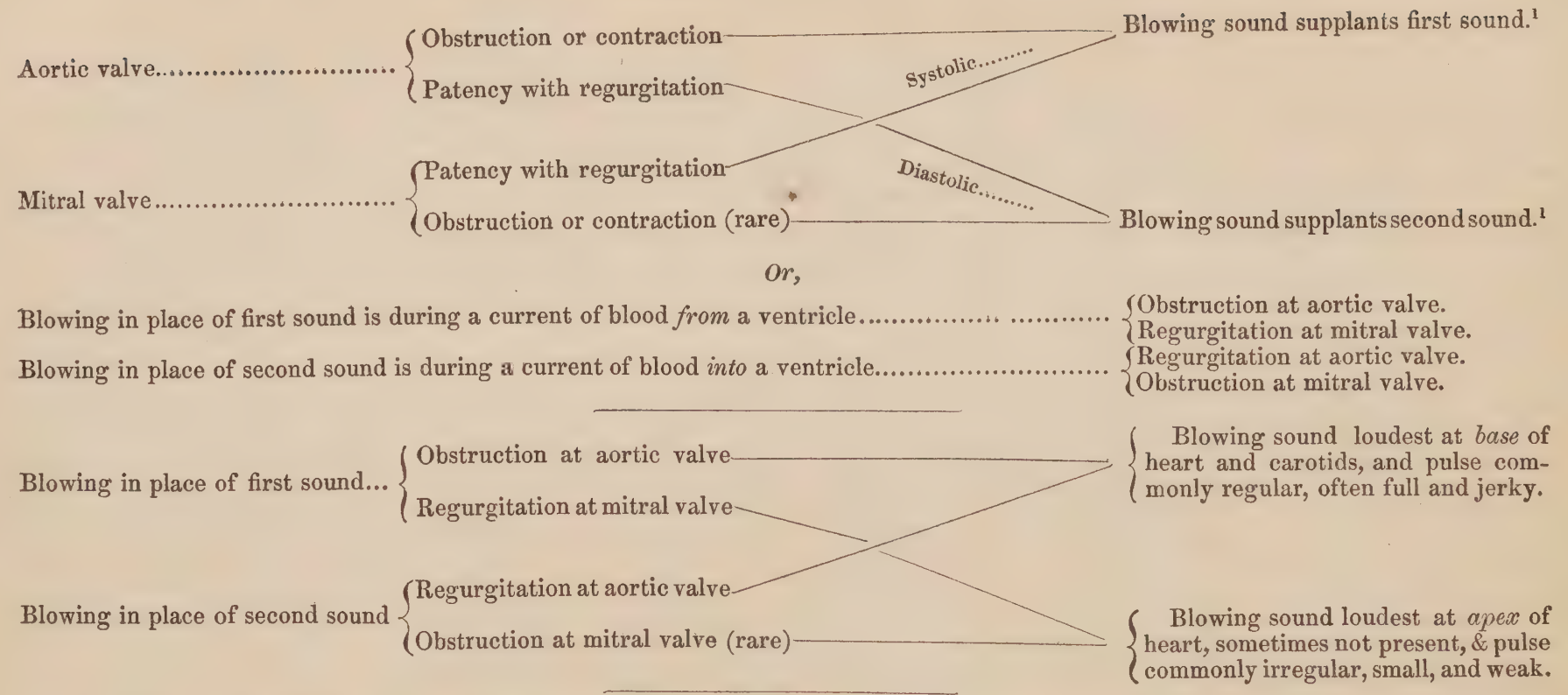
ARTICLE I.—*Practical Remarks on Disease of the Heart, and the Dropsy following it.* By ALEXANDER KILGOUR, M.D., one of the Physicians to the Aberdeen Infirmary.

IN the thirty-third Number of this Journal, or that for September 1843, I published some short "Notes on Diseases of the Heart," in which I stated as the result of my experience—but submitting my opinion with every deference to that of others—that the "*bruit de soufflet*" is of value as a sign to us, when permanently present, that disease does exist *somewhere* in the heart, pericardium, or large vessels, but that it is no sure guide to the special site of the lesion.

It was my conviction that authors, and particularly young authors, who meditated much on their cases at home, and were so ready to explain everything on mechanical principles, were apt to refine a little too much, and lay too great stress on a diagnosis from the abnormal sounds, call them by what name they may, alone.

Nearly seven years' longer experience, in which I have seen many additional cases of heart disease in hospital and private practice, has not brought me to materially alter that opinion. I allow that we may often make a pretty good *guess* as to the seat of the disease, but that is nearly all. "Guesses at truth," however, are often interesting and even instructive; and as an easy mode of impressing on my students in the clinical class the usual views, or guesses, as to the seat of the morbid sounds of the heart, and probable condition of the parts causing them, I have been in the way of exhibiting the following diagram. I have reason to think it has been convenient, for I have been applied to for copies of it by several of my medical brethren. There are not many—few, perhaps, but those who make heart disease their hobby in their closets or in practice—that can always carry in their minds, or bring to their recollection, the seats and causes of the abnormal sounds of the heart as given by the writers on these diseases; but a copy of this diagram, which can be easily made on a slip of paper, and carried in the pocket-book, will at a glance give the practitioner the information which has passed from his memory.

DIAGRAM OF MORBID SOUNDS OF THE HEART.



Purring tremor (*fremissement*) is *felt* sometimes, in disease of the valves, by the hand laid over the heart.

Purring sound (*frottement*), or creaking, or see-sawing sound, is heard in disease of the pericardium.

[1] Similar sounds arise from disease of the corresponding valves of the right side; but disease of the left is twenty times more frequent than of the right.

It will be observed that the second form of the diagram is merely another way, so to speak, of stating the same thing as in the first one. But the third is essentially different in its object, having reference to another sign than that depending upon the sounds. Perhaps some will object to the indications derived from the pulse, as not being deserving of so much importance as I am inclined here to attribute to them. I give them, however, only as farther aids to our guesses as to the true seat of the disease; and I must say, for my own part, that, having satisfied myself by my ear, and by such signs and symptoms from the condition of other organs as I can get, that heart disease does exist, I rely more on the state of the pulse than on anything else as a guide to the exact seat of the lesion.

When so much has been written on the treatment of the diseases of the heart, it may be something like going over a more than twice-told tale to offer any remarks on that subject. I shall, however, be brief, and the want of novelty will perhaps be atoned for by the fact, that I speak from the results of considerable experience, and that I offer these observations to the young practitioner chiefly.

If any one were to ask me, "What can you do to cure disease of the heart?"—I would honestly answer at once, "Nothing." But if any one were to ask, "What can you do to ward off the symptoms or feelings that too often become concomitants of that disease?"—I would say that, next to quietness of mind and body, and equable temperature, and moderate or rather low living, I know nothing equal to the use of small doses of colchicum wine, weak saline purges, and inserting a seton over the region of the heart. It will not do to trust to these when the severe sequelæ of heart disease—distressing dyspnœa, hæmoptysis, or dropsy—have more than once occurred; but I know no means equal to them in warding off these latter. I could mention several instances where this treatment has proved most beneficial. One case has been repeatedly under my notice. It was a female, a servant, past the middle period of life, who applied first for a slight attack of anasarca, arising from heart disease. The effusion was soon removed, and she was sent out of hospital with a seton in the side. She kept it open for some time; but, thinking herself well, withdrew it. In a few days after she returned, begging that it might be re-opened,—which was done; and painful experience has more than once taught this woman, in the course of eight years, the necessity of keeping up a discharge from a seton, renewed when necessary, and aiding its influence by the occasional use of the above medicines.

It has often been doubted whether blood should be abstracted in cases of advanced heart disease in elderly persons, when labouring under a sudden paroxysm of violent dyspnœa, giving the feeling of almost immediate suffocation. No doubt, mustard poultices over the heart, and the immersion of the feet and legs in hot water holding some mustard in it, will give some relief; but no means will be

found equal to abstracting a small quantity—from two to four ounces —of blood from the arm.

In the treatment of cardiac dropsy, every practitioner has his favourite remedy, taken from the class chiefly of diuretics or purgatives, or both. Of the former, I have found the following combination the most efficient. It pumps the patient out, so to speak, sometimes in a few hours; and it often will do so in repeated attacks of the anasarca.

℞ Infusi Digitalis, ℥iv. Acetatis Potassæ, ʒij. Spiritus Ætheris Nitrosi, ʒij. Aquæ Cassiæ, ℥iss. Capiat cochleare magnum quartâ quâque horâ.

At last there comes an attack in which this and other diuretics cease to act, and we must then fall back on purgatives. Of the latter, unquestionably the most powerful is elaterium. But there surely must be a very great diversity in the strength of this medicine. Some practitioners, from the days of Sydenham, and long before him, downwards, appear to have given it in the dose of two grains, or even more; but I have found a single pill, according to the following formula, generally very powerful:—

℞ Elaterii, gr. j. Extracti Colocynthidis Comp., ℈ij. ss. Extracti Hyoscyami, gr. xij. M. Divide in pilulas xij. Capiat unam nocte maneque.

The great objection to the elaterium is the intense sickness, even in this small dose, produced by it. Do the large doses produce less sickness than the smaller? It may be so; but, in the few instances in which I have tried large doses, the sickness was not less. Is there any mode by which this sickening property in this valuable medicine could be removed? The same effect was found by the ancients in the well-known and much-used, but undeservedly now almost discarded, hellebore; and for the sickness occasioned by it we find it recommended (*vide* "Oribasius," lib. viii., cap. v.) that, amongst other remedies, the patient should be entertained with a funny little story, or be tossed, like Sancho Panza, in a blanket.[1] Something more efficient than the former, and less disturbing than the latter, would be a desideratum.

It is the opinion of some that the elaterium acts as a diuretic, as well as a hydragogue cathartic. I remember, when in consultation with Dr Adams, of Banchory, in a case of cardiac dropsy, having my attention called by him to a formula, where the elaterium was combined with a diuretic, which he had seen prescribed with very great success. I detected it at once as a formula given in Ferriar's valuable "Medical Histories." It acts most powerfully by stool and urine (being composed of several of the most powerful of the diu-

[1] My learned friend, the translator of Hippocrates and Paulus Ægineta, writes, in a note, in his translation of the latter, "shaken in a garment;" but mine, I assert, is the more spirited version.

retics, along with the elaterium); but I always found it to cause much and violent sickness.

℞ Extracti Elaterii, gr. ij. Spiritus Ætheris Nitrosi, ʒij. Tincturæ Scillæ. Oxymellis Colchici, āā ʒss. Syrupi Rhamni, ʒi. M. Ft. Solutio. Capiat drachmam unam ex aquæ pauxillo, ter, quarterve in die.

The combination of a bitter purgative with a saline one composed of the vegetable alkali and a vegetable acid, is in my experience much more efficient than any single purgative, or than a bitter with a salt formed of a mineral acid. The old compound powder of jalap is a well-known instance of a mixture of this kind, and is still one of our best purgatives in all dropsies where this class of medicines may be suitable. Ferriar used, as did also Home, a combination of half an ounce of the bitartrate of potass with two grains of gamboge. The infusion of senna with bitartrate of potass is also an old-fashioned and valuable remedy; but the insolubility of the salt is an impediment to the efficiency of this formula. The senna infusion with tartrate of potass, or with the tartrate of potass and soda, is not liable to the same objection; and the advantage of the frequent use of this combination in cardiac disease having a tendency to dropsy, or in the dropsy itself attending that complaint, has been in my hands, and those of my brethren to whom I have recommended it, so unequivocal, that I can speak for it in the highest terms.

The preparations of mercury have proven, no doubt, very successful in the treatment of this form of dropsy, and consequently many practitioners give them a preference. Without wishing at all to detract from the merits of this most valuable agent, I must confess that, in chronic diseases of the heart, I have the same objection to it, and founded on the same grounds, as was that to the celebrated Dr Fell, or, to use the more classic words of Martial,—

> "Non amo te, Sabidi, nec possum dicere quare;
> Hoc tantum possum dicere, non amō te."

I do not like the mercury, and cannot speak from experience of its efficacy in cardiac dropsy.

There comes a time in the treatment of this complaint when not only diuretics in all forms, but even purgatives, cease to remove or even to keep in check the anasarca. And this brings me to speak of another mode of treatment, which often proves palliative for a time,—viz., puncturing the lower extremities, and thereby draining off the fluid.

This is not a new mode of treating the disease, though it has at various times fallen into unmerited neglect, and perhaps at the present time more so than at any other. Freind, in his "Historia Medicinæ," refers to the passage in Ætius which treats of this method of curing dropsy. Ætius is quoting from Asclepiades, and says that "an incision is to be made in the internal part of the leg, about four fingers' breadth above the ankle, and that it is to be of

the same depth as in venesection. A small quantity of blood flows first, and then there is a continuous discharge of water; and, without inflammation, the wound remains open until the whole dropsy has run off, no internal medicine being used." Freind continues, "Ipsa operatio ab Hippocrate memoratur, et ab ejus temporibus usque ad hunc diem multo sæpe cum successu adhibita est. Aliam puncturæ, scilicet per acum, viam proponit Sylvius de la Boe, hanc a se primo excogitatam glorians; quanquam evidens sit ea omnia ex hac descriptione desumpta esse, atque totidem verbis ab Avicenna expressa." And then he adds, "Verum haud sola hæc inventio recens dicitur, quam antiqui nobis medicinæ scriptores diserte tradiderint. Id autem omnibus in chirurgia vel minimum versatis constat, lanceolam longe præferendam esse cuivis acui, in aperiendis iis, quæ anasarcam comitantur, tumoribus."—("Historia Medicinæ," p. 385: Lond., 1733.) Mead commences his notice of the treatment of dropsy ("Monita," p. 130: Lond., 1771) with an account of this operation. He directs an incision to be made, two fingers' breadth above the ankle, down to the cellular membrane, and no further; and he orders the leg to be fomented with a decoction of emollient herbs, to which some spirits of wine and camphor have been added. He tells us that he has often found this mode of treatment, not only in this disease (anasarca), but also in ascites, of great service, and sometimes curative, the water running out for many days to an extent to exceed all belief. He carefully cautions us to support the patient's strength under a serous discharge from this or any other wound. He then gives a case, apparently hopeless, of anasarca, combined with ascites, where, by a wound made in this manner in each leg, followed by a combination of bitters, squills, and such purgatives as elaterium, calomel, and jalap, the patient recovered, and died five years afterwards of another disease.

This method of treatment is also brought under our notice by Heberden, in his "Commentaries," but not with so flattering hopes of advantage. He says,—"When these and other diuretics have failed, as is often the case, some have attempted to draw off the water by incisions in the legs, and even by cantharides blisters applied to them; and sometimes blisters take place spontaneously on the legs of dropsical persons. By these means," he continues, "I have seen not a small quantity of water drawn off, but never the disease cured ('morbum autem nunquam sanatum');—but they gave a brief respite, and eased the patient a little." He then notices the great objection to this mode of treatment—the difficulty of healing, and the danger of these wounds,—"Odiosum est quod hæc vulnuscula interdum fiunt ulcera sanatu difficilia, vel periculosa, quanquam bis aut ter quotidie pannis laneis ex aqua calida expressis foveantur. Fomenta hæc utilia sunt ad aquam copiosius evocandam."—"Commentarii," p. 191: Londini, 1807.)

I have here quoted the opinions of these very practical men as a

test, so far, of the value of the treatment by incisions and puncture. I have no hesitation in saying, with both Mead and Heberden, that I have not only often seen it of great benefit to a patient labouring under anasarca, no matter what the pathological cause of that disease, but have also in some cases seen it prove a cure, or, at any rate, that, after recourse had to it, internal medicines, which previously exerted no effect on the dropsical effusion, began to act, and the anasarca did not reappear.

I do not, however, recommend it until all internal remedies have failed. I never have recourse to it till it has become, to use Mead's expression, "unica spes salutis;" and I do not forget the qualification he makes, "et ea dubia." Still I am decidedly of opinion that, whether as a palliative or as a hope of cure, it deserves more notice than now-a-days it seems to me to get.

There are two modes, as stated above, in which the opening for draining off the fluid may be made,—either a puncture with the lancet, or with a good stout sewing needle. From a single puncture by the latter, a quantity of water will sometimes run out so as to pass through the bed in a few hours, and require to be collected in vessels placed below it.

Heberden has evidently seen the danger attending this mode of treatment, though he does not say specifically in what it lies. He tells that the wounds are difficult to be healed: but that we need not fear. He also says they are "periculosa." Now, the danger consists in the inflammation of an erysipelatous character, that not unfrequently takes place, extending up the limb, sometimes bringing on *sphacelation*, and for the most part ending in the death of the patient. But if the medical attendant would be satisfied with one incision, and one, or at the most two, punctures, in the proper place, erysipelas is not so apt to occur. The state of the skin—its low vitality during the existence of the dropsy under it—is singularly favourable for this form of inflammation; but a small clean cut, or one or two punctures,—and, if there be two, at a distance of not less than two inches from each other,—is not so likely to be followed by this as when, in the anxiety to get the water all off speedily, several have been made. The writers whom I have quoted all recommend fomentations to be applied to the wound; but I have often found the evaporating lotion very successful in keeping down any disposition in the part to erysipelas. Frequent ablution also, with tepid water, and the removal of any cloths that may be soaked with the serum that has run out, will serve to prevent, so far, any irritation from being set up in the skin.

When the operation, even with the needle, has to be often repeated, as is sometimes the case, for the ease of the patient, especially in cardiac dropsy, there is sometimes a degree of inflammatory action set up in the cellular tissue by which its interstices become obliterated, and the skin and it become quite closely adherent to the fascia below. I remember this occurring in a gentleman, who had

been kept alive for some months by punctures, and who was at last dropsical everywhere, except below the knees, where the adherent and thickened integuments would not distend,—thus giving him, however, not a little pain. It has of late been recommended to have recourse to this treatment much earlier in the disease than was the custom with the old practitioners; and it has been said that the chances of success from it are much greater than when the patient's strength has been exhausted, and the vitality of the skin has been impaired by the long existence of a dropsical effusion. This reasoning is fair; but, whilst admitting it, one practical point may be mentioned, which is, that the fluid does not flow so freely from the incisions as when the areolar tissue has become more open and less resilient by the long existent pressure of the effused serum. And, little as the danger of erysipelatous attacks may be from puncture of the skin in the early stage of anasarca, still there are few that will subject the patient to this chance of danger until all the other more usual and often successful modes of treatment have been tried in vain.

As to another topical means of treatment in this disease—viz., the application of blisters to the anasarcous legs, with the view of draining off the serum—I would have scarcely thought it necessary to speak, believing that almost none, now-a-days, would adopt this practice; but lately I met a very intelligent country practitioner, who told me that he occasionally had recourse to this application. Sydenham has condemned the practice, and branded it as a favourite application of empirics; and he states that "blisters entirely extinguish the natural heat, already almost overpowered by the water and deficiency of the animal spirits, and bring on a gangrene—too common in such cases." Sydenham's authority (and, by the way, he also condemns acupuncture) is great; but I have seen a small vesicle, which had formed on one of the lower limbs, burst and drain off the whole serum from a person affected with extensive anasarca. The case alluded to is one of interest otherwise, though more properly suited to an Article on renal dropsy. The patient, a lady, had the most albuminous urine I ever examined. Every remedy for dropsy had been tried by her medical attendant but mercury, and it was only left to me to suggest that, before the patient died, this medicine, which I had never seen more dangerous in this form of dropsy, as has been asserted by writers, than in any other, should be cautiously tried. This was done. Two days afterwards the vesicle above alluded to showed itself and burst, and our patient, much to our surprise, recovered, and is still in good health, —seven years after the above attack. What nature does may not always be successful in the hands of art; and the exciting of a vesication, by means of a blister, is much more likely to be followed by dangerous, than by curative, effects in all anasarcous limbs, from whatever cause the dropsical effusion may have arisen.

Article II.—*Case of Fibrous Tumour of the Dura Mater.* Communicated (through Professor Syme) by John Lyell, M.D., M.R.C.S.E., Newburgh, Fife.

Although cases analogous to the following are by no means uncommon, and the subjects of them almost uniformly beyond the pale of medical relief, yet the distressing march of symptoms, their peculiar features and grave import, will always interest the medical observer, and furnish occasionally a fit subject of record. Possibly the case selected, which was watched, from first to last, for the space of nearly two and a-half years, may be accepted as such, and briefly given.

W. B., a stout healthy-looking man, about forty years of age, was struck somewhat smartly on the ridge of the nose, a little to the left, in the beginning of July 1847; there was no abrasion of the skin, and scarcely any mark left; but he felt a little stunned at the time, and had some slight pain in the part for a fortnight after. Succeeding this, diminished sensation, and a feeling of numbness, were experienced on the side of the nose; and thus matters continued for about twelve months, except on exposure to cold, when the benumbed feeling increased, and was accompanied with more or less pain.

In July 1848—about a year after the accident—an aggravation of the feelings complained of took place. The pain and torpor, no longer confined to the left side of the nose, extended over that side of the face, but more especially assailed the subsidiary branches of the first division of the trifacial nerve; pains, simulating rheumatism, though evidently neuralgic, likewise attacked the neck and shoulders, stiffening the muscles, and interfering with the natural movements of the head and upper extremities. Although by no means regularly periodical, remissions and exacerbations of these symptoms were manifest, and, indeed, partook of this character almost to the last. One of these remissions occupied the latter months of the year; but, in December, an aggravation occurred, when, feeling an increase of the nasal pain on pressing the left canine tooth of the upper jaw, I removed it, but without affording relief.

During the early months of 1849 the patient's complaints rapidly gained ground; the sense of touch was greatly impaired over the left side of the face generally, but most marked over the eye, side of the nose, and upper lip; the eye also got sluggish in its movements, so as not readily to obey the will, and hence to induce diplopia, and the upper eyelid to droop, till complete fixedness of the eyeball, and ptosis of the eyelid, took place. Hitherto the patient had been able, with slight interruptions, to work at his employment—that of a handloom weaver; but the pains in his neck and shoulders, and stiffness in his arms, got so severe as forced him to desist. He still saw with the motionless eye, when the eyelid was raised; but in March, whilst in the Royal Infirmary, Edinburgh, where I had sent him for advice, vision became indistinct, and in the course of ten or twelve days complete amaurosis of the affected eye took place.

In May he was obliged to take to bed, from inability to uphold the weight of his head and arms when sitting up. On one occasion, while getting out of bed, which he occasionally did, his head, though supported by a splint passing down the spine, got a jerk or twist, when he was seized with pains shooting through the whole body, and speedily followed by general paralysis. This occurred in July, and he was so completely powerless that he was unable to

move finger or toe, or to evacuate the contents of the bladder or bowels. It was remarked, however, that although voluntary motor power was completely gone, sensation, save in those portions of the face previously affected, remained intact; it was also remarked that the lower limbs involuntarily retracted, on tickling the soles of the feet. In the course of eight or ten days, he began to recover power, particularly with the lower limbs and left arm; the right remained motionless, but sentive enough to the touch. He also regained power over the bladder and bowels, so as to discard the use of the catheter and enema syringe.

On the 15th of August he was seized with rather severe pain in the right side, in the site of the lower lobe of the lung, accompanied with bloody expectoration,—an attack, in fact, simulating pneumonia, but the physical signs were absent, and the sputa rather bloody than rusty. From this time the recovery of motive power over his limbs was arrested, and he began to lose what he had already gained; in the course of another month he was as helplessly paralytic as ever,—he could only move the fingers of the left hand, and the toes of the right foot. The bladder, unable to perform its function of evacuation, or the bowels theirs, he required the constant use of the catheter and syringe; the urine got fetid and ammoniacal, causing the vesical mucous lining to suffer, and throw out an immense discharge of ropy mucus, so as to choke up the catheter, and render the voidance of urine tedious and difficult.

The right eye, about the time he was seized with thoracic pain, began also to get dim-sighted, and, fading daily, in the course of three weeks had become as amaurotic as the left. Thus had the patient complete blindness added to his sufferings and helplessness, yet his intellect wavered not, and he bore all with the utmost fortitude. Though the right eye had thus lost its vision, its movements continued unrestrained.

About this time it was observed that he had in a great measure lost the sense of taste in the left half of his tongue, yet the temporal and masseter muscles of the same side still appeared to act. His breathing became almost solely diaphragmatic; the sense of touch or feeling over the general surface got gradually impaired; the muscles stiffened; hearing became dull; bed-sores appeared; and on the 27th December he died.

Autopsy.—On removing the calvarium, the brain and membranes appeared healthy; two of the glands of Pacchionus, towards the vertex, seemed preternaturally large. On removing the brain, a large tumour was found to occupy the left orbital fossa of the frontal bone; in size it seemed about half of an ordinary orange, but irregular in outline. The base of the left anterior cerebral lobe rested on, and was somewhat displaced by, the tumour, to which it adhered, but not very firmly; involved in it were the chief part of the orbital plate of the frontal, and that portion of the sphenoid bones containing the foramen opticum and foramen lacerum; these parts being, in fact, with their *in transitu* nerves and vessels, included and imbedded in the substance of the tumour. A portion of the mass passed over to the right, and embraced the right optic nerve. There was certainly less bony matter in the included osseous parts, rendering the orbital plate thin and elastic, but nowhere were the hard parts entirely removed. I have said, that the portion of the sphenoid forming the bottom of the orbit was included in the diseased mass,—this, in fact, filled up the bottom of the orbit; the back part of the eyeball was firmly adherent to, and could not be separated from, it; but the eye itself, anterior to this, appeared healthy. In the bottom of the orbit, the tumour was strong, firm, and fibrous; but within the cranium, though firm, much less tenacious, and, indeed, so friable, that the finger could readily be pushed through its substance. The tumour, with included portions of bone and back part of adherent sclerotic, weighed three ounces.

Remarks.—Although fibrous tumour of the brain and its membranes are known to spring up and exist, without any assignable

cause, yet—since the admirable paper by Louis, secretary to the French Academy of Surgery, wherein he graphically describes many such cases as the consequence of a blow on the head, or concussion of its contained parts—there can be no doubt that such are frequently the results of violence, particularly as regards those originating from the dura mater. Is it possible that, in the present case, the comparatively slight blow inflicted on the nose was the origin of the succeeding train of symptoms, suffering, and death? The patient himself ascribed his illness wholly to this; he was constitutionally sound, and enjoyed excellent health, but, accidentally struck on the nose, suffered, went on suffering, and died. The *post hoc, ergo propter hoc*, is often, no doubt, a fallacious guide in medicine; but, taking all things into consideration, I think it highly probable that the stroke originated the morbid growth, from which the patient died.

From a review of the symptoms, it seems evident that the disease had commenced in, or adjacent to, the foramen lacerum. The first division of the trifacial nerve, passing out here, gave in its branches the first indication of suffering; that the origo mali did not extend back into the cranium, at least as far as the Casserian ganglion, was evidenced by the fact, that neither the second nor third divisions of the same nerve were affected; hence the differential diagnosis. When the muscular movements of the eye became apparently sluggish, the diagnosis became more clear. The third, fourth, and sixth nerves, passing also through the same foramen lacerum, were subjected to the same functional interference. At this time I sent the patient to Edinburgh for advice; and, in a note to Professor Syme with the case, stated this as my opinion,—"A growing tumour in the foramen lacerum might account for all the symptoms." As the tumour continued to grow, the optic nerve got involved, followed by amaurosis. When it extended over and enveloped the right optic nerve, amaurosis of that eye also ensued. Pressure being exerted generally on the encephalon as the tumour enlarged, the function of that important organ was generally compromised; hence general paralysis, anæsthesia, and train of grave symptoms preceding death.

ARTICLE III.—*Origin of the Epigastric and Obturator Arteries by a common trunk from the Internal Iliac; with an inquiry into the amount of danger occasioned by various positions of arteries in the ordinary operations for Femoral and Inguinal Herniæ.* By P. REDFERN, M.D. London, Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

IN the summer of the year 1849, I met with the following very unusual case of origin of the epigastric and obturator arteries from the internal iliac, in a male adult æt. 27, five feet eleven inches in height, who died of hæmoptysis from tubercular deposition, and was

brought to the anatomical rooms. The arrangement of all the large vessels was carefully examined, and a drawing, from which the accompanying woodcut has been executed, was made at the time, the preparation being also preserved.

Arrangement of the vessels.—The abdominal aorta divides in the usual manner, on the left side of the fourth lumbar vertebra, into the two common iliac trunks. Of these, the left one was cut away in a *post-mortem* examination made before the body came into my possession; the right terminates by division into an external and internal iliac artery at the sacro-iliac synchondrosis, and is an inch and three-quarters long.

The external iliac artery follows its usual course, and has its ordinary relations. It is $3\frac{1}{6}$th inches long, and gives off from its outer side, immediately under Poupart's ligament, the circumflex iliac artery of its usual size, and having its customary course and distribution. The inner side of the external iliac, quite close to Poupart's ligament, furnishes a branch, $\frac{1}{24}$th of an inch in diameter, which, inclining inwards over the external iliac vein, meets with the spermatic cord, and ramifies upon it in the inguinal canal, thus representing the cremasteric branch of the epigastric artery. All the small vessels are well filled with ordinary injection, but no communication of the cremasteric branch, nor of the external iliac trunk, with the epigastric artery, can be found.

The internal iliac artery runs downwards without giving off any branch for $1\frac{2}{3}$d inch, reaching nearly to the upper edge of the sacro-sciatic notch. It then furnishes the following branches:—

1st, From its right side and in front, a common trunk for the epigastric and obturator arteries, or an epigastric artery which furnishes the obturator in its course.

2d, From its posterior part, an ilio-lumbar artery of large size.

3d, From the front of the artery, a little lower down, a common trunk for the partially obliterated hypogastric, the pudic, and middle hæmorrhoidal arteries, with a small vessel, which runs out of the pelvis at the upper part of the sacro-sciatic notch, giving off a small branch, which runs forwards on the surface of the obturator-internus muscle, and divides into two branches, the higher of which is not far from the usual position of the obturator artery, and anastomoses by small branches, with a descending branch of the obturator given off close to the obturator-foramen; whilst the lower division runs downwards and forwards to anastomose with a branch ascending from the pudic to join with the small descending branch of the obturator.

4th, The internal iliac gives off a small vessel, which passes obliquely upwards and outwards over the brim of the pelvis into the substance of the iliacus muscle, and anastomoses in it with the ilio-lumbar artery.

5th and 6th, Two lateral sacral arteries pass off from the posterior part of the iliac, the higher in position entering the first anterior

sacral foramen; the other inclining downwards, and furnishing branches which pass into the other sacral foramina.

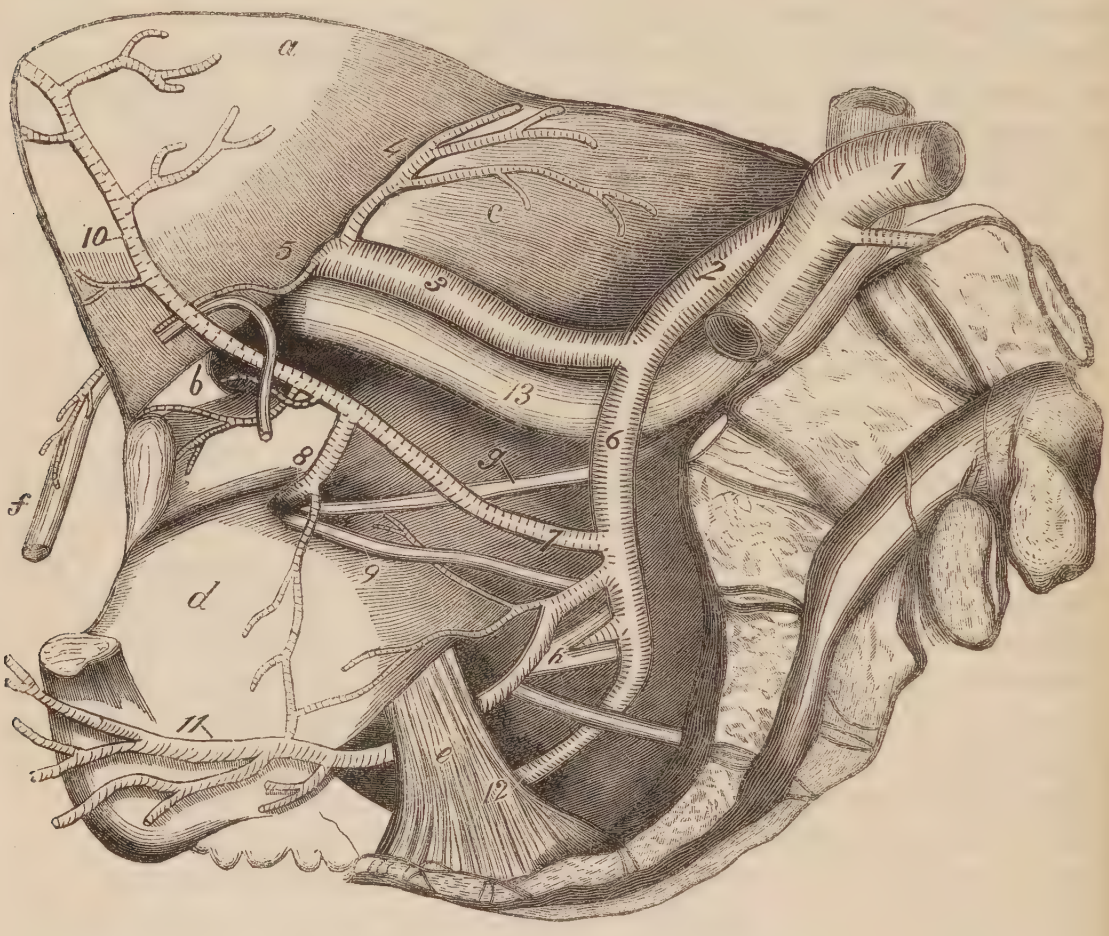
7th, The internal iliac divides, at the distance of $2\frac{1}{2}$ inches from its origin, into the gluteal and ischiadic arteries; the gluteal passing between the lumbo-sacral and the first sacral nerves, and escaping from the pelvis at the upper part of the sacro-sciatic notch, breaks up into branches at $1\frac{2}{3}$ inch distant from its origin; the ischiadic passing between the first and second sacral nerves, leaves the pelvis below the pyriformis muscle, and has its usual distribution.

The common trunk of the obturator and epigastric arteries, arising from the internal iliac very near the ordinary point of origin of the obturator, is $2\frac{1}{12}$th inches in length, and $\frac{2}{15}$ths of an inch in diameter. From its direction, it appears to be epigastric rather than obturator, for it follows almost a direct course, curving only very slightly downwards, in its passage to the crural ring, at which point it turns upwards, in the wall of the abdomen, like the ordinary epigastric. In its course along the side of the pelvis, the common trunk crosses obliquely over the small fourth branch of the internal iliac, then at an angle of 40° over the obturator nerve, and at its division it lies an inch above the obturator vein, which passes backwards from the obturator foramen to the internal iliac vein. After giving off the obturator artery at $2\frac{1}{12}$th inches distant from the internal iliac, the trunk continues in its original direction, very slightly diminished in size, and becomes the epigastric artery. This vessel crosses the centre of the crural ring at $\frac{1}{3}$d of an inch distant from the external iliac vein, and, applying itself to the posterior boundary of the inguinal canal, close to the inner edge of the internal abdominal ring, it has the vas deferens hooked round its outer side, and the spermatic cord, with the cremasteric branch of the external iliac artery, in front. At the distance of half-an-inch beyond the origin of the obturator artery, a small branch comes from the epigastric, passing through the crural ring, and terminating by ramifications on the surface of the pectineus muscle. A sixth of an inch further on, the epigastric furnishes another small branch, which is ramified on the posterior surface of the pubes, and at $1\frac{1}{3}$d inch from the origin of the obturator, the course of the epigastric is crossed by Poupart's ligament.

The epigastric artery is accompanied by two veins as far as Poupart's ligament, when one opens into the external iliac vein; the other is joined in front of the artery by two small veins which come from the posterior surface of the pubes, and run directly across the crural ring,—the vein, formed by the junction, opening also into the external iliac quite close to the other epigastric vein.

The obturator artery, of the same size as the epigastric, passes off from the common trunk exactly at a right angle, and, inclining downwards and inwards for an extent of an inch, it joins the nerve and vein at the sub-pubic canal, through which all these parts then pass in the usual manner. Before entering the canal, the artery gives off a small branch which runs a straight course upon the obturator muscle

to join with an ascending branch of the pudic, this anastomosis receiving also the small branches which run downwards and forwards from the internal iliac trunk.[1]



The woodcut represents the two last lumbar vertebræ and the sacrum seen in section, and the right side of the pelvis. The iliacus and obturator internus muscles remain in their places, covered by their respective fasciæ. A triangular flap of the abdominal wall, having the epigastric artery on its inner surface, is also shewn. The symphysis of the pubes, and a portion of the boundary of the obturator foramen, were removed in making the *post-mortem* examination.

a, Triangular flap of the wall of the abdomen.
b, Gimbernat's ligament.
c, Iliacus muscle.
d, Obturator internus muscle, covered by fascia.
e, Coccygeus muscle and small sacro-sciatic ligament.
f, Spermatic cord.
g, Obturator nerve.
h, Nerves forming the sacral plexus.

Vessels.

1, Aorta.
2, Right common iliac artery.
3, External iliac artery.
4, Circumflex iliac artery.
5, Cremasteric artery arising from the external iliac.
6, Right internal iliac artery.
7, Common trunk of the obturator and epigastric arteries.
8, Obturator artery giving a branch to anastomose with the pudic.
9, Obturator vein.
10, Epigastric artery furnishing a branch to ramify behind the pubes, and another to pass through the crural ring.
11, Pudic artery.
12, Ischiadic artery.
13, Right external iliac vein.

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[1] A few irregularities of origin and arrangement of the smaller arteries also existed, but none of these was of special importance.

An instance almost precisely similar is recorded by A. K. Hesselbach,[1] in which a common trunk for the obturator and epigastric arteries arose from the internal iliac as its third branch, and divided at an inch and four lines from the transversalis fascia. The obturator descended towards the front, to the obturator foramen,—the epigastric passed forwards on the inner side of the external iliac vein, as far as Poupart's ligament, where it turned up and ascended obliquely upon the posterior surface of the rectus muscle.

Dr Monro also met with a similar case, but he merely says, in speaking of it[2]—"I have a specimen in which the epigastric artery takes its rise from the obturator, and passes upwards and inwards to the rectus muscle."

Not having been able to find any other recorded case of the same kind, I believe that the one now given is the third instance in which this origin of these important arteries has been noticed, and the only one in which anastomosing branches were sought for, with a view to ascertain the causes or modes of production of such varieties. It is necessary to notice here, the occurrence of two epigastric arteries, one furnished by the external, the other by the internal iliac, and lying on different sides of the spermatic cord, as observed by Lauth, and mentioned by M. Velpeau;[3]—the origin of the epigastric artery from the femoral (Burns[4] and Quain[5]); from the superficial femoral (Burns); from the profunda femoris (Monro, Green,[6] Hesselbach, Tiedemann,[7] and Quain); or from the internal circumflex (Dr John Reid[8] and Michelet[9]);—the common origin of the epigastric, obturator, and circumflex iliac, or internal circumflex of the thigh from the femoral (Quain and Hesselbach);—the origin of the epigastric with the internal circumflex and circumflex iliac arteries (Hesselbach), or with either of these (Quain);—the origin of the obturator

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[1] Ueber den Ursprung und Verlauf der unteren Bauchdeckenschlagader und der Hüftbeinlochschlagader. 4to. 1819.

[2] The Morbid Anatomy of the Human Gullet, Stomach, and Intestines. By Alexander Monro, jun. Edinburgh, 1811, p. 427.—My kind friend, Mr Spence of Edinburgh, whose valuable papers on the arrangements of the arteries of the pelvis have added so much to our knowledge of their importance in operations, kindly undertook to make inquiry for me regarding the case which occurred to Dr Monro. Unfortunately, however, the preparation cannot be found, and Dr Monro has no recollection of its nature.

[3] Nouveaux Eléments de Méd. Opératoire. Paris, 1832. Tom. ii. p. 452; or Brussels edition, 1835, p. 392.

[4] Observations on the Diseases of the Heart, &c. Edinburgh, 1809.

[5] The Anatomy of the Arteries of the Human Body, with lithographic drawings and commentaries. By R. Quain. London, 1844.

[6] Cyclopædia of Practical Surgery; article—Arteries—Anomalies of.

[7] Tabulæ Arteriarum Corporis Humani. Tab. xxxiii., fig. 3.

[8] Anatomical Observations, in No. 128 of the Edinburgh Med. and Surgical Journal.

[9] Nouveaux Eléments de Médecine Opératoire. Par M. Velpeau. Tom. ii. p. 452.

from the profunda femoris (Green and Hesselbach); or from the superficial femoral (Monro and Burns);—and the origin of the internal circumflex from the external iliac (Burns); from the circumflex iliac or obturator (Quain); or from the epigastric (Quain and Dr John Reid.)

These comprise the whole recorded variations in the origin and connections of the epigastric artery, as far as I can ascertain; and it will be seen by a reference to the case which I have just related, that there is good reason to believe that the position of the vessel in all its varieties of origin may be explained by the enlargement of anastomosing branches, which probably always exist, at some period of development, between the larger trunks; and many of which were large enough, in the present instance, to admit ordinary coarse injection. Thus, the epigastric, generally arising from the external iliac artery, communicates by branches which ramify behind the pubes with the obturator artery of the internal iliac trunk, and in three instances it has appeared to arise altogether from the last-named vessel. Under such circumstances, it might be expected that a communication would exist between the epigastric of the internal iliac and the external iliac artery, and it is very possible that such a connection does take place, though, as in the present instance, the branches may not be of sufficient size to become evident after the use of coarse injection. This appears more probable when it is considered that the cremasteric branch was, in this case, still connected with the external iliac artery, though no communication existed between the former and the epigastric. No mention is made of the position of anastomosing vessels in the cases recorded by Hesselbach and Dr Monro, nor in the one mentioned by M. Velpeau as occurring to Lauth; and this is especially to be regretted in the latter case, as in it the supply of blood usually conveyed by the epigastric was furnished by two branches, one arising from the external, the other from the internal iliac artery. Further explanation is also required of the cause of the position of the common trunk of the two arteries, in the instance now recorded, not being that of the ordinary obturator, the position of which is very nearly occupied by a small vessel running forwards in close relation with the obturator vein. It is more than probable that this explanation would have been afforded by Lauth's case, if the epigastric branch, which arose from the internal iliac, had been fully described and found unassociated with the obturator.

By reference to the woodcut here given, it will be seen that a considerable branch connects the obturator and pudic arteries on the surface of the obturator internus muscle; and other modes of connection of these vessels have been seen by others—thus Haller[1] observed several instances in which their branches anastomosed in supplying

[1] Icones Anatomicæ. Gottingue, 1743. Fascic. iv., p. 32.

the penis, and two cases in which the branch to the penis was furnished by the obturator, an occurrence which, he states, was also observed by Lieutaud and Columbus. Mr Quain has shown the accessory pudic artery to be derived, in different instances, from the pudic in the pelvic cavity,—from the internal iliac,—from the external iliac by means of the obturator arising from the epigastric,—or from the epigastric artery.

Again, the internal circumflex artery sometimes arises from the external iliac, the epigastric or obturator; and the epigastric and obturator vessels have been found arising from the femoral, the profunda femoris, or from the internal circumflex arteries, and proceeding to their destination through the crural canal, in which position a small branch from the epigastric artery will be found in the accompanying wood-cut.

So it appears very generally to be true that varieties in the position or origin of the larger arteries result from the enlargement of their usual anastomosing branches with other vessels, the artery which occupies the normal position becoming at the same time proportionally diminished in its diameter. This is remarkably exemplified by the obturator artery, which, in different instances, arises from the internal iliac,—from the epigastric branch of the external iliac, or from the trunk itself,—or again, from both iliac arteries at once, by branches either of equal or of different sizes, as has been so beautifully demonstrated by Professor Quain and Mr Maclise; and a few other instances of removal of the epigastric artery from the external iliac, may give as complete an explanation of the manner in which its varieties become developed.

However great the interest which is excited by varieties in the position and origin of arteries when viewed physiologically, it can never equal in importance that which they acquire from their surgical relations. These are so important, in the case of the epigastric and obturator arteries, that all their varieties have ever been regarded with peculiar anxiety; and the estimate of the increased amount of danger which they may occasion under various circumstances yet remains a desideratum. To attain this, it appears necessary, 1st, to review those varieties of position in which a vessel of considerable size appears likely to be exposed to danger in the performance of ordinary operations, and, if possible, to ascertain the frequency of like positions; 2nd, to inquire in what proportion of such operations vessels of importance have been injured in actual practice; and, 3d, to determine the probable result of future cases of injury of these vessels, by a reference to the termination of those which have already been made public. In what follows, these inquiries will be pursued as far as our present imperfect data will warrant, and they will be limited to those arteries which may possibly be endangered by the ordinary operations for inguinal and femoral hernia.

Inguinal Hernia.—The epigastric artery has a remarkably constant position in all the various forms of inguinal hernia, lying on the inner side of the internal abdominal ring and the neck of an oblique hernia, and to the outer side of the neck of the sac of both superior and inferior direct herniæ. To this general rule there appear only two exceptions, one being in a case of internal labial hernia, recorded by F. C. Hesselbach;[1] the other, Lauth's case before named. In Hesselbach's case "the obturator artery arose from the inner side of the crural more than half-an-inch distant from the internal inguinal ligament (transversalis fascia), and running for the length of an inch obliquely downwards and inwards over the crural vein, bent suddenly, on the horizontal ramus of the pubes, towards the obturator foramen; from this bend arose the epigastric artery, which proceeded transversely inwards on the horizontal ramus of the pubes, behind the neck of the hernial sac, and ascended on its inner side to get behind the rectus muscle of the abdomen, accompanying the ligamentous remains of the umbilical artery which were close behind it."

The epigastric artery is carried inwards to the edge of the rectus muscle by large oblique scrotal herniæ of long standing, but in no instance has it been found in such a position as to be endangered by the division of the stricture upwards, a plan now almost universally adopted in all cases of inguinal hernia. Though, in ordinary cases, this artery could not be injured by the division of the stricture of an oblique inguinal hernia upwards and outwards, and that of direct inguinal herniæ upwards and inwards, it has been fully shown that these different forms of inguinal rupture cannot always be distinguished from each other, and, consequently, that such methods of operating must occasionally lead to fatal results. Moreover, there are other sources of danger; for if, in such a case as Hesselbach's above referred to, the stricture of an inferior direct hernia were to be cut upwards and inwards, the edge of the knife would be applied upon the epigastric artery, and a similar occurrence would take place in the division of the stricture of an oblique hernia upwards and outwards, in any instance like that named by Lauth and Velpeau.

The following cases of injury of important arteries, in operations for inguinal hernia, are on record:—

1. Scrotal hernia—Stricture cut by Sir A. Cooper[2] upwards and outwards; epigastric artery divided; hemorrhage stayed for four hours by pressure in the groin; death in ten hours after operation; abdomen distended with blood.

2. Inguinal hernia caused by the kick of a horse—Stricture cut

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[1] Disquisitiones Anatomico-pathologicæ de Ortu et Progressu Herniarum, cum Tabulis xvii. æneis. Wirceburgi, 1816, p. 15.

[2] The Anatomical and Surgical Treatment of Inguinal Hernia, First Part. London, 1827, p. 69.

upwards, and epigastric artery completely divided; both ends were tied; ligatures were brought away on the eleventh day; the wound was healed on the fourteenth, and the patient returned to his work on the sixteenth day after operation. This operation was performed by Dr Mackay, R.N., assisted by Mr Giraud of Faversham, who communicated the particulars to Sir A. Cooper.[1]

3. A small oblique inguinal hernia passing down by the side of the sac of an older hernia full of fluid—Stricture divided inwards; immediate and copious hemorrhage from the epigastric artery stopped by fainting; recurrence of very serious hemorrhage owing to a clot being forced out by vomiting; hemorrhage finally suppressed by lint thrust into the wound, and by pressure in the groin kept up for four days; lint removed without hemorrhage after four days; occupation resumed after five weeks. The case occurred to Mr Davie, but is recorded by Sir A. Cooper,[2] who states that he could give other and similar cases, had he the permission of the surgeons attending.

4. Oblique inguinal hernia—Division of the tendon of the external oblique muscle upwards and outwards for two inches, also upwards and inwards for three-fourths of an inch; complete division of the epigastric artery, at three-quarters of an inch from its origin, by the incision made upwards and inwards, including the inferior margin of the internal oblique and transversalis; no hemorrhage; death on the following evening from mortification of the intestines. Recorded by Mr Laurence.[3]

5. Direct inguinal hernia—Stricture cut upwards and outwards to a very slight extent, and not within three-fourths of an inch of the epigastric artery; division of the cremasteric branch of the epigastric; hemorrhage, the same evening, restrained by cold cloths; profuse arterial hemorrhage on the eighth day, ceasing after the loss of two pints of blood; death, a week after the last hemorrhage, from inflammation. Recorded by Mr Laurence.[4]

6. Scrotal hernia, of nine years' standing, in a patient æt. 64—Inflammation and suppuration of the whole scrotum; hemorrhage on the tenth day; castration for the purpose of securing the vessel; loss of above a pound of blood; gradual recovery. Recorded by Sir E. Home.[5]

7. Scrotal hernia of eight or nine years' standing—Considerable hemorrhage, soon after the operation, restrained by the application of sponge; recovery. Recorded by Mr William Chalmers.[6]

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[1] Op. Cit., p. 41.
[2] Op. Cit., p. 69.
[3] Treatise on Ruptures, Fifth Edition. London, 1838, p. 271.
[4] Op. Cit., p. 273.
[5] Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge. London, 1800. Vol. ii., p. 105.
[6] Medical and Philosophical Commentaries by a Society in Edinburgh. London, 1773. Vol. i., p. 413.

8. Scarpa says,[1] in speaking of wounds of the epigastric artery, "I have had the misfortune to be an eye-witness of this severe and irreparable accident, supervening upon an operation performed in a dexterous manner, and with the greatest facility."

9 and 10. Bertrandi writes,[2]—"I can affirm that I have opened the bodies of men who have died a few hours after this operation, although performed with great facility, dexterity, and quickness, so that the operators thought highly of themselves on that account; and they were quite astonished, and could not understand the cause of so unexpected death; but their astonishment ceased when they saw the abdomen full of blood, discharged from this artery (the epigastric) wounded.

11. Leblanc[3] also speaks of having seen the epigastric cut by expert operators.

12. Mennel[4] opened an artery in cutting the stricture inwards. The hemorrhage was considerable, but was controlled after a quarter of an hour.

13. Saviard[5] (Obs. 20) saw a hemorrhage which occurred half-an-hour after an operation, and was arrested by compression. Four or five *palettes* (sixteen to twenty ounces) of blood were lost.

Numerous cases are recorded where death resulted from wounds of the epigastric and other arteries in the operation of paracentesis. Mr Edward Ford[6] relates a case of Mr Pearson's, in which death took place in twelve hours, and a large quantity of coagulated blood was found in the abdomen; and at page 135 of the same publication, he speaks of a like case, where he stopped very serious hemorrhage by pressure of the integuments and muscles between the fingers and thumb for six hours. Mr Ford also knew of similar hemorrhage in the hands of other surgeons. Dr James Carmichael Smyth[7] gives the case of a man, æt. 36, who died in forty-eight hours from hemorrhage, caused by puncture of the epigastric artery in tapping for ascites; and another case, where profuse hemorrhage occurred, and the patient died some weeks after the operation. He also speaks of eight other cases which happened in the practice of various surgeons of his acquaintance. A fatal case is given in Kleinert's Repertorium, Nov. 1835, from wounding an anomalous branch of the epigastric. Ramsay[8] gives a fatal case from puncture of the abdominal branch of the circumflex iliac artery; and Allan

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[1] Wishart's Translation of Scarpa on Hernia, p. 125.

[2] Traité des Operations de Chirurgie, traduit de l'Italien. Paris, 1769, p. 29.

[3] Précis d'Opérations. Paris, 1775. Tom. ii., p. 129.

[4] See F. A. Waitz, Med. Chirurg. Aufsätze, vier Theil, p. 123.

[5] Traité des Hernies de A. G. Richter. Par J. C. Rougemont. Seconde Edition, Cologne. Tom. i., p. 324.

[6] Medical Communications. London, 1790. Vol. ii., p. 136.

[7] Medical Communications, vol. ii., p. 482.

[8] Edinburgh Medical and Surgical Journal, vol. viii., p. 282.

Burns [1] also speaks of wounding the same artery, or its accompanying veins, in paracentesis.

It appears, therefore, that serious hemorrhage is to be expected after wounds or division of the epigastric artery, and that no reasonable hope can be entertained that such cases will have a favourable termination without prompt interference by the surgeon. Mr Laurence's case of complete division of the epigastric without hemorrhage must be viewed as an exception to the rule, and it should not prevent the surgeon cutting down upon the divided artery and tying both ends, if possible, in every instance in which it is known to have been wounded, for a fatal result from internal hemorrhage is very likely to take place, even in instances in which no blood escapes externally. As a fatal hemorrhage may proceed from division of the cremasteric branch of the epigastric, such a case should be treated like cases of hemorrhage from the trunk of that vessel.

But the most important question to decide is, whether the epigastric or other large artery ever lies in such a position, in regard to an inguinal hernia, as to be endangered by the operation now commonly practised? Hitherto the epigastric has been situated either on the inner or outer side of the neck of the sac of every inguinal hernia which has been examined, and in no single instance has any large vessel been found on the upper part of the neck of the sac of any such hernia. Scarpa cut the stricture upwards in several cases of oblique and direct inguinal hernia in the dead body, and he says [2] that in all "the epigastric artery remained uninjured, although the incision was extended an inch above the inguinal ring." There ought therefore to be no risk of hemorrhage, in any case, from cutting the stricture directly upwards; but, in the second case before referred to, as recorded by Sir A. Cooper, the epigastric is said to have been completely divided by cutting upwards. On referring to the particulars of this case, it will be seen that the stricture was found from an inch to an inch and a-half distant from the external abdominal ring, and that it was divided, without exposure of the canal by the division of any part of the tendon of the external oblique muscle, whence it may reasonably be doubted whether, under such circumstances, it was possible to cut the stricture directly upwards. The probability is, that the edge of the knife was directed upwards and inwards, and the incision made exactly at the part invariably occupied by the epigastric artery in such forms of hernia. Whilst this case cannot be considered as indicating the existence of the slightest risk in the ordinary plan of dividing the stricture directly upwards in every form of inguinal hernia, it offers a very serious warning against the division of the stricture of any hernia whatever, without

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[1] Observations on the Diseases of the Heart, &c. Edinburgh, 1809, p. 317.
[2] Wishart's Translation, p. 130.

as complete exposure of the strictured part as can be secured under the circumstances.

In every variety of inguinal hernia it may be safely asserted that there is not the slightest risk of hemorrhage from the division of the stricture directly upwards.

Femoral Hernia.—The crural ring and the neck of an ordinary femoral hernia, have the following relations to important vessels :—Immediately outside, with the femoral vein ; above and on the outer side, with the epigastric artery, at the distance of half-an-inch ; immediately above, or in front, with the round ligament in the female, and the spermatic cord in the male, the spermatic artery and the cremasteric branch of the epigastric being not more than half-an-inch distant from the neck of a recent hernia, and within two or three lines of one which has dilated the aperture.

It is probable, as appears from the researches of Mr Quain, that the inner side of the ring has no large vessel upon it in about 90 cases per cent., but in the rest this position may be occupied by the obturator or epigastric arteries,—by a considerable branch of communication between these two vessels, this branch, at times, becoming so large as to constitute a second origin of the obturator,—or by small branches of the epigastric ramifying behind the pubes.[1]

The obturator artery runs in front and on the inner side of the ring in 2·8 cases per cent. ; and in 10·46 instances per cent. it crosses the opening[2] and is displaced by the occurrence of a hernial protrusion, being tensely stretched over the inner side of the neck of the sac, or pushed outwards against the external iliac vein. It may even be found below or behind the neck of the sac (Cloquet).[3] When two femoral herniæ occur, the obturator artery, arising from the external iliac by a common trunk with the epigastric, may pass between them in its course into the pelvis, as in a case dissected by Allan Burns, in which he says, one hernia passed into the sheath of the lymphatics, and the other into that of the vein.[4] The course taken by the obturator varies very much even when it arises in the same position, so that its relation with the inner or outer side of the neck of a femoral hernia does not altogether depend on the length

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[1] See Mr Quain's Plate 68, Figs. 3 and 4, shewing the obturator artery passing over the anterior and inner side of the neck of the sac of a femoral hernia : Plate lxxii. Fig. 1, shewing the epigastric close to the iliac vein, in Hesselbach's case, and the wood-cut here given, for the epigastric crossing the crural ring : Mr Quain's Plate lxvi. Fig. 1, for a branch of artery crossing the ring, and communicating between the obturator and epigastric arteries ; and Plate lxvi. Fig. 3, for a similar branch much enlarged, and curving over the anterior and inner side of the ring.

[2] See Mr Quain's Commentaries on the Arteries, p. 451.

[3] Recherches Anatomiques sur les Hernies de l'Abdomen. 4to, Paris, 1817. P. 86, prop. 50.

[4] Observations on Diseases of the Heart, and on the Larger Arteries. By Allan Burns. Edinburgh, 1809. P. 313 ; and Dr Monro's Plate xvi., Fig. 2. op. cit.

of the common trunk by which it and the epigastric artery arise from the external iliac.

The epigastric artery, arising from the internal iliac by a common trunk with the obturator, may lie on either side of the neck of a crural hernia. In Hesselbach's case it was close to the iliac vein, and would have been on the outside of the neck of a hernial sac. In the case here recorded the artery crossed the aperture, and would probably have been displaced with equal readiness to either side; whilst in Dr Monro's case, no mention is made of the position of the artery. One of the two epigastric arteries seen by Lauth, arising from the external and internal iliac arteries respectively, would in all probability have been close to the neck of a femoral hernia, but nothing is recorded of their relative position further than that they were placed on opposite sides of the spermatic cord. Arnaud[1] has an engraving showing the epigastric artery arising from the inner side of the external iliac, and passing under the external iliac vein, along the outer side of the femoral ring, to gain the abdominal wall.

Relations of the Sac of a Femoral Hernia to large vessels.—The sac of an ordinary femoral hernia may have the epigastric, obturator, or internal circumflex arteries, in front of it, as appears from a case seen by Dr P. Hennis Green,[2] in which the epigastric was given off by the profunda femoris, and passed very far forwards in ascending;—from one or two subjects in which the epigastric arose from the femoral, and passed in front of the sheath of the lymphatics, to gain the abdomen;—from one or two cases where the epigastric arose from the superficial femoral;—from a very long epigastric artery being protruded into the front of the crural canal,—all which cases were observed by Allan Burns;[3]—from two instances in which Dr P. H. Green observed the obturator to arise from the femoral artery, and to advance so far forwards in ascending internal to the femoral vein, that, he says, a hernia must have passed behind it;—and from an instance recorded by Burns (p. 319), in which the internal circumflex artery arose from the external iliac, and passed in front of the sheath in descending to the thigh. Dr Monro saw a branch of the circumflex iliac artery, which, he says,[4] was nearly as large as the epigastric, and might have been divided in an operation for femoral or inguinal hernia. It passed under the crural arch, about two inches from the symphysis pubis, and then divided into branches, which were distributed upon the symphysis, over the crural arch, and on the iliac portion of the fascia lata.

The posterior surface of the sac may be in contact with the obturator artery, when this arises from the superficial femoral, two inches below Poupart's ligament, and ascends on the posterior part of the

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[1] Mémoires de Chirurgie. Paris, 1768. Tom. ii. p. 782.
[2] Cyclopædia of Practical Surgery. Article—Arteries—Anomalies of.
[3] Op. citat., p. 316.
[4] Op. citat., p. 431.

sheath, as in two cases recorded by Allan Burns (p. 315). The inferior and internal border of the neck of the sack of a femoral hernia, examined by Hesselbach, had the epigastric artery running along it, after arising from the profunda femoris. Mr Quain has an instance (Plate lxxiv. Fig. 3), in which the internal circumflex and epigastric arteries arise, by a common trunk, below Poupart's ligament, the internal circumflex curves over the femoral vein, by the side of the crural canal, and the epigastric passes through the ring in front and on the outer side. He has also an instance (Plate lxxiv. Fig. 4), in which the obturator, epigastric, and internal circumflex arteries, arise by a common trunk below Poupart's ligament, the internal circumflex, in this instance also, turns over the femoral vein, and both the obturator and epigastric arteries pass into the abdomen through the crural ring. Michelet[1] saw the epigastric artery arise from the internal circumflex in the thigh, and then ascend to its ordinary position; and Dr John Reid[2] found the epigastric arising from the internal circumflex in one case, and the internal circumflex from the epigastric in another; the epigastric, in the first instance, and the internal circumflex in the other, being in such positions that they would probably have lain on the outer side of a femoral hernia if one had descended.

There are, perhaps, no large arteries in the body so likely to present numerous and considerable irregularities in their origin and course as those which have been named in connection with the femoral ring and canal, and there is certainly no other space so limited, and, at the same time, so likely to present us with irregular arteries of such great surgical importance, both from their size and position. There is scarcely any spot around the neck, or on the body of a crural hernia, lying within the canal, which may not be occupied by a large artery; but the instances in which any large vessel, except the femoral vein, lies in relation with the boundaries of the canal, are exceedingly rare; and should an artery lie in such a position, and be wounded during the performance of an operation, it is satisfactory to know that it will be in a position in which it can be exposed and secured without any serious consequence. But it is very different with vessels which may be found *in front, or inside the neck*, of a similar hernia, for these could scarcely be secured if divided, and a fatal result might be expected from internal hemorrhage. Here it is to be remembered that the obturator artery arises from the epigastric, and passes in front of the neck of the sac of a femoral hernia, to gain its inner side in 2·8 cases per cent., and that it would in all probability occupy the same position in several of the 10·46 other cases per cent., in which it was found[3] passing across the aperture

[1] Nouveaux Eléments de Méd. Opératoire. Par M. Velpeau. Paris, 1832. Tom. ii. p. 452.

[2] Anatomical Observations in Edinburgh Medical and Surgical Journal, No. 128.

[3] Quain's Commentaries.

when no hernia had been formed. In either sex, therefore, the division of the femoral ring upwards would be attended with great danger of hemorrhage in the proportion of cases just named, the whole amount of danger to the patient being in proportion to the difficulty of securing the vessel when divided, and to the length of the incision, which will require to be greater when this is made upwards than when it is directed inwards. In a male, other dangers attend the division of the stricture upwards, for the spermatic artery is greatly endangered if the testis has descended, and though it may be secured if divided, the testis will thus be sacrificed; consequently, a stricture at the neck of a femoral hernia, in the male, never ought be divided directly upwards.

As, however, there can be no doubt that the tight stricture formed by the femoral ring, is most effectually relaxed by the division of the edge of Gimbernat's ligament, or the angle of connection between it and Poupart's ligament, where the fascia lata is attached to both, *i. e.*, by cutting inwards, or upwards and inwards, we are more especially concerned with the dangers which result from the adoption of such methods of operating in 2·8 cases per cent., in which the obturator artery passes in front of the crural ring, and descends near to the edge of Gimbernat's ligament;—in about 5 cases per cent., in which the same artery would be so displaced by the occurrence of a hernia, as to lie tightly stretched over the front and inner side of its neck;—in an instance like that recorded in this paper, where the epigastric artery might have been displaced by a hernia, and stretched over the inner side of its neck;—and in the few cases in which considerable communications exist between the epigastric and obturator arteries by vessels running near to the edge of Gimbernat's ligament. The most important inquiry bearing on this point is whether arteries lying in the positions which have just been named, must necessarily be divided with the tense edge of the ring in an operation. If the mode of connection of such vessels with Gimbernat's ligament be examined, it will be found to be maintained by loose cellular tissue, readily allowing of slight displacement of the vessels, so that in those instances in which a vessel has not been displaced by the descent of a hernia, and thus stretched tensely over its neck, there is no doubt that it may be avoided,[1] even though running across the direction of the line of incision. This is to be accom-

[1] An interesting case is recorded by Breschet, in which Gimbernat's ligament was divided to the extent of two lines. On reducing the hernia, and passing up the index finger, the pulsations of an artery were distinctly felt on either side of the ring. The patient died, and, on examination, it was found that the obturator artery lay on the inner side of the neck of the sac, and would have been divided had the incision been carried one or two lines further. The epigastric artery ran along the outer side of the sac, but was distant from it four or five lines, while the spermatic and supra-pubic arteries lay so much in front that they would have been wounded by carrying the incision directly upwards.

plished by passing no more than the blunt point of the bistoury beyond the edge of the stricture, and then dividing the tense fibres by pressing the edge of the knife rather upon the anterior surface than directly on the edge of the ligament, in carrying the handle inwards, or upwards and inwards. So few fibres require to be divided—and these, too, at a time when they are stretched to the utmost—that an artery which is neither tense nor firmly attached to the part to be divided, may frequently escape in a carefully-conducted operation; but an obturator, or epigastric artery, which, in the natural state, ran across the femoral ring, and, by the protrusion of a hernia, has been displaced inwards, will be tensely stretched across the direction of the incision inwards, or upwards and inwards, and will consequently be in great danger. If a vessel, in such a position, can be avoided, it will assuredly be by the same method as secures an artery not displaced by the hernia.

When a hernia passes under some other part of the crural arch than at the ring, its relations to the vessels differ very materially. Two herniæ passing out of the abdomen, close to the outer side of the femoral vessels, were dissected by Mr Stanley;[1] in both, the sac, about the size of a walnut, was placed directly in front of the femoral artery and vein. In one of these instances the epigastric artery arose from the femoral and gave origin to the obturator. The common trunk, and its division into the epigastric and obturator branches, were placed in front of the sac. A. K. Hesselbach[2] met with a hernia between the anterior-superior spinous process of the ilium and the great vessels. Its fundus lay beneath the iliac portion of the fascia lata, and the neck was crossed anteriorly by the circumflex iliac artery. Seven cases of femoral hernia, protruding outside the femoral vessels will be found noticed in the Register of the London Truss Society;[3] and the same occurrence is noticed by Petit,[4] Sabatier,[5] Callisen,[6] Arnaud,[7] Richter,[8] Richerand,[9] and Maclise.[10] A hernia in front of the femoral vessels, having the epigastric artery along the inner border of the sac, was observed by Cloquet,[11] who also saw one which passed through the posterior part of the femoral sheath, and lay on the pectineus muscle,

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[1] Laurence's Treatise on Ruptures. Fifth edition. P. 486.

[2] Lehre von den Eingeweidebrüchen. Würzburg, 1829.

[3] Laurence on Ruptures. Fifth edition. P. 11.

[4] Traité des Maladies Chirurgicales. Nouvelle édition. Paris, 1790. Tom. ii. p. 219.

[5] Médecine Opératoire. Tom. i. p. 143.

[6] Systema Chirurg. Hodiern. Pars, post., p. 495.

[7] Mémoires de Chirurgie. Tom. ii., p. 779 and 768.

[8] Traité des Hernies. Tom. ii., p. 138.

[9] Nosographie Chirurg. Cinquième édition. Paris, 1821. Tom. iii. p. 404.

[10] Surgical Anatomy. By J. Maclise. London, 1850. Note to Commentary on Plate xxiv.

[11] Recherches Anatomiques sur les Hernies. Paris, 1817. Page 85, props. 46 and 48.

having the pubic portion of the fascia lata, and the femoral vessels, in front. The occurrence of hernia in front of the vessels is likewise mentioned by Callisen, Richerand, Arnaud, and Richter.[1]

In the following cases vessels of importance have been injured in operations for strangulated femoral hernia :—

1. Hospital of La Charité at Berlin—Strangulated femoral hernia in a female—crural ligament divided by incision parallel to horizontal ramus of pubes—death eight days afterwards, from trismus and opisthotonos—the obturator artery was found cut—it arose from the epigastric, and passed in front and on the inner side of the neck of the sac—six ounces of putrid blood were found in the cavity of the pelvis.[2]

2. Femoral hernia passing through the ring and having the obturator artery on the inner side, the epigastric on the outer, with both vessels crossing immediately in front of the tumour—coat of the obturator grazed by the knife in the operation—hemorrhage supervened on the eighth day, and carried off the patient. The case was Mursinna's, and is mentioned by Robert.[3]

3. Strangulated femoral hernia in a male, æt. 22, operated on by Arnaud in 1736—Stricture divided upwards—no unusual hemorrhage externally—death in an hour after operation—spermatic artery found divided, and a large effusion of blood in the abdomen.[4]

4. Strangulated femoral hernia in a female—Stricture divided upwards and outwards—incision extended in the same direction—wound then immediately filled with arterial blood—the vessel could not be seen—complete cessation of hemorrhage from fainting after the loss of a pint of blood—complete recovery. Case recorded by Mr Laurence.[5]

5. Strangulated femoral hernia in a female—Profuse arterial hemorrhage after division of the stricture—source of hemorrhage could not be discovered—the patient became faint—a compress and bandage were applied, and the bleeding ceased. Operation performed and case recorded by Mr Lawrence.[6]

6. Strangulated femoral hernia in a female—Operation performed by Professor Benedict, of Breslau—a winged probe was introduced into the neck of the sac, and stricture cut from without inwards and from below upwards—epigastric artery, running along the neck of the sac was divided—free hemorrhage—the mouth of the vessel was immediately seized by an assistant, and easily tied.[7]

7. Strangulated femoral hernia in an aged female—Mr Hey's

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[1] Opera Citata.

[2] De Extensionis in Solvendis Herniis Cruralibus incarceratis præ Incisione præstantia. By F. L. Trüstedt. 4to, Berol. 1816. Quoted in Cooper's Surgical Dictionary, p. 741.

[3] Journal des Progrés des Sciences Médicales. Tom. vii., p. 193.

[4] Mémoires de Chirurgie. Paris, 1768. Tom. ii. p. 755.

[5] Op. cit., p. 272.

[6] Op. cit., p. 512.

[7] Rust. Magazin für die gesammte Heilkunde, vol. xliv., p. 178.

Case 9[1]—Stricture cut directly upwards for half an inch, and not on the inner side of the intestine—copious arterial hemorrhage—no orifice to be seen—hemorrhage suppressed by dry sponge, a bandage, and pressure exerted by an assistant, during the first day after operation—sponge removed on the 14th day—wound cicatrised at the end of five weeks.

8 and 9. Mr Guthrie states,[2] that he has been made aware of more than one instance of the obturator artery, surrounding the neck of the sac, having been wounded by some of the best anatomists and surgeons in London, in the division of Gimbernat's ligament horizontally, and that the patients subsequently bled at intervals, until they died from hemorrhage.

10. Strangulated femoral hernia in a female, æt. 60—Operation performed by Dupuytren, at the Hôtel-Dieu, in 1831, and witnessed by Dr John Reid—hemorrhage during the operation greater than usual, but it soon ceased—patient went on well for a fortnight, when she was seized with severe diarrhœa, which soon carried her off—a broad sheet of blood was found between the peritoneum and the fascia transversalis, extending from the lower part of the pelvis up nearly to the umbilicus. The obturator artery, which arose from the epigastric, nearly surrounded the neck of the sac, and had been completely divided in the operation. Recorded by Dr J. Reid.[3]

11 and 12. Guntz[4] heard of two instances in Paris in which the epigastric artery was cut in dividing the stricture upwards, and the patients died from hemorrhage.

13. Sabatier[5] states, that a fatal hemorrhage has been seen to follow the division of Gimbernat's ligament, in the operation for strangulated femoral hernia, owing to the section of small branches of the epigastric artery.

14. Hospital of La Charité, Paris—Strangulated femoral hernia—stricture divided inwards—considerable hemorrhage, arrested by a graduated compress, the farthest extremity of which projected into the iliac fossa—compress removed in five days, without farther hemorrhage. Recorded by M. Velpeau.[6]

15 and 16. Two cases of strangulated femoral hernia mistaken for inguinal—stricture divided outwards, and femoral vein wounded in both cases—profuse hemorrhage, restrained with great difficulty. Mentioned by Sir A. Cooper.[7]

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[1] Practical Observations in Surgery. By Wm. Hey, 2d edition, 1810, p. 160.

[2] Guthrie on Some Points connected with the Anatomy and Surgery of Inguinal and Crural Hernia, 4to, p. 36.

[3] Anatomical Observations, in Ed. Med. and Surg. Journal, No. 128.

[4] Obs. Anatomico-Chirurg. de Herniis Libellus. Lips. 1744.

[5] De la Médecine Opératoire. Par R. B. Sabatier. Nouvelle édition, par Sanson et Begin. Paris, 1832. Tom. iii., p. 608.

[6] Médecine Opératoire. Tom. ii., p. 489.

[7] On Crural Hernia. Op. cit., 2d edition, p. 4.

17, 18, and 19. Dr P. Hennis Green says,[1]—"The late Dr A. Thomson has assured us, that he had positive proofs that Dupuytren wounded the epigastric artery on three several occasions." (Yet Dupuytren endeavoured to avoid this by cutting that portion of the fascia lata which is attached to the crural arch, rendering it tense.)

20 and 21. M. Robert[2] examined the bodies of two persons who died from hemorrhage under circumstances similar to those in the last three cases.

22. A case occurring to M. La Chaud—Femoral hernia in a female—epigastric or circumflex iliac artery divided in an operation performed in the presence of Arnaud, by whom the case is named.[3] (The probability is, that the stricture was cut upwards, and that death was the result, though no positive statement is made on these heads.)

23. Schindler[4] witnessed the death of a female from division of the epigastric artery, in an operation for femoral hernia.[5]

Notwithstanding that the records of many of the above cases are very imperfect, they are of considerable value in connection with the anatomy of the vessels before named; for, whilst it is proved, on the one hand, that large vessels have occasionally such relations to the neck of a crural hernia as seem to place them in the greatest danger from the incisions ordinarily made for the relief of the stricture of such a hernia, there can, on the other hand, be no doubt that such vessels have, on many occasions, been injured, and that fatal hemorrhage has resulted from operations performed with great care by very eminent surgeons. In these respects there is a remarkable difference between inguinal and femoral hernia; in the former, no vessel has been shown to have been injured in any single instance by the division of the stricture upwards when the part had been fairly exposed, nor has any vessel been seen in a position in which it could have been injured by such a proceeding; in the latter, the division of the stricture inwards, upwards, or outwards,

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[1] Cyclopædia, ante cit.

[2] Nouvelle Méthode pour le Débridement de la Hernie Crurale. Par Verpillat. Paris, 1834. In 8vo.

[3] Op. cit., pp. 756 and 770.

[4] H. T. Schindler. Diss. de Herniis Observationes et Meletemata quædam. Wittenberg, 1796.

[5] It is possible that all the cases referred to in Nos. 10, 17, 18, 19, 20, and 21, did not occur as individual instances, but that the same case, or cases, were observed and mentioned by several persons, though there is nothing to enable us to identify them. MM. Roche and Sanson state, that Dupuytren never wounded the epigastric artery; but we have the positive evidence of Dr A. Thomson, that that eminent surgeon wounded the epigastric on three occasions, whilst Dr Reid saw him divide the obturator directly across.

has alike proved fatal from hemorrhage, and we have the best reasons for believing that such accidents will continue to recur, in a small proportion of cases, unless the possibility of arteries lying across the recognised lines of incision of the stricture be borne in mind, and the greatest care be exercised in all such operations. The necessity of accurate diagnosis is so apparent as to need little comment, for to mistake an inguinal for a femoral hernia, or the reverse, and to operate upon it with such a mistaken impression, must necessarily lead to death from hemorrhage in a considerable number of instances.

Lastly, in the instances in which a femoral hernia does not pass through the canal, but appears under some other part of the crural arch, and becomes strangulated, nothing but an accurate diagnosis of the position of the hernia, and a precise anatomical knowledge of the position and connections of the parts with which it must come into relation, can warrant a departure from the ordinary method of operating, and save the patient from the risk of fatal hemorrhage to which he would be subjected by the performance of the ordinary operation for femoral or inguinal hernia.

The conclusions which may be drawn from the foregoing considerations are the following:—

1st, In every possible variety of inguinal rupture, the stricture ought to be divided directly upwards, as by so doing there is not the slightest risk of hemorrhage.

2d, The probability of the occurrence of hemorrhage is much greater in operations for femoral than for inguinal hernia, as the epigastric, obturator, or internal circumflex artery may be divided in exposing the sac; and the obturator, epigastric, or a large anastomosing branch, may be injured in the division of the stricture.

3d, The varieties in the arteries named are sufficiently frequent to render them of great surgical importance, and that variety is most important in which the vessel is displaced inwards, by the descent of a femoral hernia, and becomes tensely stretched over the front and inner side of the neck of the sac.

4th, The risk of hemorrhage is not lessened by dividing the stricture of a femoral hernia in any other direction than inwards, or upwards and inwards, but it is very much diminished by dividing as few of the tense fibres of the edge of the ring as possible,—by avoiding a sawing motion,—and by pressing the edge of the knife rather upon the anterior surface than directly on the edge of the fibres constituting the stricture.

5th, In a male, whose testis has passed into the scrotum, the spermatic artery may be wounded, and the testicle lost, by dividing the stricture at the neck of the sac of a femoral hernia upwards, and, therefore, this method of operating ought to be relinquished.

ARTICLE IV.—*On the Excision of Enlarged Tonsils.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

IN cold moist climates the tonsils are very subject to a simple enlargement, which gives rise to much inconvenience of different kinds. In the first place it seems to constitute a weak point, always apt to suffer from derangement of the system, productive of inflammatory disturbance, and thus occasioning frequent sore throat. It also renders the voice husky and disagreeable, impairs hearing by obstructing the orifice of the Eustachian tube, and impedes respiration, especially during sleep,— when the stertorous noise caused by breathing through the mouth, and the fits of threatened suffocation which are apt to awaken the patient, occasion no less distress to him than alarm and annoyance to his neighbours.

Childhood is the age most exposed to this morbid condition; which, however, not unfrequently makes its first appearance during adolescence, and is sometimes met with at a considerably more advanced time of life. After maturity has been attained, however, there is not only less disposition to the morbid growth, but a tendency to its spontaneous disappearance;—whence active measures for removal of the swelling have been deemed improper, and would be so if the effects of the enlargement were of trivial importance and short duration, or if the means of remedy were either severe or dangerous. It may be added that the relief through spontaneous change is neither certain nor complete, and that if the enlargement be permitted to continue during the season of youth, it may increase the risk of pulmonary disease by embarrassing the action of the respiratory organs.

The only source of speedy and effectual relief is local treatment, for which the means that have at different times been in use are escharotics, ligature, and excision. Of these, the first has generally been preferred, from dread of encountering the difficulty which attends the second, and the danger apprehended from the third. The actual cautery has been employed to a small extent for the purpose, but the nitrate of silver is the destructive agent that has obtained by far the largest share of confidence, together with the sulphate of copper, burnt alum, and similar applications of less potent energy. But as the effect of all these means is necessarily superficial, no material improvement can be obtained through their use, however long and carefully continued. Indeed, there is no light reason to believe that so far from diminishing the size of the swelling they really tend to increase it by exciting a degree of activity in the nutrient action of the texture more than sufficient to compensate for the loss of substance on its surface.

With regard to the ligature, in 1750, in his "Critical Inquiry

into the state of Surgery," Mr Samuel Sharp, of Guy's Hospital, thus wrote:—"The extirpation of scirrhous tonsils by ligature, seems to be a practice as yet almost entirely confined to England, though for no other reason, as I imagine, but because it generally requires some time for the propagation of an improvement. It is acknowledged on all hands that the application of escharotics is a tedious, painful, and sometimes an ineffectual method of cure. It is likewise granted that the hemorrhage which follows upon the excision of scirrhous tonsils is greatly to be feared; but still, the tying them is neglected."[1] But notwithstanding so confident an expression of opinion in its favour, the ligature has never been generally adopted for the purpose in question. Indeed, the all but insuperable difficulty attending its application in a situation so inaccessible as the fauces, with all the assistance that the patient can afford, instead of his involuntary resistance, or determined efforts of opposition, as must be the case when children are subjected to the operation, has usually rendered one trial sufficient to prevent any desire for its repetition. In 1812, Mr Chevalier, of London, published[2] an "Improved Method of tying diseased Tonsils," which does not appear to have rendered this mode of treatment more popular; indeed, his account of the process, after the ligature has been applied, is not very encouraging. "A good deal of inflammation usually arises in the mouth and fauces, and about the sublingual and submaxillary glands soon after the operation, which is best soothed by small doses of laudanum and antimonial wine. The mouth may be frequently washed with warm water, and the poppy fomentation employed externally, if the pain should be considerable. This inflammation begins to subside on the third day. I think the tumour is generally detached on the fourth or fifth day, and the sore readily heals."[3]

The method of excision dates so far back as the time of Celsus, who advises, when other means have failed to remove enlargement of the tonsil, "*hamulo excipere, et scalpello excidere.*" It is occasionally mentioned by succeeding writers, either for commendation or rejection; but does not appear to have been systematically or extensively employed until the middle of last century, when various French surgeons devoted much attention to its practice and improvement. Of these, M. Caqué, surgeon of the Hôtel-Dieu of Reims, and his colleague, M. Muzeux, seem, from the excellent papers of M. Louis, in the Memoirs of the Academy of Surgery,[4] to have been most successful in their exertions. The former of these gentlemen operated in many cases with great success; and the latter, in order to facilitate the process of removal, contrived a peculiar sort

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[1] Sharp's Critical Inquiry, p. 244.
[2] Medico-Chirurgical Transactions, vol. ii. p. 30.
[3] Loc. cit.
[4] Tom. v., p. 423.

of forceps, which still retains his name. This instrument is about seven inches in length, and, from the handle of the blades to their junction, resembles that employed for the evulsion of nasal polypus, The extremities of the blades are slightly curved, and each terminates in a double hook, so fashioned as to take a sure hold of any soft substance such as the tonsil. Though the operation was thus rendered wonderfully easy and safe, it still retained a character of difficulty and danger that opposed its general introduction into practice, and in this country precluded its employment entirely until comparatively a very recent period.

In the year 1821, during my residence in the Royal Infirmary of Edinburgh, as House Surgeon, one of the female patients suffered from enlargement of a tonsil. After trying sulphate of copper, and similar applications, without success, I attempted the ligature, but without being able to accomplish it, and then as the tumour was of no great size resolved to remove it by excision, which was accordingly done very readily by means of a hook and curved scissors. This, so far as I know, was the first instance of a tonsil being cut out in Edinburgh. Next year I went to Paris, and there found M. Lisfranc in his course of operative surgery on the dead body, treating excision of the tonsil as an established operation. He employed for the purpose a straight narrow blunt-pointed bistoury, of which the blade was sheathed, except for rather more than an inch from the point, and explained that if the tumour was pulled inwards from the side of the fauces or merely held steady in its natural position, the knife might be carried with perfect safety through its base, as the blade must then be parallel with the carotid artery, and at such a distance from it as to render any injury of the vessel quite impossible.

Much impressed with the advantages of the operation as thus performed, I endeavoured, on my return home, to promote its adoption, and having carried with me the forceps of Muzeux, placed them in the hands of Mr Liston, who also ever afterwards did all in his power, by precept as well as example, to establish the practice. The counties of Perth, Stirling, Peebles, and Roxburgh, are very prolific of enlarged tonsils, and the success attending their removal by excision soon brought patients from all parts of the country, on both sides of the Tweed. Abundant opportunity has consequently been afforded for determining the merits of the operation on the sure ground of extensive and long continued experience.

In no instance has hemorrhage or any other unpleasant accident occurred. The texture concerned possesses so little sensibility that the excision is accomplished with hardly any pain; and children, who are sometimes induced with difficulty to submit, in the first instance, seldom offer any objection to removal of the second tonsil. I never employ a speculum of any sort,—even in the youngest and most obstreporous patients,—the forceps sufficiently keeping down the

tongue, and the wide opening of the mouth preparatory to the emission of a cry, affording ample space for the knife to act. In the whole course of my practice I have only on two occasions failed in accomplishing the object in view, through the excessive size of the tongue and contraction of the mouth. The knife should possess a very keen edge, as the glandular substance is sometimes indurated, and a partial division of it would lead to great embarrassment, from the blood trickling into the pharynx, and causing such cough or convulsive efforts as must render a fresh seizure of the tonsil next to impossible. I generally remove the left tumour first, and then the right one, by crossing my hands so as still to retain the knife in the right. The process is instantaneous, and the relief immediate. The bleeding seldom exceeds a teaspoonful or two, and no after treatment is required.

ARTICLE V.—*Case of Impracticable Labour, in which the Cæsarean Operation was performed.* By MATTHEW NIMMO, Esq., Surgeon to the Dundee Infirmary, &c.

(*Read before the Edinburgh Medico-Chirurgical Society, 5th June* 1850.)

I WAS requested about one o'clock A.M., on 20th May 1850, by Mr Kyle, surgeon, to meet Dr Munro and Mr Drummond, surgeon, at the house of Mrs Kennaway, who had been in regular labour from half-past nine the preceding evening. We were informed that the liquor amnii had been discharged on the evening of the 15th May, and that since then she had experienced occasional pains. An Irish midwife had been in attendance, but till the night of 19th May they did not consider the woman so ill as to require farther assistance. Mr Kyle was requested to visit her, and, on examination per vaginam, was soon satisfied that deformity of the pelvis existed to such an extent as to render it impracticable to deliver per vias naturales, and therefore the attendance of Dr Munro, Mr Drummond, and myself was requested.

On inquiring into the previous history of the case of this poor woman, we learned the following particulars partly from the patient herself, partly from her husband, and those in attendance. Mrs Kennaway was forty-three years of age, small in stature, deformed, emaciated, and of unhealthy appearance. She had born three children; the first when twenty-four years of age, and had an easy labour of four hours' duration. She again became pregnant when twenty-seven years of age; had an easy labour, lasting only two hours. A third time she became pregnant when twenty-nine years of age. During this pregnancy she suffered from severe pains in the pelvic and lumbar regions, which she ascribed to rheumatism,

and became slightly lame. Her third labour was severe and protracted, lasting three days, and the forceps were required. The child only survived a few minutes, and the mother made a good recovery. Since that time, which was in 1835, she had gradually become more and more decrepit, and continued to suffer much from pelvic pains and constipation.

On examination per vaginam, a hard rounded body, somewhat resembling the fœtal head, was felt in the position of the upper part of the sacrum. Anteriorly the rami of the pubis were so approximated that the finger could scarcely be introduced between the protuberance and symphysis pubis. At each side of the projection, between it and the rami pubis, a fissure scarcely capable of admitting the finger existed. Neither the os uteri nor presenting part of the child could be reached by the finger. The fœtal head could be distinctly felt resting on the brim of the pelvis, on laying the hand on the hypo-gastric region. The fœtal heart was heard distinctly to beat on application of the stethoscope to the left iliac region.

The patient was removed at two A.M. to the Dundee Royal Infirmary, and a consultation of the medical officers was called. It was agreed that there was no possibility of delivery, but by having recourse to the Cæsarean section. About this time the labour pains were becoming more frequent and urgent, and continued so during the morning.

At two o'clock P.M., I proceeded to perform the operation, in presence of many of the medical gentlemen of the town. The ward into which the patient was removed, had been heated to a temperature of between 70° and 80° Fahr. The rectum had been emptied in the morning by enema, and a catheter was introduced to evacuate the bladder. The patient had the chloroform administered to her, and was soon deeply under its influence. An incision was made in the linea alba, from the umbilicus to within an inch and a-half of the symphysis pubis, through the integuments. I then made an opening through the aponeurosis and peritoneum, at the upper extremity of the incision, and by means of a probe-pointed bistoury, guarded by my finger, slit open the peritoneum to the same extent. In the same manner the uterus was divided and in the same direction. The placenta was attached to the fundus, and partly in the line of the incision, and a portion of it was cut into, when considerable hemorrhage followed. The placenta was quickly detached by Dr Munro, and the child speedily removed from the uterus. I then passed my finger through the os uteri, to allow the escape of any discharge from the womb. I grasped the uterus in my hand to promote as full contraction as possible. Immediately, the uterus was emptied of its contents, the patient became sick and vomited, and a large portion of intestine was protruded, which was soon replaced, and the wound brought together by a few stitches of

the interrupted suture, with adhesive straps in the spaces between the sutures. A compress and broad bandage were then applied around the abdomen. The poor woman continued to vomit at intervals; the pulse became very rapid and feeble, and at times imperceptible; she continued to sink notwithstanding the administration of stimuli of various kinds, and expired about five P.M.

The fatal result of the operation to the mother was to be expected; at least there appeared very little chance of the poor woman surviving, when we consider her sufferings for the last fifteen years from disease of the osseous system. Her emaciated and cachectic appearance, indicated the ravages the disease had made on her health, and showed with how small prospect of success she could be subjected to an operation of such magnitude and danger.

The infant, a female, was taken charge of by some of my medical friends, while my attention was directed to the patient; and in half an hour, by their well-directed exertions, we had the pleasure of hearing the child cry lustily. The child is alive up to this date, and looks thriving and healthy.

The cavity of the abdomen was examined some hours after death. The uterus was found very well contracted. A small coagulum of blood was found adhering to the wound in the uterus, and there was none effused into the abdomen or pelvis. The pelvis was removed, and is now in the possession of Professor Simpson. The accompanying plate shows, Fig. 1, the form of the brim; and, Fig. 2, a transverse section of the pelvis. The following are some of its principal measurements :—

a to *c*.—From upper edge of fifth lumbar vertebra to upper border of symphysis pubis, $1\frac{5}{8}$ inches.
e to *c*.—From promontory of sacrum to upper border of symphysis pubis, $2\frac{7}{8}$ inches.
e to *d*.—From promontory of sacrum to lower border of symphysis pubis, $2\frac{6}{8}$ inches.
a to *f*.—From upper edge of fifth lumbar vertebra to linea ileo-pectinea of each side, $\frac{3}{8}$th of an inch.
b to *b*.—Transverse diameter between furthest points of brim, $4\frac{1}{8}$ inches.
k to *e*.—From point of coccyx to promontory of sacrum, $1\frac{7}{8}$ inch.
k to *d*.—From point of coccyx to symphysis pubis, 3 inches.
g to *g*.—Between furthest points of the crests of the ilia, $8\frac{1}{2}$ inches.

The Cæsarean operation has been performed upon the living mother in Great Britain and Ireland in about 47 cases. Among these only 7 of the mothers have been saved, and 40 have died. Of the children 26 have been saved, and 21 lost. The following table shows the number of cases operated on in this country, with the result to the mothers and children, so far as I have been able to collect them from different periodicals and works, up to this time.

PELVIS in Mr NIMMOS' CASE of CÆSAREAN SECTION.

Fig. 1

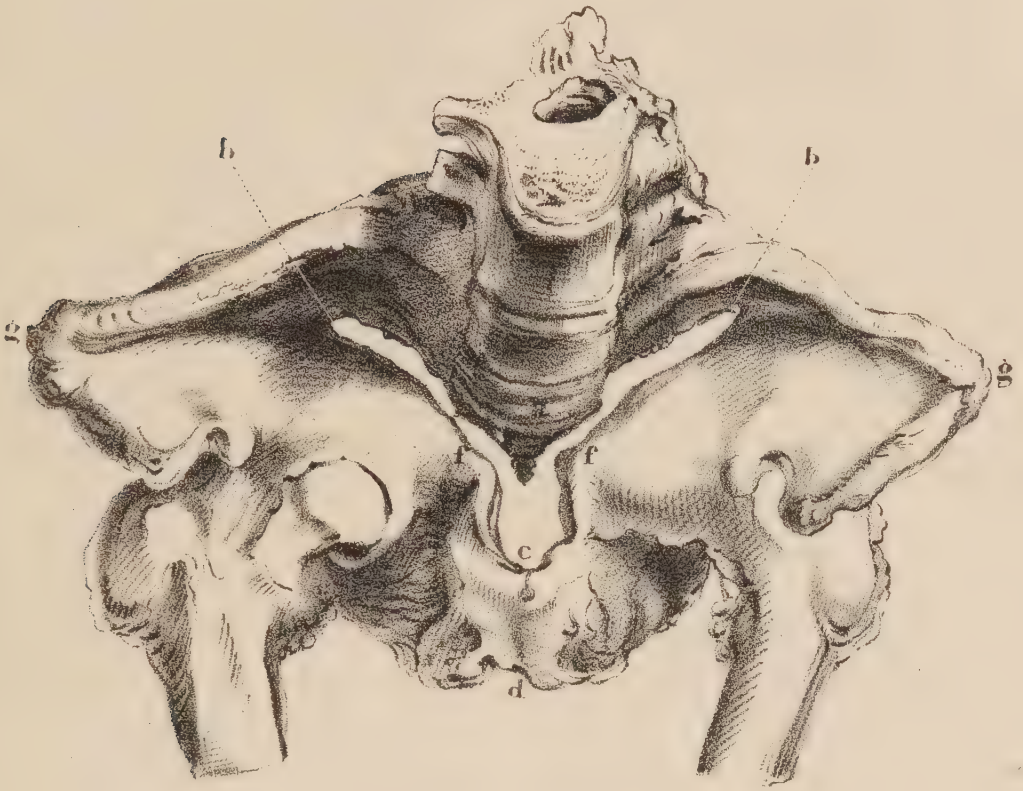

Fig. 2

N. Stewart. delt

W. L. Tod lith.

No.	Date.	Operator, or Authority.	Patients' Names.	Hours in Labour.	Cause.	Result to Mother.	Result to Child.	References.
1	1739	Mary Dunelly, midwife.	Alice O'Neal.	12 days.	... ...	recovered.	dead.	Ed. Med. Essays, vol. v., p. 439.
2	1737	Mr R. Smith, Edinburgh.	—— Paterson.	7 days.	... ..	died.	alive.	Smellie's Mid., vol. iii , p. 423.
3	...	Professor Young, do.	... ...	...	... ...	died.	alive.	MSS. Lectures.
4	...	Ditto.	... ...	...	... ...	died.	alive.	Do.
5	1740	Dr White, Manchester.	... ...	...	... ...	died.	dead.	Hull's First Letter.
6	...	Dr Wood, Edinburgh.	... ...	...	... ...	died.	dead.	Do.
7	1769	Mr Thomson, London.	Martha Rhodes.	24 hours.	... ...	died.	alive.	Med. Obs. and Enq., vol. iv., p. 261.
8	1774	Dr Cooper, London.	Eliz. Foster.	2 days.	moll. ossium.	died.	alive.	Do. vol. v., p. 218.
9	1774	Mr Chalmers, Edinburgh.	Eliz. Clark.	12 days.	... ...	died.	alive.	Hamilton's Outlines, p. 339.
10	1775	Mr White, Glasgow.	... ...	...	... ...	died.	dead.	Hull's Letter.
11	1777	Mr Atkinson, Leicester.	El. Hutchison.	8 days.	moll. ossium.	died.	alive.	Hull, p. 67.
12	...	Mr Clark, Wellingburgh.	... ...	3 days.	... ...	died.	dead.	Mem. Med. Soc., vol. v.
13	1793	Mr Barlow.	Jane Foster.	5 days.	distortion.	recovered.	dead.	Med. Rec. and Res., p. 154.
14	1794	Dr Hull, Manchester.	Isabel Redman.	12 hours.	moll. ossium.	died.	saved.	First Letter, p. 162.
15	1795	Dr Hamilton, jun., Edinburgh.	Jean Douglas.	2 days.	malacosteon.	died.	alive.	Outlines.
16	1798	Dr Hull, Manchester.	Ann Lee.	10 days.	... ...	died.	dead.	First Letter, p. 172.
17	1798	Mr Kay, Forfar.	... ...	3 days.	malacosteon.	died.	alive.	Hull's Letter.
18	1799	Mr Wood, Manchester.	El. Thomson.	...	distortion.	died.	saved.	Mem. Med. Soc., vol. v.
19	1800	Mr John Bell, Edinburgh.	... ...	...	... ...	died.	saved.	Med. Chir. Trans., vol. iv., p. 347.
20	...	Mr Dunlop, Rochdale.	Susan Holt.	...	... ...	died.	alive.	Hull's Baudeloc., p. 134.
21	...	Mr Wood, Manchester.	H. Rheubotham.	...	... ...	died.	dead.	Med. and Phys. Jour., vol. vi., p. 346.
22	...	Dr Kellie, Leith.	... ...	24 hours.	... ...	died.	dead.	Ed. Med. and Surg. Jour., vol. viii., p. 11.
23	...	Mr K. Wood, Manchester.	... ...	...	... ...	died.	dead.	Med. Chir. Trans., vol. vii., p. 264.
24	1817	Barlow and Cort, Blackburn.	Ann Hacking.	...	... ...	died.	alive.	Barlow's Essays.
25	1821	Barlow and Dugdale.	Mrs Ridgedale.	...	... ...	died.	alive.	Merriman, p. 317.
26	...	Dr Henderson, Perth.	Mrs Lowe.	18 hours.	distortion.	died.	alive.	Do.
27	1820	Dr Radford, Manchester.	Mary Ashwell.	34 hours.	distortion.	died.	dead.	Ed. Jour., No. 146.
28	1821	Ditto.	Mary Nixon.	19 hours.	distortion.	died.	dead.	Do.
29	...	Ditto.	... ...	53 hours.	... ...	died.	dead.	Ranking, vol. x., p. 212.
30	...	Ditto.	... ...	...	... ...	recovered.	saved.	Do.
31	1849	Ditto.	Mrs Haigh.	3 days.	... ...	recovered.	saved.	Prov. Med. and Surg. Jour. for 1849.
32	1826	Mr Crichton, Dundee.	... ...	6 days.	... ...	died.	saved.	Ed. Med. and Surg. Jour. for 1828.
33	1829	Dr M'Keblin, Belfast.	... ...	...	exostosis.	died.	dead.	Do. for 1831, p. 352
34	1827	Mr Knowles, Manchester.	... ...	...	... ...	recovered.	dead.	Trans. of Prov. Assoc., vol iv.
35	1829	Mr Ward.	... ...	...	... ...	died	dead.	Lancet, March 28, 1840.
36	1833	Mr Greaves, Rockingham.	Mary Bamford.	27 hours.	distortion.	recovered.	saved.	Do. for 1833-34, p. 148.
37	1834	Dr Montgomery, Dublin.	... ...	...	fibrous tumour.	died.	dead.	Dublin Jour., vol. vi. p. 418.
38	1843	Dr Elliot, Waterford.	... ...	...	distortion.	died.	dead.	Letter to Dr Churchill.
39	...	Dr Whitehead, Manchester.	... ...	...	... ...	died.	alive.	Do do.
40	...	Mr Braid, Manchester.	... ...	...	... ...	died.	dead.	Ranking, vol. vii., p. 330.
41	...	Messrs Bailey and Hardy.	... ...	...	... ...	died.	dead.	Do. do.
42	1845	Mr Goodman, Manchester.	Mrs Sankey.	5 days.	distortion.	recovered.	saved.	British Obstetric Record, vol. i.
43	...	Mr Lyon, Glasgow.	... ...	...	tumour.	died.	saved.	Ed. Mon. Jour., Dec. 1845.
44	1841	Mr Ross, Invergordon.	... ...	...	... ...	died.	alive.	Do. for 1842.
45	1847	Mr Skey, London.	... ...	...	distortion.	died.	dead.	Lancet, for 1847.
46	1849	Mr Campbell, Lisburn.	... ...	...	distortion.	died.	saved.	Ranking, vol. x., p. 330.
47	1850	Mr Nimmo, Dundee.	Mrs Kennaway.	17 hours.	distortion.	died.	saved.	Ed. Mon. Jour., Sept. 1850.

ARTICLE VI.—*On the Pathological States of the Lung connected with Bronchitis and Bronchial Obstruction.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.

PART I.—PRIMARY RESULTS OF BRONCHITIS.—(*Continued from p.* 138.)

IN the August Number of the "Monthly Journal," I endeavoured to show that, among the pathological phenomena connected with obstruction of the bronchi by tough mucus, or other inflammatory products, one of the most important and frequent is collapse, to a greater or less extent, of the air-vesicles to which the obstructed bronchi lead. I adduced evidence that the condensation of the pulmonary tissue thus produced, which in its slighter degrees is often overlooked by anatomists, had in its more marked forms been described by many careful and exact observers under different names, and with various ideas of its pathological significance,—and that, especially in the case of children, it had been accurately distinguished from ordinary pneumonic condensation. I described the forms in which it had occurred under my own observation, and maintained that, in all essential characters, these lesions are the same in children and adults; that in both a certain degree of pulmonary collapse may be almost invariably found as a concomitant of fatal bronchitis; and that, in some cases, this state of the lung bears so obvious and undeniable a relation to obstruction of the tubes, as to lead to the almost unavoidable inference of the dependence of the former upon the latter.

That this conclusion from anatomical data is, in all respects, consistent, both with clinical experience and with correct views of the mechanism of respiration, I shall presently endeavour to prove; and, in the second part of this memoir, I propose to show the probable dependence of many important chronic alterations of the pulmonary texture on the condition of collapse from bronchial obstruction. The pathological history of this lesion, however, in relation to acute bronchitis, would not be complete without some notice of a condition which occurs so frequently in connexion with it, that I cannot consider it otherwise than as a secondary consequence of bronchitis, accompanied by pulmonary collapse.

Bronchial Abscess.

It not unfrequently happens that, in the centre of the collapsed lobules of a lung affected with acute bronchitis, there are found small collections of pus, varying in size from that of a hemp-seed upwards to double or treble that volume. These small abscesses present, on section, an appearance so like that of softening tubercles, as to be very readily mistaken by many persons for these bodies; and the resem-

blance is all the greater on account of the peculiar limited form of the condensation by which they are generally surrounded, which, when felt by the touch from the exterior of the lung, is exceedingly deceptive. In their interior, however, these little abscesses contain, in the recent state, a very fluid pus; moreover, they are often met with as acute lesions produced by a few days of illness, and without a trace of tubercle in any other organ. This is peculiarly characteristic in the young child, in which tubercle of the lung so very rarely occurs without extensive deposits in the bronchial glands, whereas the present lesion is accompanied in its pure form by nothing more than slight enlargement. When the pus contained in these abscesses, in their recent form, is pressed or scraped out, they are seen to be lined with a fine villous false membrane, very different from the thick curdy mass which generally surrounds softened tubercles; in others they are not abruptly limited at all, the pus appearing to lie in contact with the surrounding pulmonary tissue. When the bronchi leading to the lung so affected are carefully incised, they are found much inflamed; the mucous membrane vascular, thickened, and covered with pus; and some of the bronchi thus affected can be observed to communicate with these purulent collections, the mucous membrane having evidently been, at the point of communication, destroyed by ulceration, and either stopping short abruptly, or becoming gradually incorporated with the false membrane lining the abscess. Sometimes these abscesses are found to break into one another, and form more considerable excavations; in one instance (Case VII.) I found them connected with a gangrenous condition; more commonly, however, they remain of limited size, preserving perfectly the direction and relations of the bronchial tubes.

These abscesses occur in the diffused as well as in the lobular form of condensation from collapse; and both forms may sometimes be seen in the same lung. A similar lesion may accompany true pneumonia, but always in those cases where it is combined with intense bronchial inflammation. In this case, also, it tends to the formation of more considerable and more irregular excavations.

Such cavities have been pretty accurately described by Barthez and Rilliet (as well as by other French writers on the bronchitis and pneumonia of children), under the name of "*vacuoles*." They are, however, far from uncommon in adults, though, perhaps, more frequent in the so-called "lobular pneumonia" of children. As these "vacuoles" unquestionably arise from the accumulation of pus primarily in the extreme bronchial tubes of the collapsed lobules, the name of bronchial abscesses may serve to distinguish them from other forms of excavation of the lung.

That Laennec should have overlooked these small excavations is singular enough. Probably he may have considered them a form of tubercle, a term which has been used by him and others with sufficient vagueness to cover a multitude of anomalous lesions. His

assertion that in many hundreds of pneumonic lungs, he had only met with collections of pus five or six times, can only be explained upon this principle. It is, indeed, quite true that in simple pneumonia there is little tendency to the formation of abscess, the pus escaping rapidly by the pervious bronchi; but as we have seen that Laennec has probably confounded the pneumonic with the bronchitic condensation, I think it also probable that he has not distinguished the bronchial from the tubercular abscess, especially as these lesions often resemble each other so much as to have led me habitually, for some time, to call the former *tuberculoid disease* of the lung, which name is inscribed over several dissections in the pathological register from which the following cases are taken.

In three of the four cases of fatal hooping-cough recorded by Dr Alderson, in the paper formerly alluded to, these lesions are described shortly as dilatations of the smaller bronchi by thick mucus or mucopurulent secretion. Most of the French writers on the pneumonia and bronchitis of children since Barthez and Rilliet, refer to them more or less distinctly; and in Dr West's lectures they are described as a *true* lobular pneumonia—the result, however, of bronchitis. As the work of Dr West is probably accessible to most readers, I do not think it necessary to transcribe the passage, which contains, however, a very accurate description of the lesion in question—(see p. 174, op. cit.) In regard to the characters of these abscesses, as distinguished from tubercle, my observations, as detailed above, almost exactly concur with those of Dr West. In one point alone I am disposed to differ from him. He seems to regard the abscesses as not formed in truly *carnified* lung, because the condensed portions are not always *exactly* circumscribed by the interlobular tissue. I believe this appearance will be fully accounted for by the fact that when bronchial abscesses occur, the bronchitis is usually general and intense, and the collapse correspondingly diffused. Besides, I have found bronchial abscesses, and their results, in exactly circumscribed lobules, in repeated instances; and that the tissue in which they occur is mostly collapsed there can be no doubt, from the whole of its ordinary and microscopic characters. (See Case V.)

With these remarks I shall lay before the reader a few examples of this interesting lesion, which undoubtedly forms one of the most frequent consequences of intense bronchitis in children, and is also not rare in adults. With the exception of Case V., these observations, like many others which I have recorded, were made without the knowledge of those of the other authors mentioned above.

CASE V.—*Death from Dysentery—Extensive Collapse of Lung, with Bronchial Abscesses—Emphysema of Lung.*—(P. R. 350.)

A woman, æt. 30,—body examined June 8, 1850.

The body was much emaciated and pale.

Heart normal. Both pleuræ contained a small quantity of fluid, with some floating and adherent soft lymph. The lungs presented great variations in density; the anterior edges were partially emphysematous, but between the

portions thus affected could be felt numerous condensed parts, which, when superficial, presented a somewhat sunk collapsed appearance, and a deep purple colour. At the posterior part of the lungs were considerable masses similarly condensed. On cutting into the pulmonary tissue, there were seen, throughout the condensed portions, numerous small yellow points resembling softened tubercles, but more irregular in outline; these, when scraped with the knife, were found to be bronchial tubes, or small cavities filled with and surrounded by pus. Except at these points, the condensed tissue yielded to the knife only a little sero-sanguinolent fluid, which, when examined under the microscope, contained mostly blood corpuscles, with a few epithelial cells and pus corpuscles.

The colon presented a marked example of follicular dysenteric ulcers, which had destroyed a considerable portion of the mucous membrane.

This case presents an example of the very first stage of the disease, the mucous membrane of the bronchi being as yet scarcely destroyed. It is the same affection of the lung as appears to be figured by Dr Addison in Guy's Hospital Reports, Series 2d, Vol. III., Plate 3. Its resemblance to tubercle, as well as its distinguishing characters, have been perfectly correctly appreciated by Dr Addison.

CASE VI.—*Bronchitis after Fever, with Collapse of Lung—Death from this cause and Dysentery—Bronchial Abscesses.*—(P. R. 62.)

A boy, æt. 11, admitted under Dr Bennett, 13th Dec. 1848, in a delirious state, and with the symptoms of typhus fever. He continued in this state with weak pulse up to the 22d, when the tongue was cleaner, the pulse still weak, and there was slight erythema on the back. Next day cough attracted attention; the chest was somewhat dull at the lower part; there were sibilant râles, and on the 24th slight crepitation over the lower lobes of either lung. On the 27th dysenteric symptoms supervened, and continued till his death. Erythema also spread over the face. On the 6th January there was dyspnœa with clogged bronchial tubes. He died in the course of the night.

The dissection was performed on the 9th January. The body was considerably emaciated.

The posterior part of the left lung was gorged with blood and serum, and crepitated very imperfectly; in most parts of the inferior lobe, and at the lower border of the upper, it was quite dense and contained no air. The condensed portions were not granular on section, but gave out on pressure a considerable quantity of reddish serum; throughout them were irregularly disposed yellowish opaque tubercular-looking points, which could be seen and felt through the pleura, being more resisting than the rest of the tissue; they did not exceed in general the diameter of 2 or 3 lines, and were irregularly circular in form. On pressure, they yielded a quantity of yellowish fluid mixed with a small quantity of air; and most of them could be seen to be perforated by an oval aperture admitting a probe, by which the communication of these cavities with the bronchi could be easily ascertained in all the larger ones. On slitting up a few of them, they were found lined with a reddened injected mucous (?) membrane. The lower edge of the right lung was partially condensed, and contained enlarged and thickened bronchial tubes (?) in the condensed portion.

The mucous membrane of the colon presented extensive ulceration, the characters of which need not be particularly detailed.

It will be seen from the expressions now marked (?) that this, like some of the other cases given, contains what I now consider the

results of imperfect observation. The "thickened bronchial tubes" and the "mucous membrane" here adverted to, are in fact not normal structures, but false membranes supplanting those which have been destroyed by ulceration. Of this I am now well assured by other observations.

CASE VII.—*Intense Bronchitis—Bronchial Abscesses and Gangrene of Lung—Emphysema of Anterior Edges.*—(P. R. 284.)

A labourer, æt. 40, admitted under Dr Bennett, Jan. 6, 1850, with intense cough and dyspnœa. Had been sometime affected with these symptoms, but more severely for a week before admission. Face swollen, lips livid, difficulty in inspiration. Chest arched, very resonant on percussion. Expiration prolonged. Sibilant râles, with inspiration, occasionally. Expectoration tenacious, frothy, muco-purulent. On the 20th he was relieved; on the 25th much worse—feeling of "smothering." 26th, Quiet at night, but dyspnœa very great in the morning. Died at $9\frac{1}{2}$ A.M.

On dissection, the heart was found soft, and its fibres granular under the microscope. Both pleuræ presented adhesions, not very firm. The bronchi were filled with frothy sanguinolent pus, and their mucous membrane was very red. The crepitation of the upper part of both lungs was impaired, but was nowhere completely destroyed over a large space. Distributed throughout the lung were small condensed patches, which gave to the touch somewhat the feeling of tubercles, but on section appeared to be less consistent and more diffused. A few condensed patches of larger size than the majority existed in the lower and anterior part of the left lung, and one of these on section displayed a distinct gangrenous cavity, capable of containing a cherry-stone, lined by a tolerably distinct membrane, and containing a fetid sanies intermixed with debris of fibrous tissue. Anterior edges of both lungs emphysematous.

CASE VIII.—*Measles, with intense Bronchitis—Collapse of Lung—Bronchial Abscesses—Partial Emphysema.*—(P. R. 272.)

A boy, æt. 8, admitted December 19, 1849, under Dr Bennett, suffering under measles, with mucous râles over the chest, which was clear on percussion. On the 24th, he had an attack of croupy breathing, relieved by an emetic. On the 26th, chest symptoms aggravated, with loss of appetite, and fever; crepitating râles all over chest; but no dulness on percussion. *5th January* 1850.—Much dyspnœa, pulse quick and feeble. *6th.*—Dulness observed behind on both sides of the chest. Died on 10th January.

Dissection performed January 11th. The lungs collapsed imperfectly on opening the thorax; they were emphysematous at their anterior edges, and presented some irregularities of surface. Condensed lobules were felt in various parts,—frequently in close contact with, or in the midst of, the emphysematous portions. On section, the lower lobes presented irregularly disposed and irregularly formed yellowish deposits,—the largest of the size of a small bean, frequently perforated by bronchial tubes, and less sharply circumscribed than tubercles.

The last three cases, the history and symptoms of which during life are extracted from the clinical case-books of the hospital, will, I think, appear to the observant physician the type of many others which have come under his notice, especially if his field of observation have embraced the diseases of children, or of hospital patients enfeebled by fevers and chronic diseases. The rapid supervention, and sometimes equally rapid evanescence, of dulness on percussion, limited or diffused, in the midst of attacks of general bronchitis of

greater or less intensity, and when the respiratory sounds are muffled or supplanted by large and small mucous râles in every part of the chest, is an occurrence which has impressed itself on my recollection in numerous instances,—of most of which, however, I have no records, or only notes too meager to be placed before the reader. Such an event comparatively seldom occurs in the acute catarrh of healthy adults; but, as a sequela of measles and hooping-cough in children, it must be familiar to every one; and I have seen it again and again in adults, in the bronchitis of fever,—in that which often terminates in Bright's disease, or disease of the heart,—and still more frequently in those obscure and treacherous chest affections, which supervene in the last stage of exhaustion from hectic or malignant disease, and which are the accompaniments, rather than the active causes, of breaking-up of the enfeebled frame. Such affections have commonly been called latent pneumonia, and they undoubtedly may be so in some instances; but much more commonly they are nothing more than bronchitic collapse, determined by obstruction so slight as would in an otherwise healthy individual be easily overcome, and which, even in these emaciated and bloodless subjects, is only *not* overcome because the exhaustion is great and the need of respiration small,—the circulation being at a low ebb, and the attenuated and sluggish blood requiring very much less air, and consequently less lung, for its renovation than under ordinary conditions. Again and again has it occurred to me, under such circumstances, to open the bodies of persons in whom no suspicion of a respiratory affection existed during life, and in whom, nevertheless, condensation, abruptly lobular or diffused, having often the characters of the "peripneumonie des agonisans," or the "peripneumonie hypostatique" of M. Piorry (both of them varieties of the bronchitic collapse), has been found after death affecting large portions of one or both lungs.[1] On the other hand, I can remember instances in which a superfluous zeal, or *nimia diligentia*, in stethoscopic studies, has detected the signs of these lesions, when not a single rational symptom existed to call for such examination; and very many cases where the extent of the lesion was altogether out of proportion to the gravity of the symptoms that attended its accession.

Apart, however, from such latent, or very obscure cases, it has often been remarked of late years, that the pneumonic affections springing from or accompanying bronchitis, are apt to present a peculiarly asthenic or typhoid character. So much has this been the case in France, that within the last few years "pneumonie catarrhale" and "pneumonie typhoide" have become almost synonymous terms with some authors. Seeing that a great number of cases of so-called broncho-pneumonia which have come under my notice have been instances, not of true pneumonia, but of bronchitic collapse, sometimes combined with engorgement or œdema of the lung, and some-

[1] Instances of this are to be found in Cases I., III., IV., V.

times with bronchial abscesses, I am inclined to believe that the solution of the asthenic or typhoid character of this disease will be found in a modified view of its pathology. The fact is, that in adults the tendency in bronchitis to complete collapse of the lung is comparatively small, excepting in exhausted or debilitated constitutions, or under the influence of typhoid affections of the system. In these the asthenia is not a result of the disease, but one of its most essential predisposing causes.

In the child, again, owing to causes which will be presently mentioned, collapse, in its most complete form, very readily occurs under all circumstances, as a consequence of acute bronchitic attacks. In commenting upon this peculiarity, Dr West, whose opinion on this subject I have before referred to, remarks that it is by the collapse of the lung "that we must explain many of the instances in which urgent dyspnœa, and all the symptoms of serious pulmonary disease, have developed themselves in the course of a few hours out of what had seemed to be a severe cold, or a bronchitis of moderate intensity. This, too, accounts for the occasional supervention of dulness on percussion, and of bronchial respiration in the child; so that you may discover them in the morning in a situation where, overnight, the percussion was good, and no sound of graver import than large crepitation."[1] To this remark I would only add, that dulness on percussion supervening under such circumstances is not necessarily attended with any increase of fever; and that careful attention to the state of the patient in this respect may be in some cases the only mode of forming an opinion with any degree of accuracy as to the presence or absence of true pneumonia in a case of bronchitis. Of this fact a striking instance occurred to me lately in the case of a little boy, three years old, whom I attended during an attack of acute bronchitis. He was a delicate child, and had suffered several times from bronchitic attacks. His chest presented a good deal of deformity, particularly on the left side, of the kind called "pigeon-breast." On the fourth day of the attack, which had all the usual acute characters during the first three days, decided dulness on percussion, with obscurity of respiration, appeared in the right back; the dyspnœa continuing considerable, but *the fever rather abating than increasing*. Under the influence of an emetic, and a forced decubitus on the opposite side to that on which the dulness existed, almost every trace of it had disappeared in thirty-six hours, and the dyspnœa was entirely relieved.

If these views be correct, they cannot be without practical importance. The rules, both of diagnosis and treatment, in regard to bronchitis and its complications, must probably undergo some modifications, in order to be safely followed. Such an axiom as the following, at least, which is one very generally conceived to be unquestionable, will not, I believe, stand the test of renewed ob-

[1] Op. Cit. p. 183.

servation, if the fact of collapse of the lung from bronchial obstruction be kept in view. "The stroke-sound of the chest," says Dr Williams, "is not materially impaired by catarrh; and, accordingly, the partial suspension of the breath-sound in a part of the chest in this disease *cannot be mistaken for that caused by hepatization*."[1] The reader will at once perceive that the clearness of the stroke-sound here alluded to is not always, perhaps not even generally, to be expected under such circumstances; nor can the opposite condition, even when combined with diminished or suppressed respiratory murmur, or with bronchial respiration, be allowed necessarily to indicate hepatization. The judicious physician has doubtless often been saved from error in the application of his remedies in this, as in many other cases where an unsound pathology has prevailed, by adhering to the great principles of our art, and the teaching of personal experience, rather than to received formulas and rules on matters of detail. But it is difficult to escape from the conviction, that the influence of a name, or the vanity of an *exact* diagnosis, may have misled many into such errors; and that in cases of supposed broncho-pneumonia or typhoid pneumonia, the practice may frequently have embraced blood-letting, heroic antimonials, or calomel and opium, when emetics and expectorants, with suitable stimulation, would have been better adapted to the circumstances of the case. This will be still more evident on considering the mechanism of these affections.

Since the preceding observations were in type, I have received the recent work of Dr Fuchs, on the bronchitis of children,[2] which in its elaborate descriptions, both of symptoms and post-mortem appearances, furnishes additional evidence of the connection of collapsed lung with bronchitis. The state of the lung, called "lobular pneumonia" by most authors, and "état fœtal" by Legendre, is regarded by Fuchs as a direct consequence of bronchitis; and its relation to bronchial obstruction on the one hand, and to peculiarities in the infantile system on the other, is certainly more clearly stated than by any previous author. To distinguish this lesion from the congenital form of collapse, as well as from other varieties of condensation, Fuchs proposes the term *apneumatosis*;—but, although evidently anxious to point out a sufficient anatomical ground of distinction between the *unexpanded* and the secondarily *collapsed* lung, he admits that the *diagnosis* must rest chiefly on a consideration of the cause—the one being congenital and the other acquired (See pp. 112, 113). Under these circumstances it will probably, I think,

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[1] Williams on Diseases of the Chest, 4th edition, p. 80. Similar rules of diagnosis are given by Laennec, Skoda, Watson, and most other writers.

[2] Die Bronchitis der Kinder, &c., von Dr Caspar Friedrich Fuchs. Leipzig, 1849. The reader will find a notice of this work in the last No. of the Brit. and Foreign Med. Chir. Review.

appear to English readers unnecessary to burden the science of pathology with another scholastic term; and I am convinced that careful examination of both lesions will convince most observers that Legendre and Bailly are correct, and that there is no real ground for distinguishing them, excepting what may be inferred as to their mode of origin.

The simplicity and clearness of the pathological views entertained by Fuchs, as compared with most other writers on this subject, and the highly original character of his work, render it a most important contribution to the history of bronchitis; and as such, I shall have occasion to refer to it in the sequel. In the meantime, it is only necessary to say that he enumerates three stages of *apneumatosis*, each of which is described at great length. Into the anatomical description of these I do not mean to enter, as it refers exclusively to children below the age of five years. The reader of the original work will see many points of similarity between the characters there given and those which I have indicated as distinguishing bronchitic collapse in the adult as well as the child. In the case of children under five years, Dr Fuchs, indeed, denies having seen a true pneumonic condensation; and he appears also to have passed over, very lightly, the lesion above described as bronchial abscess; the only allusion to it being in p. 114, where he remarks, cursorily, on "the accumulation of yellow mucus in the bronchi and air-cells, *and their dilatations;*" this being, as he declares (in opposition to Friedleben, as well as to other authors), the only form of suppuration in the lung observed in early infancy. These assertions are entitled to due consideration, but can scarcely be accepted without further, and even more careful, examination.

It appears from the work of Dr Fuchs, that, in 1837, a year before the publication of the monograph of Rilliet and Barthez, Seifert[1] had given an excellent description of infantile bronchitis, and of the peculiar pulmonary lesions in which it commonly terminates, which he considered to be a peculiar form of pneumonia. This work seems to have excited some attention in Germany, and it is not a little remarkable, under these circumstances, that the close relation of these lesions to the congenital affection described by Jörg, should have been overlooked until the observations of Legendre and Bailly, in France, seven years afterwards. Seifert also noticed the resemblance of the "broncho-pneumonia" of children, to the "peripneumonie des agonisans" of adults; and seems to have described the bronchial abscesses as a stage of suppuration.

Mechanism, Causes, &c., of Bronchitic Collapse.—The object of the remarks hitherto made, in reference to this subject, has been chiefly to establish the frequent coincidence and probable relation of cause and effect between the obstruction of the bronchi and the col-

[1] Die Bronchio-pneumonie der Neugebornen. Philipp Seifert. Berlin, 1837.

lapse of the air-vesicles. The nature and rationale of that connection now fall to be more particularly considered.

When a bronchial tube is in any way obstructed, or much diminished in caliber, at one or more points, the question arises, what is the mechanical effect of the movements of respiration upon the amount of air thus partially imprisoned behind the obstructed part? If the obstruction be complete, of course no change can take place, at least mechanically; but in the case of its being, as it usually is, incomplete, it may be supposed that the inspiratory act tends to draw in more air than the expiration can expel, and consequently that the air tends to *accumulate* in the vesicles; or, *vice-versa*, that the air behind the obstruction tends constantly to *diminish* in amount, owing to the comparative inefficiency of the inspiratory act; or finally, that the forces equalise each other, and the quantity of the enclosed air *remains unaltered.*

Now, it cannot be denied that from the pathological anatomy of bronchitis, a *prima facie* case might be made out for each or all of these theories; for, although only one side of the question has hitherto been brought prominently forward in the preceding part of this paper, the reader will not have failed to remark that in several cases in which bronchitic collapse of the lungs existed, other parts of the same organs were affected with emphysema or dilatation of the air-cells. (See Cases III., IV., V., VII., VIII.) Indeed, so familiar is this conjunction of emphysema with bronchitis, as to have suggested to Laennec, long ago, the first of the three theories above mentioned as to the cause of dilatation of the air-vesicles in that disease. Again, fatal cases of bronchitis undoubtedly occur, at least in adults, in which there is no change of the pulmonary texture so marked as to afford support to either of the first two theories.

Laennec's view of the consequences of obstruction was founded on the idea of the comparative weakness of the expiratory, as compared with the inspiratory, forces. "The mucus secreted into the bronchi, in consequence of pulmonary catarrh, must, especially if it is very viscous, present a great resistance to the free passage of air in inspiration and expiration; and we shall show, in speaking of the râle, that this resistance often goes the length of producing complete, though momentary, obstruction of a part of the bronchial ramifications. Now, as the muscles which subserve inspiration are strong and numerous, while expiration is produced only by the elasticity of the parts, and the weak contractions of the intercostal muscles, it must necessarily happen that the air, which has been forcibly driven beyond the obstruction in inspiration, will not be able to overcome it in expiration, and will be in a manner imprisoned, by a mechanism not unlike that in the butt-end (condenser) of an air-gun." I have given this passage from Laennec entire, because, notwithstanding the palpable fallacy it contains, it has been referred to, and its conclusion adopted without comment, by almost every systematic writer in this country as well as in France. The fact is, however, that though

ordinary inspiration is more of a muscular act than *ordinary* expiration (merely because in the latter there is little or no resistance to be overcome, to which the elastic subsidence of the parietes is not adequate), yet the residual effective force for overcoming adventitious obstruction is very considerably greater in expiration. The *forced* or *muscular* expiratory act is, in fact, about one-third more powerful, as measured by its effect upon a pressure-gauge, than the extreme force of inspiration;[1] and it is this force which is thrown into action when obstruction in the tubes is to be overcome. In the act of coughing, moreover, we find a beautiful mechanism, by which the air within the vesicles is discharged outwards at a *maximum amount* of pressure, and brought to bear with all the additional mechanical advantage of a sudden impulse, on every obstructing substance within the bronchial tree,—a cumulative provision which does not exist in the case of the inspiratory force. There can be no great difficulty, therefore, on these grounds, in coming to the conclusion, that the data of Laennec's hypothesis are quite erroneous, and that the practical efficiency of the expiration in forcing air through obstructions must be, *cæteris paribus*, far greater than that of the inspiration. I have already alluded to the fact, that this is consistent with general experience; for while the inspiratory act is always, in bronchitis of considerable intensity, attended with extreme difficulty, the expiration is never so.

The question of the origin of emphysema of the lung will be considered in the second part of this memoir, in which I shall endeavour to account for its connection with bronchitis, by referring its production to a totally different mechanism from that just mentioned. In the meantime, I would remark that, in order to establish a direct relation between this lesion and bronchial obstruction, it is necessary to show not merely that emphysema occurs in connection with bronchitis, but that it occurs especially or exclusively *in those parts of the bronchitic lung where obstruction can be shown to exist.* This is the proposition which I conceive the preceding pages have tended to establish as regards bronchitic collapse, and on the ground of which I have argued for the relation of cause and effect between this lesion and obstruction. In how far does emphysema fulfil these conditions?

It is well known to every one who has studied the anatomy of this pathological state, that the emphysematous portions of a lung can generally be inflated from the bronchi with the greatest ease. Indeed, so far as my own experience in this matter is concerned, I cannot recal any instance in which the pressure of the air was not found to reach the emphysematous parts with as great rapidity as the rest of the lung. In the collapsed lung, on the contrary, as I

[1] See the numerous experiments of Hutchinson and Mendelsohn, quoted in Dr J. Reid's article on Respiration: Cyclop. Anat. and Physiol. Part 32, p. 336.

have already shown, very considerable resistance is often opposed to its inflation from the bronchi—a resistance only to be overcome by pressure many times greater than can ever occur in the vital act of inspiration. If this observation be correct, it is plain that the emphysematous parts of the lung are usually free, the collapsed parts obstructed.

Further, I cannot find that any unequivocal instances have been adduced, to prove that an obstruction, confined to a part of a lung, or to one lung only, is commonly accompanied by a corresponding distribution of emphysematous portions—a proposition which, if true, might surely be easily verified from the records of pathology. So far from this being the case, the habitual seat of emphysema leads to an inference of a directly opposite kind—a point which appears to have been overlooked in the discussion of this question. Let the reader reflect that, in the vast majority of instances, the seat of election of emphysema is the *anterior* border of the lungs, while the stethoscope, as well as the results of *post-mortem* examination, show that accumulations of mucus in bronchitis occupy in an equally numerous proportion of cases, the *posterior* and *lower* parts, which are also, especially in the adult, the principal seat of the bronchitic collapse.

Cases of the impaction of foreign bodies, and other palpable obstructions of the bronchi, are generally recorded with too little attention to the condition of the lungs to be available for the present discussion. Carswell has, however, figured the case of a monkey, in which the left bronchus was much compressed, or rather obliterated, by a mass of tuberculous glands; in this case the corresponding lung had diminished to less than a third of its normal bulk, while the *opposite* lung, of which the bronchus was free, presented emphysema in several places.[1] Andral has adduced, as before mentioned, two cases of obstruction of the upper lobe of one lung, where, from the stethoscopic phenomena, he supposed emphysema to be present, but where the examination after death proved that this was not the case.[2] He has also recorded a case[3] in which the bronchi of the right lung were compressed by a melanotic mass, and the respiratory murmur greatly enfeebled. In the short note of the appearances after death, no notice is taken of any abnormal condition of the lung in this case. Andral, indeed, states (p. 196) that emphysema is one of the consequences of stricture of the bronchi; but adduces nothing whatever in proof of this assertion, which evidently rests on the ground of Laennec's theory.

All doubt, however, as to the real effect of a solid obstruction in the bronchi on the air in the lung is removed by the direct experiments of Mendelsohn and Traube on animals.[4] The former inserted

[1] Illustrations of the Elementary Forms of Disease—Atrophy. Plate iv., fig. 3.
[2] Clinique Médicale, v. 2, pp. 187-190. [3] Ibid, p. 193.
[4] For an account of these experiments I am indebted to the work of Fuchs, not having access to the original sources.

a leaden shot into the trachea of a dog, pushing it down as far as possible into the bronchus with a probe. In another instance he inserted a ball of paper. In both cases, the parts to which the obstructed bronchi led were red and void of air. In the former there were emphysematous portions in the other parts,[1] and in the opposite lung.[2] Traube's experiments were similar, but more numerous. The general result was, that the artificial obstruction of a bronchus always produced expulsion of the air from the corresponding part of the lung, which had a dark-red colour, and presented the characters of collapse.[3]

It is clear, therefore, from experiment, as well as from pathological observation, that the most usual and most direct effect of obstruction, or of diminished caliber of the bronchi, however caused, is not accumulation but diminution in quantity of the air beyond the obstructed point. It is probable that this is due in part to the comparative weakness of the inspiratory power, and that the proposition of Laennec may, therefore, correctly enough be inverted. There is also, however, another mechanical condition which comes into play in producing collapse from obstruction, especially in the case of a viscid plug of mucus, which is most commonly, in bronchitis, the source of this affection. This condition is to be found in the form of the tubes.

The bronchi are a series of gradually diminishing cylinders, dividing, for the most part, dichotomously. If a plug of any kind, but especially one closely adapted to the form of the tube, and possessing considerable tenacity, be lodged in any portion of such a cylinder, it will move with much more difficulty towards the smaller end, and in doing so will close up the tapering tube much more tightly against the passage of air, than when moved in the opposite direction into a wider space. If such a plug be placed over a bifurcation, it will, even if freely moving in the larger space in which it lies, be of sufficient bulk to fall back upon one or other of the subdivisions during inspiration, in the manner of a ball-valve upon the orifice of a syringe, and thus completely to occlude it. (See diagram.) The consequence of this mechanical arrangement must inevitably be, that at every expiration a portion of air will be expelled, which, in

[1] The expression of this passage is not quite clear, but this is certainly the meaning, and corresponds with the author's inference.

[2] Der Mechanismus der Respiration und Circulation, p. 37. Berlin, 1845. Mendelsohn also threw a solution of gum into the air passages of an animal, with the result of collapse of some portions of the lung. In one instance I tried this experiment upon a rabbit, with a similar result; but the difficulty of limiting the fluid to particular parts of the lung makes these experiments less valuable.

[3] Beitrage zur experimentellen Pathologie und Physiologie, 1 Heft.

inspiration, is not restored, partly owing to the comparative weakness of the inspiratory force, and in part to the valvular action of the plug. If cough supervene, the plug may be entirely dislodged from its position, or expectorated, the air of course returning freely into the obstructed part; but if the expiratory force is only sufficient slightly to displace the plug, so as to allow of the outward passage of air, the inspiration will again bring it back to its former position, and the repetition of this process must, after a time, end in perfect collapse of the portion of lung usually fed with air by the obstructed bronchus.

It is not a little surprising that this simple and clear mechanical mode of explaining the collapse should not have occurred to Dr Fuchs, who, in accounting for the disappearance of the air in the experiments of Mendelsohn and Traube, finds himself reduced to the theory of its absorption into the blood-vessels. (Op. Cit., p. 63.) Surely nothing can be more superfluous than such an explanation.

In considering, as a whole, the causes which tend to produce bronchitic collapse (as revealed in the preceding investigation), they seem to resolve themselves into the following:—*Firstly*, the existence of mucus in the bronchi, which is more liable to produce obstruction according as it is tenacious and viscid; *secondly*, weakness, or inefficiency of the inspiratory power, however caused; *thirdly*, inability to cough and expectorate, and thus to remove the obstructing mucus. Of these conditions the first must be considered as the immediate exciting cause, the others as predisposing causes, co-operating with the first, but incapable without it of producing collapse. Of the exciting cause enough has already been said. The inability to expectorate is obviously enough a formidable condition, and may be owing either to simple debility, or to a laryngeal affection. But I have still a few remarks to make on the circumstances producing inefficiency of the inspiratory act, and their bearing on the present subject.

The inspiratory act is apt to be rendered inadequate from several causes. Of these the most obvious is weakness of the muscles of inspiration, usually concurring with general debility. I have already pointed out the great predisposition to bronchitic collapse which arises from an exhausted frame; so much so, that a barely appreciable amount of bronchitis, nay sometimes, I believe, the mere accumulation of the natural mucus in a debilitated subject (as in an individual near death), will give rise to a considerable extent of the pulmonary lesion. I cannot, however, see reason to believe with Dr West, that mere debility, apart from any obstruction in the tubes, is a sufficient cause for collapse in the child. The very fact of the lesion being usually more or less lobular, or partial in its distribution, appears to indicate special circumstances of a local kind, as having a marked influence on the production of this affection; and on this ground, as well as that of theory, I am disposed to think that, in the cases ascribed to debility alone (including the third case of Dr Baly, before alluded to, p. 137), the evidences of more or less obstruction might have been detected during life or after death. That

this, however, is often of a very slight character, leading to scarcely any symptoms, and probably in some cases undiscoverable, except by physical examination, I have already indicated. (See p. 235; also, Cases I., III., and IV.)

A second circumstance tending to render the inspiratory act ineffective is distension of the abdomen, impeding the descent of the diaphragm. The influence of this condition in the production of collapse I have repeatedly witnessed; having had occasion to observe that when ascites, or any other cause of similar distension, is present, a very slight amount of bronchitis will determine extensive pulmonary collapse. In Case I. this cause was in action, combined with great exhaustion; and one of the first cases that awakened my attention to the subject of this memoir, was that of a boy of 17 years of age, who died of an enormous medullary tumour of the abdomen, and in whom a most marked form of lobular collapse was found distributed over both lungs, with a good deal of thick mucus in the tubes.

A third cause of inefficiency of the inspiratory act, and one of the greatest importance in relation to this subject, is the want of due resistance on the part of the thoracic parietes. The full dilatation of the lungs is only effected when the depression of the diaphragm is accompanied by the elevation of the ribs and widening of the thorax; and if the bones of the latter be very yielding, the external muscles of inspiration cannot, of course, act effectively under an obstruction. This is obviously the reason of the greater tendency in children to collapse of the lung as a consequence of bronchitis. The respiration of the child is at all times, even in health, more diaphragmatic than that of the adult; and the observations of Rilliet and Barthez[1] afford satisfactory evidence of the comparatively small dilatation of the thorax in children, particularly of its lower part. When any obstruction exists to the entrance of air into the chest, even this small dilatation ceases, and collapse of the lung very readily takes place. Under these circumstances, Dr Rees[2] has pointed out that in very young children the motions of the chest are absolutely reversed, and instead of the descent of the diaphragm being accompanied by expansion of the chest, the ribs give way beneath the exhaustion caused by it within the thoracic cavity, and bend inwards to accommodate themselves to the collapsed lung in inspiration. The altered movement of the chest in infants is regarded by Dr Rees as pathognomic of *atelectasis*. It is also a prolific source of that permanent deformity of the chest which, in the early years of life, is often ascribed, with too little discrimination, to rickets.[3]

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[1] Op. cit., vol. iii., pp. 643-4.

[2] Atelectasis Pulmonum. Lond. 1850.

[3] Rilliet and Barthez describe a reversed movement of the ribs in inspiration as taking place in rickets. There can be little doubt that rickets, combined with chest affections, form a frequent source of the deformity; but the presence of the latter is probably essential. Vol. iii., p. 646.

Of this deformity I shall have something to say in the second part of this paper.

As to the so-called *atelectasis*, I have had but few opportunities of observing it accurately during life. I may, however, remark that in respect to its causation, it probably differs but little from the acquired collapse, and close examination would probably show that mere debility, without some obstruction in the bronchi, is as inadequate to prevent the expansion of the lung as to cause its collapse. The cases published by Jörg himself, although very imperfectly observed as regards physical signs, will, I think, on perusal, convince a careful reader that there is abundant ground for this opinion; but undoubtedly the subject requires renewed investigation.

Origin of Bronchial Abscess.—The mechanism of this lesion it is not difficult to explain satisfactorily. When pus accumulates in the central bronchi of a collapsed lobule, the evacuation of that pus is prevented from occurring, firstly, in consequence of the absence of the expiratory *vis a tergo;* and secondly, from the resistance opposed by the thickened mucous membrane and its secretion, closing up the bronchus in front. The coats of the ultimate bronchi, therefore, softened and injured by disease, gradually give way to ulceration; and the pus, which thus accumulates in still larger quantity, may at first scarcely be circumscribed, but soon begins to be surrounded by a false membrane exactly similar to that of an abscess in any other part of the body. The continuity of this membrane with that of the original bronchus, may be either maintained from its first formation, or it may be secondarily established. I believe, however, that the first of these views is the correct one; and that the bronchus acts the part of an obstructed fistulous opening, not sufficiently pervious to prevent accumulation entirely, but not permitting of its increase beyond a certain amount.

When the bronchial abscess has been of some standing, and the patency of the tube leading to it has become re-established in time to prevent its obliteration, a process of repair takes place, analogous to the cicatrization of a wound, and perfectly similar to that which is observed in all healing excavations in the lung, however formed. The false membrane which lines the cavity becomes intimately blended with the bronchial mucous membrane, and indeed comes to resemble it so closely that it is almost impossible to tell where the true mucous membrane ceases and the new structure begins.

This reparation, however, is rarely, if ever, accompanied by restoration of the perfect function and structure of the lung; and on this account it will be considered under the permanent effects of bronchitis.

The length to which these remarks on collapse of the lung have extended, can only be excused by the immense importance of this lesion in relation to the pathology of bronchitis; the whole of the

organic affections following from which seem to me more or less dependent on that which has formed the principal subject of these observations. These secondary effects of bronchitis and bronchitic collapse will form the subject of a second part of this memoir, as indicated at the commencement of it.

(To be continued.)

Part Second.

REVIEWS.

On Animal Chemistry in its Application to Stomach and Renal Diseases. By H. BENCE JONES, M.D., A.M., Cantab, Fellow of the Royal College of Physicians, Physician to St George's Hospital. 8vo. pp. 139. London, 1850.

DR BENCE JONES is already favourably known as the author of works and papers on animal chemistry, and this contribution to his favourite science is calculated to extend his reputation as an able chemist and sound physician. The volume consists of twelve lectures, delivered to the students of St George's Hospital at the conclusion of the course of chemistry, in 1849. The food, respiration, digestion, and the blood, form the subjects of the first three lectures; calculi, that of the fourth; while the remaining eight are devoted to the quantitative and qualitative variations in the urine. The first four lectures contain little that is not to be found in works with which our readers are already familiar, while of the whole book it may be said that it contains less of what is new than we might have expected from its author, writing on such a subject. There are, however, many important points discussed in these pages; and views, which the author has long entertained, are more fully, and perhaps more clearly, stated than in any of his former papers. We shall endeavour to direct attention to as many of these as our space will allow. In doing so, we must necessarily be brief, referring our readers desirous of further information, to the author's several papers published in the Philosophical and Medico-Chirurgical Transactions, and in the Lancet, and to the present work itself.

The observations to which we shall here refer are contained in the fifth and subsequent chapters. On the subject of the acidity of the urine, the author has made a number of interesting observations. He has found that urine passed the longest after food has been taken, is generally the most acid; and that that

passed while digestion is going on, is three or four times less acid; and that sometimes, even in health, it is alkaline. Immediately before each meal, in the author's experiments, the urine showed the highest degree of acidity. When no food was taken, the acidity did not fall, but for nearly twelve hours continued at the same degree. As the results of his experiments, we find the following conclusions arrived at: That the acidity of the urine is always changing, and that the changes depend on the state of the stomach; while much acid is in the stomach, the acidity of the urine is increased, and it usually reaches its highest limit before food is again taken. Animal food causes a greater oscillation than vegetable; and when no food is taken, the variation is very slight. The author professes himself unable to answer the question, upon the presence of what substance the acidity of the urine depends; and though he believes that, in many instances, the existence of the acid phosphate of soda may account for it, he admits that this explanation will not apply in all.

Dr Prout conceived the acidity to depend on the existence of an acid lithate of ammonia, a substance which Dr Jones does not believe to exist. Apart, however, from this, the impossibility of the acidity of the urine being dependent on the presence of urate of ammonia, is shown by a reference to the constantly observed facts (and to these Dr Jones alludes), that when the urine is very acid, there is usually but little urate of ammonia present, and that, when there exists a great excess of that salt, the reaction of the urine is not unfrequently alkaline.

In the sixth lecture, we have the results of the author's observations on the variations of the uric acid in the healthy state. The method adopted by Dr Jones in estimating the amount of uric acid, is that generally followed: the addition to a certain amount of urine, of a certain amount of strong hydrochloric acid; this done, the mixture is allowed to stand for about 24 hours. The first cloudiness which ensues, the author conceives to be owing to a deposit of urate of ammonia, and for the following very satisfactory reasons,—it is devoid of crystallization, appears granular under the microscope, and is dissolved by heat. The coarse powder which afterwards takes the place of the amorphous sediment is crystalline uric acid. The author's experiments show that the amount of uric acid in the urine is influenced by the food. That the kind of food taken appears to have little influence on the variation, and that the effect of exercise is not marked. The precipitation of urate of ammonia is believed by Dr Jones to be the result of three conjoint causes,—1. Decrease of temperature; 2. Increased proportion of urate of ammonia to water; 3. Increase in acidity of the urine. The precipitation of uric acid, he adds, is due solely to the acidity of the urine. The causes which influence the various forms in which uric acid crystallizes, the author does not attempt to explain. On the treatment of the deposits of uric acid

and urate of ammonia, we quote the following judicious observations :—

"If uric acid crystals are present, there are two things to be done,—namely, to give alkalies, and to forbid and remove all that may become acid. For the first, caustic alkalies, carbonated alkalies, and earths, saline draughts and phosphate of soda—these must be regulated according to the peculiar symptoms of the patient ; for the second, vegetable acids,—sugar and starch, should as far as possible be prohibited. The removal of acids by the skin, and carbonic acid by free respiration and exercise, is of still greater benefit; shortly the uric acid crystals indicate that acidity must be removed, neutralized, and prevented.

"When uric acid crystals are absent, and urate of ammonia is deposited alone, it is a proof that no great excess of acid is present in the urine, otherwise uric acid crystals would be formed ; and though, by lessening the acidity of the urine, we can keep the urate of ammonia dissolved in it, and so hinder it from appearing, yet this is not the best mode of proceeding for effecting even this purpose. An extra glass of water, or soda-water, or some slight diuretic, as nitre, is more sure to keep the urate of ammonia in solution by increasing the quantity of urine, and this is the best palliative treatment; whilst the curative consists in lessening the quantity of food taken, by smaller meals, and in lessening the acidity of the urine by increased exercise."—P. 61.

The seventh lecture is chiefly devoted to a consideration of the deposits of oxalate of lime. At its commencement we find the following observations :—

"Oxalate of lime is so frequently found in the urine of those who are in a good state of health, that I do not consider it as indicating any disease, but only a disorder of no serious importance. It scarcely indicates a more serious derangement of the general health than a deposit of urate of ammonia does."

We are certainly surprised that after all that has been written of late on the subject of the oxalic diathesis, the experience of so competent an observer as Dr Bence Jones in regard to it, should be such as we have now quoted, and that he should regard it as a disorder of no serious importance, one of which Dr Prout has written, that the bodily suffering it entails is considerable, while the mental excitement verges on insanity. Such an account of those labouring under the oxalic diathesis, as has been given by Dr Golding Bird, and by Dr Begbie in this journal, will, we apprehend, be found the correct one; and we feel persuaded that in their writings there exists abundant proof to justify the conclusion, that with the deposit of oxalate of lime in the urine there is associated an intense form of dyspepsia. It is, on the other hand, undoubtedly true, that oxalate of lime may exist in the urine under other and very different circumstances. It may be found, as our author states, in the urine of those who are free from every complaint; though we have never seen its *persistent* presence unattended by dyspeptic symptoms. It is frequently found in the urine of those suffering from acute diseases, as rheumatism and cholera, and from organic diseases, more especially in the abdomen, and chiefly of the stomach and liver. In eruptive diseases its presence in the urine

is frequent; and we know that Professor Bennett has accomplished cures in many such cases, more especially in papular eruptions, after detecting oxalate of lime in the urine, and directing treatment accordingly. The author, from frequent observation, has come to the conclusion that sometimes many hours are necessary for the oxalate of lime to crystallize out, and he believes it impossible to say that no oxalate of lime exists in any given specimen of urine until at least twenty-fours have elapsed from the time of the urine being passed. We have long been satisfied of the truth of this observation, which we regard as a very important one, for we are equally satisfied that in many instances the presence of this salt has been overlooked from the examination of the urine having been commenced at a too early period after micturition. The author continues to meet with the oxalate of lime crystallized in the form first observed by M. Donné, namely, of little flattened discs like very small blood globules. The remainder of chap. vii. is devoted to what Dr Jones has denominated the sulphuric diathesis, that is an excess of sulphates in the urine; but as he promises to bring forward some observations in regard to the variation of the sulphates in disease, we shall not refer to their chemical history here.

The eighth Lecture is occupied with the consideration of the alkaline and earthy phosphates, a subject to which the author has paid particular attention, and perhaps more than any of the other chapters this merits perusal. Here we can only quote a few of the general conclusions he has arrived at. In the healthy state, he believes:—

"1, That the amount of earthy phosphates depends chiefly on the amount of earthy matter taken into the body. 2, That the amount of alkaline phosphates depends chiefly on the food, but that exercise also causes some increase of these phosphates. These being soluble in fixed alkalies and in carbonate of ammonia, never fall as a precipitate when the urine becomes alkaline, though the earthy phosphates then become apparent."

After a long inquiry into the total amount of phosphates in various diseases, the conclusions at which the author has arrived are the following:—

1. "The variations of the earthy phosphates are so dependent on the earthy matter (lime and magnesia) present in the urine, that no deduction from them, as to the nature or state of the disease, is possible.
2. "Neither the earthy phosphates, nor the alkaline phosphates, are permanently increased in special diseases.
3. "In fevers and acute inflammations of fibrous, muscular, or cartilaginous tissues, the total amount of earthy and alkaline phosphates is not increased.
4. "In chronic diseases, in which the nervous tissue is not affected, no deduction can be drawn.
5. "Chronic cases of mania, melancholia, and general paralysis of the insane, gave no marked results.

6. "In chronic diseases of the brain, and in chronic, and even acute, disease of the membranes, there is no increase in the total amount of earthy and alkaline phosphates.

7. "In fractures of the skull, when any inflammation of the brain comes on, there is an increase in the total amount of phosphates. When there are no head symptoms, no increase of the phosphates is observed, even when other acute inflammations supervene.

8. "In acute inflammation of the brain there is an excessive amount of phosphates in the urine. When the inflammation becomes chronic no excess of phosphates can be shown to exist in the urine by the method of analysis employed.

9. "In some functional diseases of the brain an excessive amount of phosphates is observable; this ceases with the delirium. Delirium-tremens shows a remarkable deficiency in the amount of phosphates excreted, provided no food is taken. When food can be taken the diminution is not apparent."

We find an important distinction drawn in chapter 9th, between ammoniacal and alkaline urine. Whether or not there exist other ways by which the urine is rendered ammoniacal, there is one very easily understood way, namely, by the conversion of urea into carbonate of ammonia. Urine then, in which this change has occurred, is ammoniacal, but alkaline urine owes its alkalinity not to this change, but to the presence of a fixed alkali.

In concluding chapter ten, which treats of albuminous urine, the author refers to the microscopic appearances of the urine in Bright's disease. These he distinguishes into five different objects, 1. Ordinary mucous globules; 2. Laminar epithelium; 3. Blood globules; 4. Round dark globules, filled with granular matter; 5. Tubes or amorphous matter adhering in the form of tubes.

All these objects we have frequently seen in the urine of Bright's disease, as also another which is occasionally met with in the same urine, namely, pus corpuscles. We suspect, however, that under the name of altered epithelial cells or pus corpuscles, the mucous globules of Dr Jones would be more correctly classed. We find no observations as to the comparative frequency of these different objects in the urine, or the particular features of the disease, or the peculiarity in the ordinary characters of the urine which accompany them; nevertheless all these are most important subjects, on which we should like to have had the author's opinion; for, as yet, the diagnostic value of the microscopic deposit in Bright's disease is not satisfactorily made out. We have found that it is at that period of the disease, when the urine is passed in large quantity, when it is of low density, and pale in colour, that the tubes and epithelium are chiefly present; and, on the other hand, it is when the urine is passed in diminished quantity, of higher sp. gr., and darker in colour, that the exudation or granular corpuscles, and the blood globules, are commonly observed. These two appear to mark different periods of the disease.

Diabetes and diuresis are the subjects of the eleventh lecture. On the pathology of the former we find no new light, though the observations offered are of great interest. The author believes

diabetes to be a more common disease than is supposed; and that, if the tests for the presence of sugar in the urine are applied, its existence will often be detected before any symptoms have appeared. Referring to the views of Prout, Berzelius, and Leibig, the author concludes his lectures by a consideration of the relation which the urine bears to the food and system. He is especially desirous of proving that chemical actions are always taking place in the body, and that the action of oxygen never ceases. To the proof adduced in favour of the first of these propositions, there is no occasion here to refer; the facts brought forward are considered as established, and never likely to be called in question. As illustrating the action of oxygen, we shall quote a somewhat fanciful observation :—

"Let me say a few words on the colour of the urine. Yellow, brown, and pink—these are the colours which, in every shade and mixture, are to be met with in the urine. If I asked you where else in nature you could find these three colours in every variety, you would probably answer, in the changing leaves of autumn. The pale yellow of the poplar, the bright yellow of the ash, the brown of the oak, the pink of the clematis, the most changing shades of the leaves, might be matched in the urine of health or of disease."—P. 130.

We must conclude with another short quotation upon a most important point. Speaking of the value of the examination of the urine, the author says, "As knowledge extends, the examination of the urine will be more generally made, not only in Medical, but in Surgical cases also. I have known the removal of a doubtful cancer of the lip cause death, which might have been delayed for a time by examination of the urine,"—it was a case of diabetes —"and I have known the simple removal of a pile cause death, where the patient had albuminous urine." We heartily concur in the wish implied in these observations; and here take leave of our author, from whom we have differed in one or two points, the which we commend to his reconsideration. Of the value of his work we entertain a high opinion, as an evidence of much labour and patient investigation.

On Excision of the Enlarged Tonsil, and its Consequences in Cases of Deafness; with Remarks on Diseases of the Throat. By William Harvey. London, 1850.

The object of this publication is to oppose Excision of the Tonsils as a remedy for deafness. It appears that of late years this operation has been introduced into London, where it was reprobated long after being generally practised in Edinburgh, for the purpose of correcting defects in hearing and articulation; and like many other good means of treatment has been greatly abused through misapplication. The absurdity of trying to cure stammering by cutting

out the tonsils is too obvious for serious consideration, and we may therefore pass at once to the other subject of question.

One of the effects of enlarged tonsils is deafness; and when it depends upon them, the removal of their redundant portion affords a corresponding measure of relief. There is thus a pretext obtained for recommending the operation to deaf people whenever the practitioner finds or chooses to allege that the glands are enlarged. Of all patients those labouring under imperfect hearing are the most submissive and enduring, so that, whatever may be proposed for their benefit, is sure to meet with a ready assent. If the tonsil has been cut out in vain, no objection is made to curtailment of the uvula, and afterwards, in succession, to probing of the Eustachian tube,—puncture of the tympanum,—and the endless farrago of liniments, lotions, and galvanic experiments, which are prescribed so long as the fees are forthcoming. As the *curable* diseases of the ear belong to the practice of physic or surgery, in accordance with the established principles of which they are remedied with more ease and certainty than most other ailments to which the body is subject, it is only the incurable cases which properly pertain to the aurist, who has thus an ample field for the system of proceeding just mentioned. It is impossible to put down popular delusions by the force of reason; but it is not the less incumbent upon honest practitioners to purge their character of any unworthy imputation; and we therefore think Mr Harvey has done right in disclaiming any share of "the thousands of cases," in which excision of the tonsils has been performed for deafness, without relief. Sure we are, that if the tonsils had been really enlarged and the sole cause of deafness, the results would have proved different.

But Mr Harvey denies that deafness ever depends upon enlargement of the tonsils, or admits of remedy by their removal; and here we must entirely differ from him,—appealing to facts daily within reach of observation, and leaving the reader to be guided by their interpretation. He also alleges that the operation may impair the power of hearing—derange the patient's health—and cause wasting of his testicles—on the ground that some nice observers have traced a sympathetic connection of development between the tonsils and last mentioned organs. But, granting this observation to be correct, we cannot see that it opposes any greater objection to shaving off the redundant glands of the throat, than to depriving the chin of its beard, which we never knew any body suspect of causing impotency.

Rejecting excision for the treatment of enlarged tonsils, Mr Harvey recommends as a specific for their reduction within proper dimensions, six drops of colchicum wine to be taken three times a day. We should not object to this very harmless mode of affording relief if it were to prove effectual, but do not think that the evidence of its doing so is at all satisfactory, since the instances related are

almost all cases of adults, and suggest the idea of cachectic sore-throat rather than simple hypertrophy of the tonsils. We, therefore, retain our persuasion, that the only safe, easy, and effectual remedy for the enlargement in question is excision; and, as to the bad consequences alleged to result from removal, we feel comforted by their entire and invariable absence in all the cases, now amounting to many hundreds, which have fallen under our observation.

Before taking leave of Mr Harvey, we would beg to remark, that in a practical treatise addressed to readers, who must be supposed to have studied their profession, there is hardly occasion for such very elementary information as, "The amygdalæ or tonsils, are two glandular bodies, situated, as I have already said, in the narrow passage or isthmus between the mouth and the muscular membranous bag called the pharynx. Leading into this bag there are seven openings," &c., p. 3; or "An attentive, and especially an instructed, observer, looking into the throat, will readily make out, 1, the uvula; 2, the pendulous palate; 3, the isthmus of the fauces, and the tonsils on either side," &c., p. 4. We would also beg to suggest, that in quoting from the works of others it is usual and convenient to mark the passages so borrowed by inverted commas. If this be not done the reader may readily attribute to the author information proceeding from quite a different source. Thus, at p. 64, and onwards, the statements delivered in the first person singular, without the slightest distinctive mark from the context, would hardly be recognised as a transcript from another work, unless by those who happened to have read the excellent observations of Dr Green, whose name, doubtless, is previously mentioned, but without any link of connection.

A Manual of Materia Medica, Pharmacy, Toxicology, &c. By Edward Chapman, M.D., &c. Edinburgh, 1850. Pp. 416.

The author of this Manual lays no claim to originality—his work avowedly consisting of a compilation from the Pharmacopœias of Britain and Ireland—from the different Dispensatories, and other more extended treatises on the Materia Medica. Something of the kind Dr Chapman conceived to be required by students, "not as a substitute for the larger works, but to give them a zest for the more extensive study and cultivation of the science."

The volume is of reasonable dimensions,—contains a judicious selection of useful information drawn from the sources above indicated, arranged in alphabetical order,—is clearly printed,—and, so far as our examination has permitted us to judge, very accurate. We must, however, express a doubt whether the perusal of the dry details of such a Manual will fulfil the expectations of the author by prompting to farther research. We can conceive no duller

reading than an alphabetical Pharmacopœia, except, perhaps, a treatise on the natural orders of the vegetable kingdom unillustrated by drawings, for which most unpalatable of studies the requisite facilities have been provided by Dr C. in an appendix. We recollect to have read of an Italian bandit under sentence of death, who, when offered a free pardon on condition of his reading Guicciardini's History of Italy from beginning to end, demanded to be instantly led to the block. Yet Guicciardini's work is one of great merit, albeit a little prosaic, and not likely to awaken a taste for historical reading. The criminal's antipathy would have been manifested, though possibly in a lesser degree, had he been condemned to a course of Greek Lexicon, Times newspaper, or Dr Chapman's Manual,—things all good in their way; but when carefully read from title to colophon, not exactly provocative of farther study.

The chief use of Dr C.'s Manual will be to assist gentlemen attending those short courses of practical pharmacy given at our dispensaries—courses to which a previous attendance on systematic lectures on Materia Medica is considered an essential preliminary, and which are supposed to confer the finishing galenical polish upon the student.

The work contains one dangerous erratum which it is our duty to point out: Fleming's tincture of aconite cannot be safely given, two or three times a-day, in doses of from 3 to 8 minims, as stated at p. 21.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

REPORT OF CASES OF CARDIAC DISEASE IN THE CLINICAL WARDS OF THE ROYAL INFIRMARY, DURING THE SECOND HALF OF THE SUMMER SESSION, 1850.

FROM the middle of June to nearly the end of July there have been an unusual number of cardiac diseases in the clinical wards, all of which we have studied at the bedside with great care. Indeed, most of the diseases to which this organ is liable, as well as their various complications, have been brought under your review,—so that it is scarcely possible to conceive a more instructive series of cases assembled together at one time than you have had the opportunity of examining. Before proceeding, however, to narrate and comment on these cases, allow me to remind you of some of the rules which the laborious researches of many able men have established for your guidance in the diagnosis of cardiac diseases. They are as follows :—

1. In health, the cardiac dulness on percussion measures two inches across, and the extent of dulness beyond this measurement indicates either the increased size of the organ, or the extent of pericardial effusion.

2. In health, the apex of the heart may be felt and seen to strike the chest between the fourth and fifth ribs, immediately below and a little to the outside of the left nipple. Any variations that may exist indicate the altered positions of the apex in disease.

3. A friction murmur indicates pericardial exudation.

4. A bellows murmur with the first sound, heard loudest over the apex, indicates mitral insufficiency.

5. A bellows murmur with the second sound, heard loudest at the base, indicates aortic insufficiency.

6. A murmur with the second sound, loudest at the apex, is very rare, but when present it indicates mitral obstruction, is almost always associated with insufficiency, and the murmur is double.

7. A murmur with the first sound, loudest at the base, and propagated in the direction of the large arteries, is more common. It may depend,—1st, on an altered condition of the blood, as in anæmia; 2d, on dilatation or disease of the aorta itself; and, 3dly, on stricture of the aortic orifice,—in which case it is almost always associated with insufficiency, and the murmur is double.

8. Hypertrophy of the heart may be independent of valvular disease, but this is very rare. In the vast majority of cases it is the left ventricle which is affected, in connection with mitral or aortic disease. In the former case the hypertrophy is equal with rounding of the apex; in the latter there is dilated hypertrophy, with elongation of the apex.

Attention to these rules alone will, in the great majority of cases, enable you to arrive with precision at the nature of the lesion present. In cases in which there may be any doubt, you will derive further assistance from an observation of the concomitant symptoms, such as,—1st, the nature of the pulse at the wrist; 2d, the nature of the pulmonary or cerebral derangements. Thus, as a general rule, but one on which you must not place too much confidence, the pulse is soft or irregular in mitral disease, but hard, jerking, or regular in aortic disease. Again, it has been observed that cerebral symptoms are more common and urgent in aortic disease, and pulmonary symptoms more common and urgent in mitral disease.

I purposely say nothing of diseases of the right side of the heart, and of a few other rare disordered conditions of the organ,—1st, because they occur so seldom as scarcely to merit our attention; and, 2d, because I am convinced that an appreciation of the rules above given is the best method of enabling you to comprehend and easily detect any exceptional cases which may arise. In truth, however, we have seen in our examinations at the bedside that your difficulty is, not how to arrive at correct conclusions from such and such data, but how to arrive at the data themselves. You have to determine,—1st, whether the heart be of its normal size or not; 2d, whether an abnormal murmur does or does not exist; 3d, if it be present, does it accompany the first or second sound of the heart; and 4thly, at what place and in what direction the murmur is heard loudest. These points ascertained, the conclusion flows from the rules previously given. But no instruction on my part, no reading or reflection on yours, will enable you to ascertain these facts for yourself. In short, nothing but percussing the cardiac region with your own hands, and carefully listening to the sounds with your own ears, can be of the slightest service, and the sooner you feel convinced of this truth the sooner are you likely to overcome these preliminary difficulties. Hence why the series of cases we have observed—most of which are now in the ward—are so valuable. You can at once convince yourselves of the accuracy of the facts ascertained to exist by others,—reflect on the probable correctness of the diagnosis formed at the bedside,—observe how, in the fatal cases, by following the rules given, the accuracy of that diagnosis has been confirmed by post-mortem examination,—watch the various complications,—and the effects of the treatment.

To give you confidence in the method of study now recommended, and the

sufficiency of the rules I have given to enable you to arrive at correct conclusions, I shall, in the first place, direct your attention to two cases, in both of which we have seen that the lesions these rules indicated were proved to exist, by a post-mortem examination.

CASE I.—*Dilatation of Ascending Portion of Aortic Arch—Incompetency of Aortic Valves—Dilated Hypertrophy of Left Ventricle.*

History.—William M'Ritchie, æt. 38, fireman on board a Newcastle steamer, entered the clinical ward, complaining of palpitation, dyspnœa, and cough, on the 4th of January 1850. At that time it was ascertained that the cardiac dulness was of unusual extent, and that a blowing murmur existed with the second sound at the base of the heart. He remained in the house under treatment until February 2d, when all the urgent symptoms having left him, he was dismissed. He was re-admitted on the 14th of March, the palpitation, cough, and dyspnœa having returned, together with anasarcous swelling of the abdomen and inferior extremities.

Cardiac Signs.—On percussion, the cardiac dulness measures four inches transversely. The apex beats between the sixth and seventh ribs external to the nipple. The carotid and subclavian arteries beat strongly. A loud and prolonged bellows murmur is heard with the second sound, loudest at the base of the heart, and propagated in the course of the large arteries. First sound is normal in character.

Concomitant Symptoms.—Respiration hurried; cough and dyspnœa urgent; respiratory sounds on inspiration harsh; expiration prolonged; face livid; pain and dizziness in the head; occasionally loss of vision; disturbed sleep; pulse 90, feeble; nausea and anorexia; abdomen considerably swollen from ascites; inferior extremities œdematous; legs cold.

Progress of the Case.—During April the symptoms continued with more or less intermission. In May he became liable to attacks of syncope, accompanied with angina and palpitations. In the beginning of June it was observed that the bellows murmur with the second sound assumed a rougher character over the arch of the aorta. He also complained of dysphagia and a pulsation in his throat, which obliged him to keep his head in a particular position. On the 14th he was seized with an unusually severe attack of angina and syncope, which in ten minutes was fatal.

Treatment.—The treatment consisted principally in the exhibition of a variety of expectorants and antispasmodics, of which a draught containing ten minims of chloroform, and a teaspoonful of Tr. Cardam. c. afforded him most relief. A few leeches were also applied occcasionally to the cardiac region.

Sectio Cadaveris, June 16th.—The pericardium contained three ounces of serous fluid. There was hypertrophy with dilatation of the left ventricle of the heart, in consequence of which the organ weighed 1 lb. 4 oz., and its transverse diameter measured five inches. The mitral valve was healthy. The aortic valves were considerably thickened and curled inwards. Immediately above them the aorta was unusually dilated, the diameter of its caliber being two and a-quarter inches. Water poured upon the aortic valves from above passed through the orifice without apparently receiving any impediment. One inch below the origin of the left subclavian there was an aneurismal pouch, the size of a walnut, projecting half an inch from the general outline of the vessel. The arteria innominata, and the origin of the right carotid artery, were also somewhat dilated; and there was an aneurismal dilatation of the aorta opposite the superior mesenteric artery. The aorta, the coronary, and several of the larger arteries, were roughened internally by atheromatous deposits. The lungs were emphysematous anteriorly, and œdematous at their apices. Brain pale; slight subarachnoid effusion; cerebral arteries slightly atheromatous; other organs healthy.

Case II.—*Incompetency of Aortic Valves—Hypertrophy of Left Ventricle and Auricle—Obstruction and Incompetency of Mitral Valve.*

History.—Samuel Crawford, æt. 42, employed in chemical works, admitted June 10th. He has been subject to palpitation and dyspnœa, after any considerable exertion, for four or five years. Last February he had to leave off work on account of these symptoms, which subsided in a fortnight under medical treatment. Three days ago they once more returned. He has noticed, during the last four or five months, swelling of the feet, legs, and abdomen. He never had rheumatism or any other serious complaint.

Cardiac Signs.—The cardiac dulness measures three inches and a quarter transversely. The apex beats between the sixth and seventh ribs, two inches below and to the left of the nipple. The carotid and subclavian arteries beat strongly. Over the apex a bellows murmur is heard, with both sounds of the heart. Over the base there is a loud prolonged blowing murmur with the second sound, which is propagated in the course of the large vessels. The first sound heard at the base is unusually short and muffled.

Concomitant Symptoms.—The pulse is regular, strong, and jerking. He has cough and considerable dyspnœa. Percussion over the lungs is resonant, but, posteriorly and inferiorly, there are fine moist râles. He is liable to giddiness and a feeling of faintness on sudden exertion. Can only sleep in a half sitting posture, resting somewhat on his left side. Considerable œdema of the lower extremities.

Progress of the Case.—The cough and dyspnœa continued. On the 30th the urine became scanty and high coloured. On the 17th there was diarrhœa. Moist and dry râles were heard over a considerable portion of chest, and there was much cough and expectoration. On the 26th the urine was again abundant, but there was general fever, cough suppressed, and expectoration tinged with blood. Pulse 108, full, and hard. Crepitant and mucous râles were heard over the lower portion of the right side. On the 28th all œdema of the extremities had disappeared, but there was decided pneumonia on right side. Low delirium during the night. Died on the morning of the 29th.

Treatment.—At first ʒx. of blood were drawn from the arm with marked relief, and he took a mixture containing expectorants and diuretics, with tincture of digitalis. Local blood-letting, by means of leeches, was also practised from time to time. The scantiness of the urine and œdema gave way under the use of cream of tartar in ʒj. doses three times a-day. When the pneumonia came on, local blood-letting, and tartrate of antimony internally, were employed, but without success.

Sectio-Cadaveris, July 1st.—The pericardium contained four ounces of straw-coloured serum. The heart weighed twenty-three and a-half ounces. This increase in size was owing to hypertrophy of the walls of the left ventricle and auricle, and to dilatation of the right ventricle. The aortic valves were fringed with numerous warty vegetations. One of the valves was ruptured, and the ruptured edges were studded over with granules of recent exudation. In consequence of these lesions the valves allowed water to rush rapidly through, when poured on them from above. The septal leaf of the mitral valve was perforated in two places by orifices of sufficient size to admit a crow quill. These orifices were surrounded by vegetations, presenting a funnel-shaped prolongation on the internal surface of the valve, through which the orifice passes. There were several other vegetations on the opposite leaf of the valve and fringing its orifice. One of the chordæ tendineæ was broken across at its valvular attachment, the ruptured or floating end being thickly covered with fibrinous vegetations. Aorta healthy. The lower, middle, and a portion of upper lobe of right lung dense, hepatised, presenting a reddish-gray colour, and yielding sanguineous pus on squeezing the cut surface.

Commentary.—Both the cases now detailed exhibit very strongly how the

rules formerly mentioned, correctly applied, enable us to determine the nature of the cardiac lesion present,—for you will remember that, in both, the lesions named at the head of each case were confidently stated to exist, before the body was examined. In case I. "a bellows murmur was heard with the second sound, loudest at the base of the heart, and propagated in the course of the large arteries." Rule 5 tells us that this indicates aortic insufficiency, and on examination such was found to exist. As the case progressed, however, he complained of a pulsation in his throat and of dysphagia; and it is worthy of remark, that not only had an incipient aneurism formed in the arch of the aorta, which explained these symptoms, but that a tendency to the formation of aneurisms existed in other parts of the arterial system. In case II. the diagnosis, though more complicated, and therefore more difficult, was also determined on by paying attention to the same rules. "Over the apex a bellows murmur was heard with both sounds of the heart." Now rule 6 tells us that this indicates mitral obstruction with insufficiency, and a description of the lesion found affecting this valve after death, must convince us that whilst the vegetations prevented proper closure of the orifice, some of them must also have obstructed the flow of blood in its passage from the auricle to the ventricle. But there was also a bellows murmur with the second sound, heard loudest at the base; and this, as in case I., is a sign of aortic insufficiency. A careful determination of the cardiac signs, therefore, and an exact appreciation of the facts in the first instance, led us, in accordance with the laws previously generalised, to a correct conclusion as to the nature of this complicated case.

CASE III.—*Incompetency of Mitral Valve.*

History.—Agnes Murray, æt. 41, admitted June 16th. About eighteen months ago she first experienced, without any obvious cause, palpitations and pains in the cardiac region, which have continued ever since. They became more violent after exertion, and were accompanied with dyspnœa. Latterly there has been an œdematous swelling of the legs, abdomen, and face. She has had four attacks of hæmoptysis, the first occurring eighteen months, and the last three months, ago.

Cardiac Signs.—The cardiac dulness measures two and a-quarter inches across. The apex of the heart beats under the sixth rib, below and a little outside the nipple. Over the apex there is heard a harsh bellows murmur, which diminishes in intensity towards the base and large vessels.

Concomitant Symptoms.—Pulse 80, weak. Great dyspnœa and palpitations on exertion, and occasional severe pain in the cardiac region. Resonance of lungs natural. Posteriorly, over right lung, loud sibilant râles are heard, both with inspiration and expiration. Expectoration abundant. No anasarca at present, or cerebral symptoms.

Progress of the Case.—This woman, under the action of small doses of digitalis, and cream of tartar, and the occasional application of a few leeches to the cardiac region, became gradually much better. The palpitations, dyspnœa, and bronchitis disappeared. She was dismissed greatly relieved July 15th.

CASE IV.—*Incompetency of Mitral Valve—Pulmonary Apoplexy—Hydro-thorax.*

History.—Robert Ross, æt. 30, a lath-splitter, admitted June 28th. For some time past has occasionally experienced palpitation, and observed now and then slight swelling of the legs. He first became severely ill only seven weeks ago, when he was seized with repeated vomitings, which continued two days. He subsequently caught cold, to which he is very liable, and since then has been labouring under cough, dyspnœa, a feeling of tightness across the upper part of the abdomen, and general weakness.

Cardiac Signs.—Cardiac dulness cannot be distinctly defined. The apex beats feebly between the fifth and sixth ribs, two inches below the nipple. A bellows murmur is heard with the first sound over the apex, but much more

distinctly three inches to the right of it, near the sternum. It is almost inaudible over the base. The second sound is normal.

Concomitant Symptoms.—Pulse 100, small and soft. Considerable dyspnœa and cough—sputa viscid and tinged with blood. No dulness on percussion over the lungs. Sibilant, mucous, and crepitating râles are heard very generally over the inferior parts of chest, both anteriorly and posteriorly. No increase of vocal resonance. The general surface is slightly jaundiced. On careful percussion of the liver, its inferior border presents a prominence anteriorly the size of an egg, over the pylorus.

Progress of the Case.—Up to the 3d of July he experienced occasional vomiting. The inferior extremities became œdematous, and fluid accumulated in the abdomen. On percussion, the resonance over the right lung as high as the scapula is diminished ; there is slight increase of the vocal resonance. On the 8th of July, the surface generally was anasarcous. From the 10th to the 15th, the dyspnœa greatly increased. He expectorated on various occasions mouthfuls of florid blood. Latterly, could only lie on the left side. The left side of the chest became completely dull on percussion, with absence of respiration. He was now removed from the infirmary by his friends.

Treatment.—Leeches to the epigastrium, with naptha and anodynes internally, checked the vomiting. The principal object of the treatment, however, was by means of diuretics, to increase the amount of urine, and thereby diminish the anasarca. Pills of leads and opium were also administered to check the hæmoptysis.

Commentary.—The two last cases contrast very strongly with the two first. In both the bellows murmur was heard only with the first sound, loud over the apex, diminishing towards the base ; and Rule 4, tells us that this indicates mitral incompetency. The concomitant symptoms fully bear out this diagnosis. The pulse was weak,—the pulmonary organs were those disturbed, while the cerebral functions were unaffected. In case III., there was bronchitis, which diminished under appropriate treatment. In case IV., bronchitis also existed, but it was much more general, and mingled with a certain degree of pneumonia on the right side. Extravasation of blood into the pulmonary tissue of both lungs had most probably also taken place, as indicated by the hæmoptysis; and, latterly, the general dropsy which prevailed affected the thoracic cavities, causing hydrothorax on the left side. The man was evidently in a dying condition when his friends insisted on his removal ; and I was rather surprised to hear that he lingered a fortnight before death took place. No examination could be obtained.

Case V.—*Mitral Incompetency—Hypertrophy of Left Ventricle—Attack of Acute Rheumatism, followed by Aortic Incompetency.*

History.—John Conolly, æt. 49, a joiner, admitted June 22d, 1850. He has for some years past been subject to pain in, and swelling of, the joints. Eighteen months ago he was suddenly seized with pain in the cardiac region, unaccompanied by dyspnœa, but followed by severe cough. He has been copiously bled, and undergone a lengthened treatment.

Cardiac Signs.—The cardiac dulness measures $2\frac{3}{4}$ inches across. The apex beats in a hollow between the xyphoid cartilage and the cartilage of the seventh left rib. Heart's impulse strong. A bellows murmur can be heard with the first sound, synchronous with the cardiac impulse. It is loudest at the apex, and diminishes in intensity towards the base.

Concomitant Symptoms.—Pulse 74, full and strong. No cough, but considerable dyspnœa on making the slightest exertion. Percussion and auscultation indicate slight pulmonary emphysema anteriorly, but no bronchitis. Slight tinnitus-aurium, and dimness of vision occasionally. There is a patch of *psoriasis figurata*, an inch and a-half in diameter, on the right cheek and side of the nose.

Progress of the Case.—July 1st, he was attacked with severe articular rheu-

matism in the hip, knee, and wrist joints, which had entirely disappeared under appropriate treatment on the 9th. On the 14th he had diarrhœa, accompanied by considerable discharge of blood per anum. This continued in smaller quantities from time to time. On the 22d, a careful examination exhibited a change in the cardiac signs. The impulse over the apex was more prolonged, with a deep murmur and jog. The bellows murmur synchronous with the impulse was no longer audible, but one can be heard alternating with it at the base,—that is, with the second sound. Great pulsation of the carotid, subclavian, and humeral arteries was seen and felt, and a loud puffing murmur, synchronous with their dilatation, could be heard over them. His general health, however, was greatly improved, the local and other symptoms having disappeared; and he left the house, at his own desire, July 24th.

Treatment.—At first he took digitalis for six days, with a view of diminishing the cardiac impulse and pain. It was then suspended on account of the nausea and weakness it apparently occasioned. The rheumatic fever and arthritis were combated by salines, diaphoretics, and venesection to the extent of ʒxij. Afterwards the local pains rapidly yielded to small blisters placed over each affected joint. The diarrhœa and discharge of blood were checked by pills of lead and opium.

Commentary.—This man, after frequent attacks of rheumatism, entered the Infirmary, labouring under hypertrophy, with incompetency of the mitral valve. At the time there was no bronchitis, but he had previously suffered from severe cough and pulmonary derangement. Whilst in the house one of his acute rheumatic attacks came on. Many of the joints were swollen and exceedingly painful; but this affection yielded to one general bleeding, tartar emetic internally, and blisters locally, in eight days. The effect of this attack was to give rise to acute endocarditis, which, instead of affecting the auriculo-ventricular orifice formerly diseased, fixed itself upon the aortic valves. The lesion, however, must have been slight—probably limited to a few small vegetations upon the margins of the valve—because the murmur was soft in character, and the incompetency not of such amount as to occasion either cerebral or other functional symptoms. The pulsation in the large vessels, however, was greatly augmented, and there is every reason to fear, that should the incompetency continue (as is most probable) the aorta and cavity of the left ventricle will both become dilated.

CASE VI.—*Mitral Incompetency—Hypertrophy of Left Ventricle—Aortic Incompetency and Obstruction.*

History.—Edward Monro, æt. 41, a painter, admitted June 24, 1850. Two years ago, without any assignable cause, he was suddenly seized with angina, consisting of severe pain in the middle of the sternum, often running down the left arm, accompanied by violent palpitations. Since then the paroxysms have been increasing both in frequency and intensity.

Cardiac Signs.—The cardiac dulness below the nipple measures 3¼ inches transversely. The apex of the heart cannot be felt to beat at any particular spot. Heart's action is regular. A distinct bellows murmur can be heard accompanying both the first and second cardiac sounds, which are equally loud at the apex and at the base. Both are heard loudest to the right of sternum, opposite the second, third, and fourth costal cartilages. A loud blowing murmur is heard over the carotid arteries.

Concomitant Symptoms.—Pulse 74, regular. Has a slight cough, with expectoration. Lungs resonant on percussion, and on auscultation the inspiratory murmurs are louder and rougher than natural, and the expiration is slightly prolonged. He has frequently expectorated small quantities of dark-coloured blood. There is great dyspnœa on making the slightest exertion, and he has occasional severe attacks of angina. There is considerable dyspepsia. Slight dimness of vision, and muscæ volitantes, but otherwise no cerebral symptoms.

Progress of the Case.—The attacks of angina returned four and five times

a-day. They occasioned great agony, profuse perspiration, and increased action of the heart, during which the murmurs were heard louder. There was also occasional nausea and tendency to vomit. On the 8th of July he fainted, being unconscious for five minutes. At this time the murmur with the first sound assumed a whining character, heard loudest at the apex. There was a double bellows murmur heard distinct from this, at the base. July 11.—There was cough and expectoration. A fine moist râle could be heard over the lower half of left chest, both anteriorly and posteriorly. No dulness on percussion, or increased vocal resonance. July 15.—He has now only one attack of angina in the day, which is also much less severe. The cough and expectoration are diminished. A mucous râle still perceptible in left lung inferiorly. A whining murmur with the first sound is still heard at the apex, and a double bellows murmur at the base, propagated in the course of the great vessels. He left the house at his own desire.

Treatment.—The attacks of angina were at first treated with anodyne and antispasmodic draughts, containing M. v. of chloroform for a dose. Afterwards they were greatly relieved by taking carminatives, such as three drops each of the oils of aniseed and cajiput dropped on sugar. Latterly they greatly diminished after ʒvj. of blood were drawn from the cardiac region by cupping. The bronchitis was treated with anodynes and expectorants.

Commentary.—When this man entered the Infirmary it was very difficult to determine at what point the two bellows murmurs were heard loudest. Repeated and careful examination failed to discover whether one or both were referable to the apex or to the base; and in consequence we could not, according to the rules given, determine whether the disease was aortic, mitral, or both. This was probably owing to the circumstance of the abnormal murmurs originating in two places, and being, at the same time, so similar in tone, that the diffusion of sound was pretty equal over the whole cardiac region. But as the case progressed the murmurs underwent such modifications as left us in no doubt. The murmur with the first sound over the apex assumed a whining tone, so that it was easily separated from the double bellows murmur which still remained loud at the base. The former, according to the rules given, must have depended on mitral incompetency; whilst the latter, for the same reason, must have been owing both to incompetency and obstruction of the aortic orifice. The man laboured under slight pulmonary, as well as cerebral, symptoms. His chief complaint, however, was the angina, the attacks of which were in him very severe, causing the most excruciating agony, and bathing the whole surface with sweat. This, in its turn, seemed to be connected with a state of dyspepsia which existed. Whenever gas accumulated in the stomach, so as to distend that organ and press the heart upwards, the attacks were most severe. The carminatives gave relief by causing discharge of this gas. After local bleeding, and an improvement in his general health, but more especially in the dyspeptic symptoms, the angina diminished in intensity.

The two last cases recorded exhibit how important it is carefully to examine the cardiac signs from time to time as the case progresses, and to watch the modifications they undergo. Where doubt and difficulty prevail, it is only in this way they can be removed. Under such circumstances, never state an opinion at all, and continue to watch until the signs become permanent and unequivocal. This advice you will find to be even more useful in private than in hospital practice, for reasons which I shall allude to hereafter. But not only are frequent examinations useful in clearing up different points in diagnosis, but they reveal to the pathologist the changes which take place in the affected parts. Of this the following case affords us an instructive example.

CASE VII.—*Incompetency of the Aortic Valves—Dilated Hypertrophy of Left Ventricle—Pneumonia—Pulmonary Apoplexy.*

History.—William Caird, æt. 29, labourer, admitted May 30. Five months ago he first noticed that he became unusually breathless, and had palpitations

after exertion. He continued to work until two months ago, when, being engaged in lifting heavy stones, he was suddenly seized with pain in the cardiac region, violent cough, and hæmoptysis. He entered the Glasgow Infirmary, from which he was discharged, much relieved, in a fortnight. Since then he has been subject to giddiness, dyspnœa, and palpitation, with occasional hæmoptysis.

Cardiac Signs.—Cardiac dulness extends $3\frac{3}{4}$ inches transversely. The apex beats between the sixth and seventh ribs, three inches below, and a little to the left of the nipple. A bellows murmur is heard with the second sound, loudest at the base, and propagated in the course of the large vessels. The first sound is normal.

Concomitant Symptoms.—Pulse 92, strong and regular. He feels a shooting pain in the cardiac region, extending to the epigastrium. There is great dyspnœa, and palpitation on exertion. Slight cough, and fine moist râle in the chest, heard inferiorly and posteriorly. Occasional giddiness.

Progress of the Case.—The pain in the cardiac region and epigastrium was the chief source of complaint during the progress of the case. The dyspnœa and palpitations were from time to time distressing. There was occasional vomiting. On the 12th of July, it was observed that the bellows murmur assumed a whining character, and on the 15th it was distinctly musical, like the chirping of a small bird. On the 17th, the heart's action was tumultuous, and vomiting was very distressing. On the 23d there was considerable hæmoptysis, mouthfuls of blood being evacuated. On the 24th, there was dulness on percussion over the inferior portion of chest, and distinct crepitation, with increased vocal resonance, could be heard. The cardiac dulness was determined, on careful percussion, to measure five inches transversely. The vomiting and hæmoptysis defied all remedies. The pulse was 100, soft. He gradually became weaker. The urine was scanty, and œdema of the legs appeared. Latterly there was muttering delirium at night. Died on the 29th.

Treatment.—At first he experienced relief, from the cardiac and epigastric pains, after small local bleedings by means of leeches and cupping. Blisters were also applied. All kinds of remedies were tried to check the vomiting, but with little effect. Antispasmodics were employed to relieve the dyspnœa; and latterly, as the pulse became weak, wine and stimulants were freely administered.

Sectio-Cadaveris, July 30th.—Heart much enlarged, weighing 25 ounces, owing almost entirely to hypertrophy with dilatation of the left ventricle. When water was poured upon the aortic valves from above it passed rapidly through the orifice. The aortic valves were thickened throughout and shortened. The curled-in and dense margins were one-tenth of an inch thick. Two of the valves were united at their neighbouring surfaces, so as to form one, the only vestige of a septum between them being a hardened nodule at the base of the enlarged valve. On the edge of the smaller valve was a wart, soft in consistence, composed of recent exudation, and infiltrated with blood, so as to present a purple colour. There was red hepatisation of the posterior and inferior portion of both lungs, and there was considerable apoplectic extravasation in the substance and the neighbourhood of the diseased portions of the lung. The bronchi were filled with frothy mucus. The liver presented the nutmeg appearance, being in the first stage of cirrhosis. Other organs healthy.

Commentary.—We had very little difficulty in determining, from the cardiac signs in this case, that, according to the rules laid down, there was incompetency of the aortic valves, with dilated hypertrophy of the left ventricle. The bellows murmur, which was at first soft, gradually changed its character as the case progressed, without altering its position. It became whining, and then chirping, constituting what is called a musical murmur. It is generally found in such cases that a solid body projects into the current of the blood as it flows through the valve, so as to be thrown into vibrations; and it was interesting

to discover, on the examination of the body, that the vegetation described exactly fulfilled these conditions. From its softness also there is every reason to suppose it was of recent formation, originating probably about the time the musical murmur was first observed. From the great induration of the aortic valves, there can be very little doubt that they had been affected for a long time, at least many months ; but it becomes a question, whether the adhesion and formation of one valve out of two might not have been caused by a rupture of one or both valves, two months previously, at the time he was lifting heavy stones, and was suddenly seized with cardiac pain and other symptoms. It is worthy of observation, also, that, although he had cerebral symptoms, the lungs were greatly affected, the bronchitis latterly passing into pneumonia and pulmonary apoplexy.

CASE VIII.—*Mitral Incompetency—Hypertrophy of left Ventricle—Dilatation and Disease of Arch of Aorta—Aortic Incompetency.*

History.—Hugh Devine, æt. 40, labourer, admitted July 17. Dates his illness from a severe strain of the back, eighteen months ago, but is not sure when he first noticed dyspnœa and palpitation, which have prevented him from working for the last eight months. Never had rheumatism or hæmoptysis.

Cardiac Signs.—Cardiac dulness measures $2\frac{3}{4}$ inches transversely. The apex beats between the fifth and sixth ribs, two inches below, and a little to the right of, the nipple. A bellows murmur with the first sound is heard at the apex, decreasing towards the base. A bellows murmur of a rougher character is also heard with the first sound at the base, which is prolonged in the course of the large vessels. The second sound is normal. There is distinct pulsation under the clavicles, but none above the sternum.

Concomitant Symptoms.—Pulse 104, regular, full, and jerking. No cough or pulmonary symptoms, with the exception of dyspnœa on exertion. Has frequent pain in the upper part of the head and across the temples, and occasional dimness of vision. The thyroid gland is somewhat enlarged.

Progress of the Case.—Since his residence in the infirmary the symptoms are greatly ameliorated. The dyspnœa, palpitation, and cephalalgia, have nearly disappeared. The cardiac signs, however, have undergone considerable change. On the 16th of August it is reported that there is still a bellows murmur with the first sound, heard loud at the apex. An inch above, and to the inside of the nipple, a loud, harsh, grating murmur is heard with the first sound, and followed by a soft bellows murmur with the second. In the course of the aorta there is unusual impulse, and coinciding with it there is a bellows murmur, which is propagated along the carotids.

Commentary.—You will remember that we examined this man with great care, and found cardiac signs which are not often associated together. For instance, a distinct bellows murmur, loud over the apex and diminishing towards the base, which, according to the rules given, we ascribed to mitral incompetency. Over the aortic valves, however, and extending along the arch of the aorta, there was also a bellows murmur of a rougher character, and also occurring with the first sound. Now Rule 7 tells us that this may depend on three circumstances,—"1st, On an altered condition of the blood, as in anæmia : 2d, On dilatation or disease of the aorta itself ; and, 3d, On stricture of the aortic orifice, in which case it is almost always associated with insufficiency, and the murmur is double." It is clear that the first and third propositions would not apply, and I therefore came to the conclusion that in addition to mitral regurgitation, the aorta was dilated and diseased, the former indicated by the increased impulse and the latter by the roughened murmur.

[Since this was stated to the class, the roughened murmur over the aorta has assumed a rasping character, and a soft bellows murmur may also be heard with the second sound, so that very probably the dilated and diseased aorta is now associated with aortic incompetency.]

CASE IX.—*Incompetency of Aortic Valves—Hypertrophy of Left Ventricle—Aneurism of the Arch of the Aorta.*

History.—Charles Watt, æt. 31, groom, admitted June 19. During the last eight months has frequently had occasion to lift heavy weights, and has occasionally felt slight pain in the epigastrium. This suddenly became very violent on the 8th of June; and the next day, on walking, he experienced violent dyspnœa. On the 11th he was cupped, with considerable relief. Has been aware of a pulsation in the neck for two years, but never suffered any inconvenience from it. No dysphagia.

Cardiac and Aortic Signs.—The cardiac dulness extends three inches transversely. The apex beats with great force between the fifth and sixth ribs, two inches below, and a little to the left of, the nipple. A bellows murmur is heard with the second sound, loudest at the base. The first sound is normal. In the right side of neck, immediately above the sternum and clavicle, there is a pulsating tumour the size of a hen's egg, extending laterally two inches. It communicates a strong impulse, and a peculiar thrill, to the hand placed on it, and over it there may be heard a loud hoarse bellows murmur, synchronous with the impulse of the heart, and this murmur may be heard at the back, extending down the course of the aorta.

Concomitant Symptoms.—Pulse 74, regular, hard, and jerking, alternating with the impulse at the apex, stronger in the right than the left wrist. Pain in the epigastrium, and dyspnœa on exertion. No other pulmonary symptoms. Frequent pain in the left temple, extending down that side of the nose. Giddiness on rising suddenly. Frequent muscæ volitantes.

Progress of the Case.—Continued to have pain in the epigastrium, and dyspnœa at intervals. He was treated by occasional small topical bleedings and blisters, and latterly small doses of aconite. The physical signs underwent no change, but the distressing concomitant symptoms nearly disappeared, and he felt so well that he was dismissed, at his own desire, July 15.

Commentary.—In this case aortic incompetency was proved to exist by the same sign as we have seen to accompany it in the other cases. The visible swelling, diffuse pulsation, and bellows murmur, synchronous with the dilatation of the vessel, could leave little doubt that an aneurism of the aorta existed. It became a question, however, whether the innominata was or was not involved; and I am inclined to consider not, from a variety of circumstances, but more especially—1st, because the pulse at the right wrist was stronger than at the left; 2d, because the pain in the head and face was on the left, and not on the right side; and, 3d, because the bellows murmur over the tumour was superficial, anterior, and propagated down the back in the course of the aorta.

CASE X.—*Pericarditis and Endocarditis—Hydropericardium.*

History.—Barney Kilpatrick, æt. 25, a miner, admitted July 8th. Nine weeks ago he was suddenly seized with dyspnœa and a feeling of weight or dull pain in the cardiac region. A fortnight since, this pain became much more acute, and has continued up to the time of admission. For five years he has been much exposed to wet and changes of temperature, but never had rheumatism.

Cardiac Signs.—Cardiac dulness measures three and a-quarter inches transversely, and is limited above by the margin of the third rib. Apex beats between the fifth and sixth ribs, two inches below, and considerably to the right of, the nipple. All over the dull region a double friction sound is heard, resembling a roughened bellows murmur, but superficial. Beyond the region of the dulness these murmurs suddenly cease. Action of the heart regular.

Concomitant Symptoms.—Pulse 96, regular, small, and feeble, stronger on the right than on the left side. The slightest movement induces pain, extending from the cardiac region down the left arm to the fingers; great dyspnœa; no

cough or other pulmonary symptoms; no fever; no cerebral symptoms or tendency to syncope.

Treatment, and Progress of the Case.—Twelve leeches were ordered to be applied to the cardiac region, and a calomel and opium pill to be taken every six hours. On the 11th, the friction murmurs were much louder at the base than at the apex. The pulse 108; feeble on left side; on the right side it had a double impulse, a pretty strong beat being followed by a weaker one. ℥vj. of blood to be drawn from the cardiac region by cupping, and a pill to be taken every four hours. On the 13th the breath had a mercurial fetor. Pulse stronger; less dyspnœa; friction murmurs more faint; pain in arm diminished. On the 14th, pulse full; slight fever; six leeches to be applied to the cardiac region; morphia draught at night. On the 15th, friction murmur only heard at the base; anorexia; can take no food; omit calomel and opium pills. 16th, Friction murmurs have disappeared, but there is a soft bellows murmur with the second sound, heard at the base. 18th.—Had an attack of severe dyspnœa and syncope; pulse 100, regular, but feeble; ℥iv. of wine; cardiac dulness increased. 19th.—Orthopnœa; pulse weak and fluttering; a quivering pulsation felt in the cardiac region; faintness; œdema of feet and legs. Stimulants to be freely administered. Died early in the morning of the 30th.

Examination of the body was not allowed.

Commentary.—This was a well characterised case of pericarditis. At first the endocardial murmur was masked by the friction sounds, but as these disappeared, its existence became apparent. It was observed, that as the mercury affected the system, the friction murmur diminished; but there is every reason to believe that this was not so much owing to absorption of the exudation, as to increase in the amount of serous effusion. To the combined effects of pressure on the heart from liquid without, and incapability of performing its function from incompetency of the aortic valve, the fatal event must be attributed.

In the short commentary I have appended to each case, I have dwelt principally on points of diagnosis, which are those that present the chief difficulties to you in the study of these diseases. I must guard you, however, from the idea, that the group of cases we have reviewed, gives a fair notion of cardiac disorders as you will meet with them in private practice. No doubt their study will enable you, by the way of exclusion, to determine those functional derangements of the organ which are more frequently met with among individuals moving in the better ranks of life. To these, as well as to a variety of important considerations connected with the etiology and treatment of heart disease, I must direct your attention on some future occasion.

LECTURES ON GENERAL THERAPEUTICS.

By Douglas Maclagan, M.D., F.R.S.E.

(*Continued from page* 66.)

Lecture V.—chemical remedies—cauteries—caustics.

Chemical Agents.—These produce their effects by virtue of their possessing and exerting certain chemical powers. This is the nature of their action at the commencement, and it is thus far merely an instance of ordinary chemical agency brought to bear directly or indirectly on the body for the cure of its diseases. But though the action here is at first purely chemical, as that of the mechanical remedies was purely physical, the vital properties of the body sooner or later become involved, and through them the therapeutical effects are produced. A cauterant, for instance, acts at first in a purely chemical manner, but there soon follow inflammation, ulceration, and suppuration, and to these vital actions many of its more important effects are due.

You will see that in the table of classification I have divided chemical agents into three sets: those acting on the tissues of the body; those acting on its fluids; and those altering or destroying external morbific agents. I have a few general remarks to make on these before considering them separately.

With respect to the last of these sets, which includes disinfectants and antidotes, it may be remarked, that their action from first to last is purely chemical, and does not necessarily implicate any vital process whatever. It is different, however, with the others; for vital phenomena are more or less involved by all those chemical agents, which act on the tissues or fluids of the body.

With respect to the living textures, you must bear in mind this general fact, that no agent can affect them chemically except at the expense of their vitality. They either must have virtually lost their vitality previously, or, in being acted upon chemically, must surrender it. We do not need to enter into any discussion as to the nature of vitality to understand this. The phrase vital affinities, is, perhaps, for our purpose, the most convenient to express those relations of the particles of the body one to another, which tend to keep them in the living condition; and it is essential, in order to enable any agent to affect the living tissues chemically, that it be capable of overcoming these vital affinities. The part acted on chemically then becomes dead, and must be separated or thrown off from the body as a matter foreign to it.

With regard to chemical agents acting on the fluids of the body, it may be asked, are they, so long as they are retained within the body, and, as it were, form part of it, capable of being directly altered by chemical agency, or are not the chemical alterations which they undergo only secondary consequences of vital actions of the remedies on the living textures to which these fluids belong? There can be no hesitation in replying to this question, that some of the fluids of the body, viz., the secretions, can be directly affected chemically. The secretions of the stomach, for instance, may be preternaturally acid, and by introducing an alkali we can neutralize this, just as we would do in a laboratory experiment, and so relieve the symptoms to which its presence gave rise. Every dyspeptic who has suffered from heartburn knows this practically, and flies for temporary relief to his bottle of carbonate of soda. The urine, too, may be preternaturally acid, or it may present characters which indicate that it has lost its due proportion of acidity, and in the one case by the use of alkalies, and in the other by means of acids, it may be brought to a more natural state. I have no doubt that, in such cases, alkalies act directly chemically on the urine; but it is doubted, and with good reason, by many persons, if the action of acids on the urine is of this direct character. I shall, however, reserve my remarks on this till acids and alkalies come more directly under consideration. In the meantime we may admit, that some of the secretions at least may be acted on chemically in the body.

There is one fluid, however, the most important of all, the alteration of which chemically, requires a little more consideration. I allude to the blood. With regard to the question, as to whether the blood can undergo in the living body direct chemical action by remedies, we may remark the following circumstances:—

First, That many substances possessing considerable power as chemical agents may mingle with the blood without producing on it any appreciable effect. Thus alcohol, and many metallic salts, are absorbed into the circulation, and do not leave any traces of having produced those chemical effects which we know they can exert on blood out of the body. This appears, however, to be due only to their being in small proportion, and probably to their passing slowly into the blood; for when injected directly into the veins in quantity, they coagulate the blood and kill the animal. You must not, therefore, suppose, because a substance which has a known chemical action out of the body, has mingled with it in the circulation, and has produced some palpable effects on the body, that it has done so by chemical action on the blood. The blood in such circumstances appears merely to act as the vehicle by which

the medicinal substance is transported to those portions of the solids on which it acts.

Secondly, However, we know that there are certain forms of disease in which the blood is altered in its properties. In some of these, as in inflammation, disease of the solid textures precedes the change that occurs in the blood, and appears to be the cause which produces it; and in such cases, when our treatment has brought back the blood to a natural condition, it is obviously by an indirect effect—the disease of the solids being that towards which our efforts are directed, and on its removal the blood gradually returns to a normal state. In a case of pleurisy or pneumonia, for instance, if we draw blood we find it to have undergone a remarkable change in respect of the proportion of fibrinous matter which it contains. We bleed our patient, and give him antimony; the disease subsides, and the blood is now found to be no longer buffy or sizy, as it is called; but it is self-evident that this is not an instance of remedies acting chemically on the blood—the change in this fluid is only a consequence of the vital action of our remedies on the solids. In other diseases, however, such as purpura, scorbutus, and chlorosis, the diseased state of the blood appears to precede the affections of the solid organs, which, sooner or later, manifest themselves—the latter being here a consequence, not the cause, of the morbid state of the blood; and here our therapeutic efforts are directed towards the blood itself, and the *ratio medendi* is to bring back the blood to a healthy state, and thus to restore the healthy action of the solids. Now, if there be remedies capable of doing so, they must be allowed to be capable of producing a chemical change on this fluid; but it is questionable if we are entitled to assume, that they act upon the blood in accordance with our ordinary ideas as to the operation of a chemical agent. It may at least be said, that this is a point by no means established, although there are reasonable grounds for admitting that it may be true.

Purpura, scorbutus, and chlorosis, have been quoted as examples of diseases in which it is believed that the primary morbid state is an alteration in the blood, and that the disorders of the solid organs which manifest themselves are consequences of this. Now, the mineral acids have generally been esteemed as of virtue in purpura—certain vegetable acids, along with proper dietetical management, have a reputation in scorbutus,—and iron is universally admitted to be a valuable remedy for chlorosis. But is there any evidence that in these cases the remedies act by chemical agency on the blood? If we had any proof by analysis that there was an excess of alkaline matter in the blood in purpura and scurvy, we might congratulate ourselves on having got some satisfactory information, both as to the pathology of these diseases, and the therapeutical action of the remedies. But, unfortunately, this is not the case; for, although alkalies are said, by continued use, to produce a morbid state, or, as it is termed, a dyscrasia, of the blood, similar to what is supposed to exist in purpura and scurvy; and although such a theory, of the latter disease at least, has actually been propounded, and is in itself by no means an unfeasible hypothesis, yet that most important element in the matter, the fact of a superabundance of alkali in the blood, is awanting—nay, one of the latest writers on the chemical pathology of scurvy, Dr Garrod, has been led to believe, though confessedly on limited data, that there exists a too small proportion of one alkali, potash, in the blood in scurvy. We are, therefore, not entitled in the least degree to assume, that these acids, when of use in these diseases, exert a direct chemical action on the blood, as there is no proof of the existence of that pathological state of the blood which, as acids, they could chemically rectify. On the other hand, the utility of the mineral acids in purpura may, in the present state of our knowledge, be more satisfactorily explained, on the supposition that they act, as it is well known that they do, as tonics and astringents upon the solids, and that the improvement in the state of the blood which follows is a secondary consequence of the vital action; for there is nothing unphilosophical in the supposition, that though the morbid state may commence in the fluids, the

therapeutic action of the cure may commence in the solids. The doctrine, that these acids act as tonics, receives confirmation from the fact, that in the very cases to which mineral acids are applicable, other tonics, such as quinine, are found to be equally efficacious. Here, then, is an instance of what may be termed a chemical disease of the blood, which appears to be beneficially treated by a class of remedies of decided chemical powers, and yet, which, when inquired into, does not afford any satisfactory evidence that these remedies have proved curative through chemical action. In the present state of our pathological knowledge, therefore, we must hesitate, I think, to accept as established, the proposition so broadly laid down by some writers, that "some substances exercise a chemical action on the blood; as the mineral acids, the alkalies, various metallic salts, alcohol," &c. I do not say that this is not the case; but I think we are fairly entitled to say, that, with respect to mineral acids, at least, it is not proved, either by the example of purpura, which I have alluded to, or by any other instance with which I am acquainted.

In the case of chlorosis, we find the peculiarity of the blood to be a deficiency of the red globules (Andral), which, as you know, besides the red colouring animal matter, hæmatine, also contain normally a certain proportion of iron. It is this want of the colouring portion of the blood which is believed to give to chlorotic subjects that peculiar pale complexion which has procured for this morbid condition the appellation of chlorosis, or green sickness. Now it is a fact, particularly worthy of remark, in relation to our present subject, that preparations of iron are pre-eminently useful in this state of the constitution. For a long while it was supposed, that the value of this medicine consisted in its being emmenagogue, because chlorosis occurs almost exclusively in females, and is generally accompanied by more or less of disorder of the uterine functions, and this was supposed to be the cause of the chlorosis, and the iron was believed to act on the body generally, by improving the condition of the uterine system. But we now know very well that chlorosis, which is not in itself a distinct disease, but rather is a peculiar diathesis or state of the constitution, is not dependent upon uterine disorder; or rather, I should say, we know that the irregularity of menstruation, like the palpitation and other symptoms which so commonly accompany it, is an effect, not a cause, of the chlorotic state, and, therefore, some other explanation than its being emmenagogue must be sought for the action of preparations of iron, or chalybeates, as they are generally termed. Now, one very obvious theory of the action of chalybeates is, that they act vitally as tonics,—a power which they are known to possess in many diseases besides chlorosis. But if they act merely as tonics, why should not other remedies of the same class, such as quinine, be found to be equally or even more efficacious? We can, I think, ascribe it to no other cause than that, there being a deficiency of that portion of the blood which contains iron normally, some of the metal thus introduced into the blood remains in it, and furnishes the means of forming a better supply of well constituted blood corpuscles. That this is the prevailing idea among practical men of the present day, appears, not only from the universal preference by them of iron to other tonics in chlorosis, but from a little practical direction which is commonly given in such cases, and which, I think, is a good one, viz., that the iron should be taken, not as is the case with most other medicines, on an empty stomach, but some little time after a meal—the object being, that the iron shall, by absorption, enter the blood along with the products of digestion, and thus be more readily capable of being assimilated along with them. I think, however, we must hesitate to say that this function of iron, its entering as a constituent into the impoverished blood, is all the part that it plays in chlorosis. In a deficiency of red globules, there is a change in the blood, not merely of a chemical, but, as we may say, of an anatomical character; and if we found in chlorotic blood that there were globules present in due quantity, but that there was something amiss with them which we could trace to the want of iron, or if the globules consisted chiefly of iron, instead of containing

a mere fraction per cent., we might then suppose that in curing chlorosis we did so merely by pouring iron into the blood. But I think we must admit that in this disordered state of the system there is a want of power in the digestive organs to assimilate and convert into blood the organic matters taken as food ; that part of the action of the iron is that of a tonic to the digestive organs and nervous system, but that partly, also, its action is chemical,—that when thus introduced into the system, its presence, perhaps by what chemists call a predisposing affinity, enables the organic matters of the food more readily to be converted into that anatomical element of the blood, the globules, which is known to be deficient.

I think, however, you will perceive in this action of iron, that, even if it be true, as I have stated it, there is a certain distinction to be drawn between our notion of its chemical effect, and that of an agent introduced into the system coming in contact with the blood, and exercising a chemical reaction upon it. It stands more in the position of a dietetical than a medicinal treatment, and is analogous to the cure of scurvy, and other diseases, by a well regulated diet. It is rather the supplying from without of a chemical element which the digestive system requires to have at its command, and to be able to associate with other materials, and form from it good blood; and it, therefore, does not, after all, entitle us to infer, analogically, that other remedies, which do not enter normally into the constitution of our blood or tissues—such, for instance, as nitric acid—affect the vital fluid by a chemical action. In the present state of our knowledge, I think we must be content to say, that it is probable, but not proved, that the blood may be acted on chemically ; that in those diseases in which this all-important fluid undergoes a change under the administration of remedies, the effects are in great part secondary consequences of vital actions on the living solids ; and that the only strictly chemical effect which appears to be in any respect established is that which is due to our supplying some deficient element of the blood, either of an organic or inorganic nature.

You will, I dare say, be inclined to think, that these digressions into purpura and chlorosis have led us rather far away from our subject; but you must bear in mind, that they were cited as illustrations of the general question,—can the blood be chemically acted on in the living body ? The reply, perhaps, is not satisfactory, but it will, at least, show you that the subject is interesting theoretically, is even now of some interest practically, and may become highly so, if chemical pathology will only lend us a helping hand. There is hardly, in the whole range of therapeutics, a better subject for experimental investigation than the action of remedies on the blood. He would confer a benefit on the theory of medicine who should determine, for example, what effects, if any, acids and alkalies really do produce on this fluid, either in causing disease or aiding its cure.[1]

We now proceed to consider chemical remedies under the three divisions mentioned above.

Chemical Agents acting on the Solids : Cauteries—Caustics.

By the term actual cautery we generally understand a red-hot iron, but, along with it, we may include intense heat, applied in any other form, as the moxa, which is a roll of some combustible tissue, such as German tinder, or lint which has been impregnated with nitre, set on fire, and allowed to burn in contact with the body.

By the term potential cautery, or caustic, we mean a substance which has a chemical affinity for some of the constituents of the tissues, and which acts

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[1] Since the delivery of this lecture, the Harveian Society of Edinburgh have proposed, as the subject of their prize essay for 1851, an experimental inquiry into the mode of action of iron, comprising, especially, the question of its accumulation in the blood.

upon them chemically when applied to them. The effect of both is essentially the same—that of disorganising the part to which they are applied; and both the terms, cautery and caustic, have a common origin (καιω, *olim* καυω, I burn,) —as if the one, as well as the other, acted according to the popular expression by burning. The term cauterant is generally held to include both, and therefore it is quite a correct expression to talk of cauterising with caustic as well as with the hot iron.

Cauteries.—When a cautery is applied to the skin, its first effect is to cause the part acted on to assume a gray or brown shrivelled and dried-up appearance. The appearance indicates what is actually the case. The part has really been dried up, and has, in consequence, thus become shrivelled and gray. Water, as you know, enters largely into the constitution of all the living tissues of the body; the true skin contains about 60 per cent., muscle about 80 per cent., and cellular tissue probably a still larger quantity. The presence of this water is essential to their vitality; any agent which, like the cautery, can expel the water from the tissues, kills them,—and this, in fact, appears to be the nature of the primary or chemical action of a cautery. It does not need to extend beyond this, in order to destroy the vitality of the part; but the smell which is generally evolved on its application shows that some of the animal matter of the tissues is really burned. To produce its due effects, the cautery iron must be heated to at least a dull red,—if it is less hot, it will only cause inflammatory, not chemical, action; and, although this degree of heat is quite enough for insurance of its action, there is no harm in its being more intensely heated. Provided the parts which are not intended to be touched by it are protected from the radiation of its heat—which can always be easily done by covering them with wet cloths—the hotter it is the better; it will not only do its office more effectually, but is absolutely less painful to the patient,—the more rapidly the vitality of the part is destroyed, the less is the suffering. It is on this account, I believe, that the moxa has been so much superseded by the hot iron,—the slow torture of the former making it a much more formidable infliction, whilst it does not, either in point of efficacy, or in the facility with which its action can be regulated, equal the other. The pain of the cautery, though severe at the moment, is not protracted; and the sense of burning, when it is applied very hot, is not always very distinctly felt. I have heard young subjects, when cauterised, and who did not know what was being done to them, cry out that we were cutting them, and remain in perfect ignorance that they had been burned at all.

I need hardly say that the idea of being seared with a hot iron is somewhat of a trial even to the strongest nerves, and that you ought to take all precautions to make it as little terrible to the patient as possible, especially if, as is often the case, there is a possibility of its being applied more than once. The simplest way of disarming it of its terrors is to put the patient under chloroform, and it is one of the many advantages which we owe to this inestimable agent, that we can, in almost every instance in which the cautery is needed, employ this rather formidable, but valuable remedy, deliberately and extensively, without the patient suffering the sharp pain of its application. If circumstances occur to prevent you using the anæsthetic, and they can occur but seldom, unless, as happened to myself last year in the Infirmary, the patient's prejudice against chloroform leads him to prefer to bear the pain, you may still, by a little management, lessen the terrors of the hot iron. Do not, for instance, put it into the fire to heat it directly in his sight, and do not, as I have seen in a continental hospital, have an attendant deliberately blowing the fire with a pair of bellows close to his ear. No surgeon with proper feeling displays his instruments before the patient on whom he is going to operate. It is an advantage if the cautery can be heated conveniently, and without unnecessary parade, in the same room with the patient, but if it has to be heated in a neighbouring apartment, it ought to be carried to the bed-side, covered with a deep jug, or other convenient vessel, to prevent it from cooling too much

in the transit, and everything ought to be ready for its instant application, when it is brought to the operator.

It is not my business to enlarge upon the different forms of cauteries, or upon the extent to which it should be applied. These must vary according to the purpose for which it is to be used. As soon as it has been applied, let the part be covered with lint, dipped in cold water. This will so far relieve the pain, that the patient, if he has been chloroformed, will experience comparatively little pain on recovering his sensibility. In one or two hours, however, the seat of the burn will become more painful, inflammation in the surrounding skin having now fairly set in, and there is always some degree of pain felt till the slough separates, which takes place by the ordinary process of ulceration, and is attended with copious discharge of pus. The cold water application may be renewed frequently, for the first three or four hours, and then a poultice, or warm water dressing, applied till the burn heals.

When, instead of the entire skin, the cautery is applied to a raw surface, we have at first a coagulum formed of the blood or other fluids; in other respects the effect is the same.

Caustics.—These produce the same ultimate effect as the cautery, *i. e.*, they destroy the vitality of the part, but in a somewhat different way. Some of them have been supposed to act like the cautery, by merely withdrawing the constituent water from the tissues, and no doubt the majority of our more powerful caustics, such as fused potash, chloride of zinc, and the concentrated mineral acids, are substances which have a powerful affinity for water; but they do more than merely withdraw water from the tissues, they enter into combination with their fibrinous, albuminous, and gelatinous constituents, either coagulating or dissolving them, and forming compounds with them. I have never been able to make out that distinction which some authors indicate between different kinds of caustics. I believe that all the substances deserving the name of caustics really act by entering into combination with the materials of the textures on which they act. When a caustic has produced its true effect of destroying the vitality of a portion of tissue, this must, as in the case of the cautery, be separated from the body in the form of a slough or eschar (εσχαρα, a deep ulcer); hence they are termed escharotics. The separation of the slough and healing of the ulcer, whether from caustic or cautery, are accompanied by the usual suppurative discharge, and eventually a firm contracted cicatrix is formed.

To enter into a consideration of the peculiar properties of various caustics is foreign to my present purpose. They will fall to be described individually hereafter. The following several points with regard to them, however, may be noticed:—1st. Their chemical power must be sufficient, else they will cease to be caustics, and, like an insufficiently heated cautery, will only excite inflammation, and act as irritants, not as chemical agents. Diluted nitric acid, for instance, is not a caustic, whilst the concentrated acid is powerfully so. 2dly. Their properties should be such that they can be applied readily as extensively as is desired, but that they should not spread to parts which it is important not to injure. Thus, in applying a caustic to a sore affected with bad action, into all the inequalities of which we could not be sure of penetrating with a solid substance, a liquid caustic, such as concentrated nitric acid, is the best; whilst in cauterising the fauces or genitals, where the spreading of the caustic is to be avoided, a substance like the nitrate of silver, which has no tendency to spread, is preferable.

When, for the sake of its greater power, we employ such a caustic as fused potash, or chloride of zinc, which have the disadvantage of being deliquescent, we must have recourse to some expedient for preventing its spreading. The chloride of zinc may be made into a paste with flour, or the potash may be combined with lime, as in the Vienna paste, and its deliquescence is thus lessened, though certainly at the expense of its activity. If we are applying one of these caustics to the skin, the part to be cauterised should have applied

to it a piece of sticking plaster, which had better be spread on leather than on linen, having a hole cut in it a little smaller than the size of the slough intended to be formed; the caustic is applied to the bit of skin thus exposed, whilst the plaster prevents it from spreading to the surrounding skin. This protection, however, cannot be made use of in such practices as applying caustic potash to the cervix uteri, or nitric acid to hæmorrhoids. In these cases, as soon as the caustic has been fairly applied to the part, and has exerted its chemical influence, which it does immediately, we wash the part out with something which will chemically neutralise the overplus of the caustic, weak acetic acid being used in the case of the potash, and a solution of carbonate of soda in the case of nitric acid.

The entire skin is not very easily acted on by caustics. The epidermis so far monopolises the chemical action of many of them, that they will not act chemically on the true skin, but many of them—such as fused potash and chloride of zinc—will destroy the whole thickness even of unbroken integument. The mucous membranes are more easily acted upon than parts covered with epidermis; and on raw surfaces, such as ulcers, even the least powerful caustics readily exert a certain amount of destructive action. Thus nitrate of silver, applied to the whole skin, will not cauterise it, but will act as an irritant, and form a blister; on a mucous membrane it will act more powerfully, and will even slightly cauterise it; but when applied to an open sore, it will always produce distinct destruction of the surface; and sulphate of copper, which, on a soft granulating surface, acts to a certain extent as a caustic, is not more than an irritant to a mucous membrane, and on entire skin produces no effect at all. We must therefore be guided by our knowledge of the comparative potency of different caustics in selecting them, according as we wish to cauterise more or less deeply, or as we have to act on entire skin, mucous membrane, or raw surface. From the facility with which raw surfaces, and especially the sensitive cutis and the soft texture of granulations, are acted on by local applications, many substances have been called caustics which in reality do not deserve the name. There are some substances which will so violently inflame these textures as to lead to the destruction of a portion of the surface, but which do so by powerful vital, and not by chemical agency. These are irritants, not caustics, and they in general cause a much greater degree, or at least a greater duration, of pain than those substances which at once chemically destroy the part which they touch. I believe that powdered arsenic, acetic acid, creosote, subacetate, and perhaps also sulphate of copper, which are commonly described as caustics, act really as irritants, and independently of chemical action, except so far as most of them coagulate the albuminous fluids which cover all raw surfaces.

If you bear in mind what was said regarding the allopathic action of remedies, you will readily comprehend that when so powerful an action has taken place as the destruction of a portion of tissue, with the concomitant inflammation and its consequences, the effect will not be limited to the part primarily acted on, but will also extend its influence by a vital action to neighbouring parts. Hence you will be prepared to find that cauteries and caustics are capable of exciting a counter-irritant action, and that they are in fact extensively employed for the production of this physiological effect. In the action of these agents, then, we trace the following series of events: 1st, destruction of tissue; 2d, ulceration, and suppurative discharge; 3d, formation of a firm cicatrix; and 4th, counter-irritant action on other parts. Keeping these in view, then, let us see what are their practical uses and applications, and in so doing we shall for the sake of brevity consider cauteries and caustics together.

MILITARY SURGERY.

EXTRACT FROM A CONCLUDING LECTURE DELIVERED TO THE STUDENTS OF MILITARY SURGERY IN THE UNIVERSITY OF EDINBURGH, AT THE CLOSE OF THE SUMMER SESSION 1850. BY SIR G. BALLINGALL.

This suggests to me a few words in reference to our position here, as compared with that of the professors and students in the medical schools attached to the military hospitals in France.

You will recollect that, in my introductory lecture, I referred to these, and similar institutions on the Continent, as noble examples of a patriotic attention to the health of the soldier. But, even while I was speaking, the decree for the suppression of the "Hôpitaux Militaires d'Instruction" in France, had gone forth from the President of the Republic, on the suggestion of the Minister of War.

This is a measure which has been noticed with indignation by some of the medical journals of the Parisian school. The alleged motives for its adoption —expense, insubordination, and inferior attainments of the pupils—have been rigidly questioned; it is said that the first is trivial, in comparison to the object in view; that the second does not exist; and that the third remains to be proved. While professing republican principles, one of the journals—"L'Union Médicale"—exclaims, "Les institutions médicales n'auront pas à bénir les institutions 'républicaines.'" This decree is looked upon as a prelude to the evils and discouragements which other scientific establishments have to expect from the change of government,—"Le premier coup de marteau est donné dans l'edifice médical de l'armée; les coups suivants ne tarderons sans doute pas." The minister is alleged to have forgotten the duty which the state owes to the sick soldier and to his sorrowing family. These schools, it appears to me, must be reorganised,—the pay and position of the army surgeon must be improved,—or the army will soon have cause to complain; and perhaps to petition—as the seamen of our fleet did, in the mutiny at the Nore — for "better attendance when sick, and when wounded in action."

But it is not for me, gentlemen, to remedy the error which the French war minister is said to have committed. My object in adverting to this matter is, to point out the difference between the schools which he has thus summarily suppressed, and the solitary professorship which I have the honour to hold. These schools were exclusively appropriated to the maintenance and education of the medical cadets for the French army, whose scanty pay does not enable or encourage them, as in this country, to educate themselves. The professorships in these institutions were looked forward to by the surgeons of the French army as the only prizes which they could obtain for services however distinguished, —while the young men educating in them had the advantage of being constantly employed in the treatment of sick soldiers, under the tuition of men experienced in the duties of the field, the garrison, and the military hospital. There the *élèves* were instructed in all the usual branches of professional education, elementary as well as practical; but there was not in any one of these schools, so far as I know, a professorship devoted to the peculiar duties of this chair. Many years have now elapsed since the late Baron Larrey spoke to me upon this subject, with deep feeling, and with deep disappointment. You will see it referred to in the preface to his "Clinique Chirurgicale;"—and he regretted the want of such a chair as a "lacune" in the French metropolis,— a chair which he was so competent to fill, and which was at one time promised to him by the minister of the day. The reasons for supporting the "Hôpitaux d'Instruction," *in so far as they bear on the peculiar duties of the army and navy surgeon*, apply to this country with tenfold force; while the principal reasons assigned for their suppression,—expense and insubordination—

a void in the treasury, and an *emeute* in the school,—have no application here.

[Since this lecture was delivered, the Val-de-Grâce has been reopened as a school specially devoted to the instruction of doctors in medicine who are about to serve as surgeons in the French army, and who must now pursue their studies for a year in the new establishment. The different departments of study proposed are, 1, Clinical Medicine ; 2, Clinical Surgery ; 3, Operations and Use of Bandages, &c. ; 4, Hygiene, Military Legal Medicine, and Laws applicable to the service ; 5, Manipulations of Toxicology, and of Chemistry applied to Hygiene. These different departments are to be presided over by five professors, chosen, in the first instance, by the Minister of War, from a leet of fifteen, presented by the Military Council of Health, and selected from the professors of the former hospitals of final instruction. Future vacancies to be filled up by "concours."—EDITOR.]

Part Fourth.

PERISCOPE.

MEDICINE.

ON THE BEST MODES OF TREATMENT OF PSORIASIS AND LEPRA-VULGARIS. BY M. EMERY.

The following is a sort of statistical balance sheet drawn up by M. Emery as the result of his experience in the St Louis Hospital of Paris :—

He states that, after having failed to obtain any good amount of success from following systematic plans of treatment,—such as the purgatives of Hamilton, the blood-lettings of Duffin, Wallace, and Graves, and the lunar caustic applications of Alibert,—he adopted the plan of Biett, who had introduced the treatment by arsenical preparations, tincture of cantharides, and frictions with iodide of sulphur. Here are the results of these methods in his hands :—

"I have treated with different preparations of arsenic 140 cases of psoriasis, or lepra vulgaris, and have kept notes of them. Of these only 38 have gone out apparently cured, after two, four, six, eight, and even fifteen months of treatment. Within six months, 6 of these (3 women and 2 men) had returned to my wards. Within eighteen months, I had re-admitted 22. I have never again seen the other 16. Of the 102 other cases, 40 went out covered with darkish spots, having scales of psoriasis alongside of them, or even upon them, but in small quantity. Before the end of a year, I saw 24 of these again. The shortest treatment had been three months, and in 18 of them (11 men and 7 women), the duration had not been less than a whole year. The remaining 62 had so often suffered from various accidental ailments, produced by the remedies, that after eight or nine months of ineffectual treatment, I submitted 40 of them to tar ointment frictions for two or three months, when they went out cured. On 12 of them I had tried tincture of cantharides, but only cured one solitary case,—the other 11 went out of hospital without much improvement, after remaining in a year. Lastly, of the remaining 10 I treated 2 with ointment of napthaline (1 part to 15 of axunge). These were cured in six weeks. I treated 8 with proto-iodide of mercury ointment (1 part to 16) ; an ounce of this being used daily, which, in 4, produced sharp itching, and in 2 swelling of the gums, with salivation, after a month of treatment. They seemed radically cured in two months and a-half." In all these cases the various forms of treatment were accompanied by

simple or sulphureous baths, or with the vapour bath or vapour douche. The preparation of arsenic which he found to answer best was Fowler's solution, in doses of from 5 to 18 drops. The arsenical plan is not without a certain amount of danger, and sometimes it has to be suspended six or eight times in the course of the treatment; but M. Emery has no hesitation in proclaiming it to be the best of all the internal remedies.

He then refers to external remedies, and after noticing a great many which had failed in his hands, he mentions his having himself introduced, in 1836, tar ointment in large proportion (a fourth or a third of a part of tar to two or three parts of axunge), against which no graver objections have been offered by the many practitioners who have adopted it, than its soiling the linen and being palpable to the senses. It cures more rapidly, and as surely as any other remedy. Even in the largest quantities it does not injure the health, and only produces occasionally a few superficial pustules which heal rapidly. He had treated by this means from fifteen to eighteen hundred cases, had cured more than five-sixths of them, and never saw any unpleasant consequences attend its use. He at first employed along with it calomel and sulphuric acid internally, and sulphureous baths externally; but discontinued them, not only because they appeared to be useless, but because they seemed even to retard the cure. He subsequently contented himself with a few simple baths at long intervals. He thinks patients treated in this way not so liable to relapses as those treated by internal remedies alone, and especially by arsenic. Upon a suggestion of M. Cazenave, however, he tried a combination of the external treatment by tar and the internal administration of arsenic, and the following are the results :— "I have treated by this method 228 cases of psoriasis, and cured 200 in the average space of six weeks to two months. I have had very few effects of the nature of poisoning, having seldom excceded 10 drops of Fowler's solution daily. I have observed that by this treatment the disease underwent at the same time two curative processes : the scales of psoriasis are by the tar effaced from the circumference to the centre, and by the arsenic they diminish in thickness and assume a blackish gray tint. By combining these methods of treatment, these two phenomena manifest themselves at the same time."

In the cases which resisted this treatment, amounting to about a seventh of the number, M. Emery tried various mercurial preparations, but found only one to succeed,—that of M. Boivet, which is one part of proto-iodide of mercury to 8 of axunge; but it has the disadvantage, when applied over large surfaces, of frequently causing salivation. Another ointment which is also of some value is that of iodide of sulphur, containing from 1 part in 32 to 1 in 8 of lard. Psoriasis of the scalp yields to this, but it is apt to cause erysipelas when applied over large surfaces, and especially when used of the strength of 1 to 8. M. Emery's conclusions, then, are,—"That arsenical preparations, and especially Fowler's solution, are the best internal remedies ; that tar takes the first place as an external application; that the combination of these is the best treatment for psoriasis; that the iodide of mercury ointment is occasionally useful when judiciously applied, and next to it the iodide of sulphur."

He also tried, on the faith of an article in the "Gazette des Hôpitaux," about three years ago, baths containing 5 drachms of bichloride of mercury, which were said to cure psoriasis in 8 days.

These baths were tried first in 22 cases (14 males and 8 females), subsequently upon 20 others, 16 of whom were under 14 years of age. None were cured, while several suffered severely from the remedy. M. Gibert had also tried these baths in 15 cases, and failed in all of them ; so that the inutility of this remedy appears to be settled.

M. Emery adds, that too great reliance must not be placed upon the certainty of the cures, as relapses are numerous,—the same causes which produced the disease being apt to cause its recurrence. Dietetical and hygienic measures must also be adopted ; the diet mild and not too substantial ; all aliments difficult of digestion to be avoided, especially salted meats, the richer kinds of fish,

and fried articles. The patients should take regular exercises, use baths frequently, and have recourse again to the treatment which had benefited them, on the least appearance of the disease, without waiting till it has developed itself extensively.

The following is his method of applying these remedies:—"Before commencing the frictions I order a bath, and make slight frictions with the ointment the moment the patient comes out of it. This is repeated three times daily, and at the end of two or three days I increase the quantity of the ointment and the activity of the frictions. After six or seven days the patients have the ointment constantly upon them; and when the disease is of long standing I cover the larger patches of eruption with compresses spread with the tar ointment to the depth of a line. The patients take a tepid bath once or twice a-week. It is rarely necessary to suspend this treatment. There are, however, subjects with very irritable skins, in whom it produces occasionally a few impetiginous pustules or some small boils; but very often they continue their treatment in spite of these. In about ten days there is observed in the patches of psoriasis, from which the scales have separated, a whitish circle around them, which gradually extends from the circumference to the centre. This is the first indication of the decrease of the complaint, which commonly disappears in the space of two or three months, without the patient experiencing any change in his general health. In the lepra vulgaris the centre commences to break up. The rings, which form the circular chain of eruption, separate, and then follow the same course as the psoriasis.

The Fowler's solution should be given cautiously, beginning with five drops in four ounces of liquid, which is divided into two portions. An additional drop is given each day; and if no unpleasant consequences follow, the quantity may thus be rapidly raised to twelve drops daily. If it is observed that the patches of eruption become thinner, and assume a blackish-gray colour, the dose is not increased further for fear of accident, as the above appearances indicate saturation of the system. When, on the contrary, the patient bears the remedy well, and there is no appearance of improvement, the Fowler's solution may be augmented in quantity to fifteen or sixteen drops, which it is rarely necessary to exceed."

"The skin sometimes becomes the seat of rather sharp pain, or, without being absolutely painful, becomes hot and slightly red around the patches; a few tepid baths, diluent drinks, and the diminution of the dose of arsenic, suffice to remove these slight symptoms. There are some delicate stomachs which cannot support the arsenic even in the smallest doses; and when that is fairly established we must have recourse to some other treatment. Sometimes, after continuing the above treatment for fourteen or fifteen days, there occur sense of constriction in the throat, and rather severe pain in the stomach. The whole treatment must then be suspended. These symptoms disappear in two or three days. The arsenic may then be resumed, recommencing with five drops in the day, and increasing the dose by one drop every second day. The pains in the region of the heart, which render all its pulsations painful, sometimes make it necessary to have recourse to blood-letting. The symptom demanding most attention is, undoubtedly, contraction of the extensors of the limbs. So soon as this shows itself the arsenic must be abandoned, or we run the risk of seeing this increase and become rapidly incurable."

"When the patients have been able to bear full doses of the remedy, and when the patches of eruption have given place to spots of a blackish-gray colour, the use of the arsenic may be discontinued, and the treatment limited to baths and vapour douches. The saturation is then complete, and the arsenic would not then act any farther on the system at large. These spots, which announce the approaching cure, may not disappear till after some months of treatment."—*Bulletin de Thérapeutique in the Revue Méd.-Chirurgicale.*

CLINICAL REMARKS ON ORGANIC DISEASE OF THE BRAIN. BY PROFESSOR ROMBERG OF BERLIN.

Case.—A woman, of 48 years of age, presented the following symptoms :—Headach, chiefly frontal, existing for more than a year, excited by motion, and by expiratory efforts, increasing in intensity after sleep ; sparks seen before the eyes ; noise in the ears ; sometimes faintings without convulsion ; likewise nausea and vomiting ; convergent strabismus of the right eye for the last two months.

Professor Romberg remarked :—In all obstinate cases of headach, it is the physician's first duty to ascertain whether he has to deal with a neuralgic affection of the integuments, or with a disease seated in the brain itself. In this case, the continuous character of the pain is opposed to the idea of the case being one of neuralgia. The strabismus, too, due to paralysis of the right adductor muscle of the eye, opposes this idea. In cerebral headach, the train of symptoms which co-exist is of great importance in establishing a correct diagnosis. It is in connection with cerebral headach that we meet with strabismus, partial anæsthesia, weakness, or contraction of single fingers. This form of headach also becomes more intense during expiratory acts, by which the cerebrum is urged against the vault of the cranium, the cerebellum against the tentorium ; the increase of pressure causing increase of pain, dizziness, sparks before the eyes, &c. These symptoms are aggravated by every movement of the head. Darkness causes relief through the suspension of consciousness. The influence of light upon nervous diseases is worthy of remark ; nervous asthma, in particular, is alleviated when light is admitted into the chambers of the sick. In the recumbent posture there is aggravation ; in the erect posture alleviation of cerebral headach, dependent upon the alternate increase and diminution of the supply of blood to the brain. The influence of different positions of the head is also of some use in assisting diagnosis. By compressing the carotids, we have it in our power to operate upon the brain, but in the case under observation this manipulation produces no particular result. The sickness observed when the patient is in the erect posture, is a symptom indicative of cerebral disease. The pulse is regular, although irregularity and intermissions in the action of the heart are often observed in lesions of the base of the brain, in the neighbourhood of the vagus nerve ; and *e. g.*, in the meningitis of children, these symptoms are even more important than retardation of the pulse. The faintings are an important symptom ; but their character must be studied in order to ascertain if they are of a hysterical or epileptiform nature, in which case they would materially influence the diagnosis and prognosis.

In determining the precise nature of this case, it must be recollected that tubercle and cancer are the most common causes of severe cerebral headach. The first of these causes we may here exclude,—*firstly*, because in adults it is decidedly rare ; *secondly*, because we are unable to ascertain the existence of any other tubercular affection. We may also exclude softening of the brain, for in cases of that description when headach has lasted so long, sensation and motion of the limbs, and the power of articulation, usually begin to be impaired. The disease is probably situated at the base of the brain, and it may consist either of a tumour or of a meningeal exudation. The chief ground for this opinion is the paralytic strabismus of the right eye,—a condition indicating compression of the right nervus abducens.

As to the treatment, there should be no detraction of blood, general or even local. Revulsion to the integuments of the scalp is a better and more trustworthy practice, and will be affected by rubbing successive portions of the scalp with antimonial ointment. We can with this practice combine the use of drastic purgatives, to excite the action of the torpid bowels.

CASES OF PARAPLEGIA. BY PROFESSOR ROMBERG.

CASE I.—A man, aged 52, has been for three or four months suffering from formication and pain in the lower extremities. The formication extends from the upper part of the thighs to the soles of the feet. At night there are startings of the legs, like electric shocks; the power of moving both limbs is weakened,—the left is the weaker. The soles of the feet are benumbed, and feel as if covered with hide. There is vertigo. Last summer the patient had retention of urine, and his present symptoms immediately succeeded the cure of that affection.

These symptoms undoubtedly show that the origin of the disease is central. Its very commencement with loss of sensation and of motor power in the lower limbs, leads to the suspicion that disease has been developing itself in the spinal cord. That the left leg is chiefly affected accords with the successive invasion of symptoms frequently observed in spinal affections. The vertigo draws our attention to the state of the brain. As to the former retention of urine, it is to be remarked that the present symptoms rapidly succeeded its cure. Now, there is a class of paralytic affections which depend upon diseases of internal organs, especially of the kidneys—more rarely of the urinary bladder—which must be regarded as reflex lesions, and may give rise to paraplegia. In such cases, the palsy gradually vanishes when the disease on which it depended is cured; it is possible, then, that our patient's present paraplegia was connected with the ischuria. The whole train of symptoms, moreover, points to the development of a spinal affection,—the tickling in the extremities, gradually increasing weakness, and sensation of abdominal constriction, often for years precede the true paraplegia, and are often disregarded as symptoms of rheumatism or of hemorrhoids.

The species morbi it is, however, difficult to decide upon in the present instance. The case is not one of tabes dorsalis, for the characteristic symptom of that affection is wanting,—viz., the staggering when the eyes are shut,—at least our patient's gait is not more unsteady when the eyes are shut than when they are open.

For the removal of the hyperæmia in the spinal canal, which is often present at least in the early stage of such affections, we should employ cupping along the course of the spine, and repeat the operation after a few weeks' interval. Derivation to the intestinal canal by means of active cathartics will also be proper.

CASE II.—An upholsterer, 47 years of age, who in March was working for a long time upon a floor where there was a strong draught, and had returned home with his limbs half benumbed, three days afterwards complained of weariness, coldness, and sense of weight in the legs. When the cold season of the year came on, he became obviously worse; the mobility of the legs being so much impaired that he could not extend the toes and feet at all; while flexion of the knee-joint was difficult, and progression without support impossible. No difference in this last respect whether the eyes were open or shut. Pricking with a needle caused little sensation of pain in the legs; the abdominal muscles were weakened; expulsive efforts at stool made with difficulty; inability to retain the fæces; erections without sexual emotion; urine containing lithic salts; habitual perspirations of the feet, to which he had been subject, were not now observed; the upper extremities unaffected; the general health, in other respects, excellent.

This case, which is likewise one of paraplegia, shows that the influence of the will is simultaneously withdrawn from the parts affected,—especially from the muscles of the lower extremities. This in itself is opposed to the diagnosis of tabes dorsalis,—a disease in which the conducting power of the spinal cord is not removed, and in which, consequently, voluntary motion is permitted. Besides the influence of impeded vision upon the power of motion is here null. The cause of this paraplegia, then, is situated in the spinal mar-

row, or in the spinal canal, and at least as high up as the middle dorsal vertebra,—for the loss of sensation in the integuments extends as high as the umbilicus. It cannot be a rheumatic affection, for there is paralysis of the sphincter ani, a symptom always of *central* origin. The case seems to have originated in the suppression of the habitual perspiration of the feet—a very frequent source both of paraplegia and of other diseases. Although in this, as in similar cases, it is difficult to determine the species morbi, it seems at least certain that we have not to do with a mere compressing tumour ; for the contractions which from time to time are observed in the parts affected indicate irritation of the spinal cord itself. The seat of disease must be in the anterior columns of the medulla spinalis, for there are no symptoms of that deranged sensation proper to affections of the posterior columns.

The paralysis of the abdominal muscles is also worthy of attention ; they are relaxed, and permit the distension of the intestines with gas. To obtain satisfactory proof of this condition, the patient should be desired to strain as if at stool. It will then be found that the contraction of the abdominal muscles is no longer practicable, or at least is not energetic ; and the same difficulty is experienced during the acts of coughing, sneezing, shouting, and even of speaking. This difficulty indicates the extension of disease to the upper part of the spinal cord, where the nerves which supply the abdominal muscles originate. The urine is loaded with lithic salts, although commonly in spinal diseases it is alkaline, even at the commencement of the affection, and before the urine begins to stagnate in the bladder.

Treatment.—Romberg recommends the Russian vapour bath, and cold douche to the back, succeeded by friction, and attempts to induce perspiration. The cold water cure in such cases was pernicious.—*Schmidt's Jahrbucher*, 1850, No. 4, p. 30.

INOCULATION WITH SECONDARY SYPHILIS. BY M. VIDAL.

M. Vidal lately made the following communication to the Parisian Societé de Chirurgie :—

" The pustular ecthyma, termed a secondary symptom of syphilis, may be transmitted by means of experimental inoculation, from the sick to the sick, and from the sick to the sound. The individual who voluntarily and courageously submitted himself to the experiment was in perfect health, and had never been affected with primary syphilis. The matter inoculated was taken from one of a crop of pustules on the person of a man who had suffered from indurated chancre, but who was at the time of the operation free from primary symptoms. This and other successful experiments induce me to consider the transmissibility of secondary syphilitic symptoms as perfectly established."—*L'Union Médicale.*

M. Vidal proposes to submit to the society a formal memoir on this subject, which will no doubt provoke a lively debate.

NIGHTMARE CAUSED BY EXCESSIVE PROLONGATION OF THE UVULA, AND CURED BY EXCISION OF THE PART. BY M. WEST, OF BELGIUM.

Bronckaert, a fusileer of the 7th infantry, who had been a soldier since January 1849, of feeble aspect, came to M. West, in the course of last March, to request twenty-four hours' exemption from duty, to enable him to recover from the fatigue and fright which he suffered during the night from the apparition of a monster, which threw itself suddenly upon him, so as nearly to smother him, and against which, he said, he had struggled for a long time before making his escape. This strange story led the author at once to suppose that the man had had nightmare. After quieting his mind upon the subject of the monster, which so much alarmed him, the doctor gave him twenty-four hours' exemption, at the same telling him not to come back to him upon the subject of his dreams. He thought he would not see the man again, but next day Bronckaert came back to say, that he had passed a night as horrible

as the preceding one. M. West tried to explain to the man the absurdity of his terror; advised him to take little food in the evening, to sleep on his right side, with the head and shoulders a little raised, thinking that these means would suffice to relieve him.

Bronckaert's torments, however, not having ceased, he came again to the doctor, after having followed his advice for three weeks, and told him that he now saw no hope for relief from these attacks, which had begun more than a year before his enlistment, and under which he expected soon to perish. Having remarked during the interview an obvious imperfection in the man's respiration, Dr West looked into his mouth to examine the cause, and saw, to his astonishment, that the uvula was about two inches long and four lines broad, although the man had never referred any of his complaints to this quarter. Thinking that the long uvula was the cause of the suffocation and fright which the man suffered during his sleep, Dr W. instantly cut off from the uvula an inch and three-quarters of its length. The portion excised, being of unusual length, was preserved in spirits, and, after the usual shrinking from the alcohol, still measures an inch and half.

Dr West saw the man next morning. He was in an ecstasy of joy. The monster had not re-appeared, and he had passed a good night. That the uvula getting into the glottis was the cause of the man's sufferings, appeared both from the complete relief which he thus obtained, and from his subsequently becoming strong and fat.—*Archives Belges de Méd. Militaire, in Revue Médico-Chirurgicale*, July 1850.

CHLOROFORM IN A CASE OF POISONING BY STRYCHNINE. BY A. W. MUNSON, OF KENTON, OHIO.

Mr G———, aged about forty, of intemperate habits, took, from among my medicines, on the 5th inst., a bottle of strychnine, and supposing it to be morphine, as he said, swallowed a dose supposed to be about one or two grains. In about twenty minutes afterwards I was requested to see him, as he was supposed to be in a "fit." I found him in the following condition:—The whole muscular system rigid; the muscles of the back, and of the upper and lower extremities, rigidly contracted; the head drawn back; articulation difficult; sense of tightness about the chest; perspiration flowing profusely from the face and chest. A number of the physicians of the place came to his assistance. The usual remedies recommended in such cases were resorted to, but without any mitigation of the urgent symptoms. The patient was failing rapidly under the increasing spasmodic action of the whole muscular system. It was now determined to administer chloroform, as death was apparently certain without some relief. One drachm of chloroform was put upon a silk handkerchief, and the patient directed to inhale it. The effect was decisive. The patient (who was at this time in a sitting posture, held so by assistants, who could not move him in the least degree without exciting the most frightful and alarming spasms) requested to be placed in a recumbent position, which was done without exciting the least spasm. The chloroform was carefully administered for some hours, the patient holding the handkerchief most of the time himself, in order, as he said, "to keep off the dreadful spasms." From this time he recovered rapidly, and on the 7th inst. was able to leave for home, a distance of six or seven miles.

In the "Western Lancet and Hospital Reporter" for February 1850, published at Cincinnati, Ohio, there is a similar case reported, which is the only one that I know of in which chloroform has been given in case of poisoning by strychnine. Is it not possible that chloroform is the remedy in such cases?—*Boston Med. and Surg. Journal*, 26th June 1850.

SURGERY.

EXCISION OF THE KNEE JOINT.

In the "Lancet" of 27th July, a student complains of having been unable to witness an operation at King's College Hospital, on the 20th of that month, from the area of the theatre being occupied by *three and twenty* surgeons. It now appears from the "Mirror of London Surgery," that the object which had attracted this great congregation of doctors, was to witness an excision of the knee joint. We regret the occasion of such an assemblage, in the first place, on account of the patient, who, we believe, thereby lost a life which might probably have been saved; secondly, on account of the practitioners, whose time, we think, might have been more profitably spent; and thirdly, on account of the science of surgery, which it seems to us here took a step in the wrong direction.

Although the recorded cases in which the knee joint has been cut out are not numerous, they are quite sufficient to determine the inexpediency of this operation. The great duty of a surgeon is to save life; and to this object all other considerations, however important, must be held secondary. But the facts already collected show, beyond any question, that cutting out the knee joint is attended with more risk of life than amputation of the limb. The "Mirror of Surgery" alludes to two cases in which Mr Syme, twenty years ago, tried excision of the knee joint. Both of the patients were children, and would probably have both recovered if treated in the ordinary way. But one of them died; and the other retained a limb which could not be considered superior to a wooden one, since it did not grow with the general growth of the frame, so that when the boy became a man there was a great disparity of length between the sound extremity and that which had been the subject of operation. Mr Syme, we believe, has performed excision of the elbow joint in nearly eighty cases, but has never felt warranted to repeat the knee joint experiment; and we think it would have been better if Mr Fergusson had taken advantage of the experience acquired from this and other sources, instead of setting up a new beacon of warning against the operation by performing it on this "strong-made man, aged twenty-one, of a ruddy complexion, a labourer in the country, generally enjoying good health," who "sunk on the ninth day after the operation," and in whose body, "on a post-mortem examination, all the viscera were found perfectly healthy."

We have no doubt that these remarks will be characterised as "personal" by those writers with whom this expression is equivalent to the fair criticism of published facts, if it should involve any difference of opinion. But our readers may be assured that we shall not be deterred from following the path of duty to the profession by any such silly and ill-natured misrepresentations. Like the hero of the nursery story, we will stuff our ears, so that the revilings of unsubstantial voices may hiss around us without harm, and steadily advance towards the attainment of our object, which is the establishment and diffusion of sound principles.

PUNCTURE OF THE BLADDER. BY MR GAY.

In commenting on a case of retention of urine, where puncture of the bladder was performed, Mr Gay stated, "that, notwithstanding the objections which have been brought against the operation of puncturing through the rectum, he had not met, in his experience, any unfavourable results from this practice. He had operated on persons of different ages, and on one person, in particular, under the most unpromising circumstances, and nevertheless not a single bad symptom had ever followed. Mr Gay believed that such had likewise been the fact in the practice of other surgeons. He therefore unhesitatingly recommended this method as being vastly superior to incisions into the perinæum.

Should the stricture be of such an obstinate character that the relief which was obtained in the preceding case does not immediately follow, and the bladder again become distended, Mr Gay would prefer, especially if there were the slightest prospect of effecting a passage by the use of bougies and other treatment, puncturing the bladder again and again to cutting down on the stricture; and he is convinced that there are very few strictures, in the condition alluded to, which do not admit of cure by a well-directed course of treatment and use of instruments. The dangers attending the other modes of relieving the bladder are very great, and the fatality frightful. Such operations on individuals whose constitutions have been impaired by irregularities and excesses of all kinds (and these are the very persons who are generally subjected to this treatment) are ill borne, and should, if possible, be avoided. Mr Gay believes the time will come, and that shortly, when the perinæal section will be very rarely performed, and the puncture from the rectum be, so far as operations are concerned, the *rule* of practice."—*Lancet*, July 13, p. 60.

[We understood that the rule of practice had been to relieve the patient by the use of catheters; and that the operation of puncturing the bladder, though taught in the schools for the benefit of practitioners so situated as not to acquire the practice requisite for effecting their introduction, was an expedient to which an hospital surgeon should never require to have recourse.]

PRESSURE *v.* LIGATURE.

The "Dublin Medical Press" lately complained that both of the Edinburgh professors of surgery were opposed to the plan of pressure for the cure of aneurism; but has since discovered its error, and claimed the occupant of the systematic chair as a supporter. That gentleman has just published another edition of his "Principles of Surgery," which we are glad to see no longer retains the form of a compendium. In this work he advocates compression of the femoral artery, instead of its ligature, and states the result of his experience. It amounts to the treatment of two cases, in one of which, after a lengthened trial of pressure, the ligature was found necessary; and in the other, mortification, amputation, and death, realised the worst fears which the Professor of Clinical Surgery has ever expressed in regard to this mode of treatment. The Dublin doctors may well say, "Protect us from our friends."

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

DISPLACEMENT OF THE OVARY. BY DR RIGBY.

During the last three years I have had occasion to notice several instances of a painful affection of the pelvic region, which I had not observed before, and which, as far as I know, has not yet been described. It is characterised by intense and peculiarly sickening pain about the sacral region, extending to one or other of the groins, and coming on in paroxysms of such agonising severity as to render the patient perfectly frantic with the intolerable suffering. In some patients the intermissions of ease were nearly or quite entire; in others, the pain, although divested of its characteristic intensity, never wholly abated. The source of the pain was evidently connected, directly or indirectly, with the rectum, for the passage of fæces was frequently attended with some difficulty, and always with great suffering.

According to the patient's feelings, it seemed as if a partial obstruction existed somewhere up the rectum, the smallest pressure upon which, by the passage of fæces, was sufficient to bring on a paroxysm of this much dreaded pain. At other times she could scarcely tell what had been the exciting cause of the attack; for, like a fit of tic douloureux, it would frequently come on from no assignable reason, and cause her the severest sufferings for some hours. Patients describe the pain as being quite peculiar, and of a sickening and utterly

intolerable character, such as they had never before experienced; indeed, from the descriptions which one or two have given of it, I should presume that it closely resembles the intense and peculiar suffering which patients describe in cases of orchitis. The pain was usually attended with great throbbing, and with a painful sense of forcing, or distension of the tender part, amounting almost to bursting, like something strangulated.

The menstrual periods have always been attended with intense pain, particularly during the early part of the discharge, though this varied a good deal in different patients, and (as far as I have seen) the discharge was invariably attended with exudations and small coagula. At these times the whole lower part of the abdomen was frequently tender to the touch, and more or less fever was generally present, probably arising in part from the degree of suffering which had been induced. The tongue always showed the dry short-napped fur which is so constantly seen in cases of disease, or displacement of the pelvic viscera; the digestive organs were much deranged, and not unusually the stomach was extremely irritable.

On making a vaginal examination she would frequently wince, and complain as soon as the finger touched the os and cervix uteri; but a little care quickly sufficed to show that these parts were not morbidly tender, but that pain was produced by pressing them against the tender spot which was behind and to one side, in the direction of one or other of the sacro-iliac synchondroses, or sacro-ischiatic notches. On passing the finger, therefore, behind and to one side of the cervix, and pressing against the wall of the vagina in the above-mentioned direction, the painful spot is at once reached, and sometimes a slight degree of hardness is perceived.

On examining per rectum, the finger soon reaches the same acutely painful spot, which had been felt per vaginam. The patient dreads the slightest touch of it, however carefully applied. It is evidently a convex body, like an enlarged gland, though usually softer, situated in the recto-vaginal pouch; it is moveable, if the patient can bear a sufficient amount of pressure for that purpose, and usually one or more vessels are felt throbbing when the finger presses upon it. The ovary is generally larger than natural, being more or less swollen from the strangulation produced by its displacement; and when the swelling is considerable, not only will pain be produced by pressing in the groin of the same side, but the ovary will be distinctly moved upon the finger per rectum. From the fact of its mobility can be explained the circumstance of our being able to feel it sometimes lower in the pelvis than at others, and why the patient is in greater suffering when it is so than when felt higher up. Hence the passage of a solid mass of feculent matter is attended with fearful sufferings; the ovary is pushed down by the fæcal mass as it descends through the rectum, until its attachments are put considerably on the stretch; a further amount of swelling is produced by this state of strangulation, and in this condition the fæcal mass is at length forced past, to the indescribable agony of the patient, frequently leaving her in severe pain for many hours afterwards.

In other cases the ovary is nearly or quite fixed, having apparently contracted adhesions to the neighbouring parts.

It is not easy to speak decidedly as to the causes of this displacement, but I have chiefly, or almost solely, observed it among women of a lax, flabby habit, prone to passive menorrhagia, leucorrhœa, and abortion, but most particularly where the uterus has been retroverted. I have long since pointed out that ovarian irritation or inflammation is a frequent result of retroversion in the unimpregnated state, arising probably from the tension to which the broad ligaments are exposed, and consequent engorgement of the ovary.

The diagnosis is not difficult, for the pain is quite peculiar. It is of a forcing, throbbing character, so sickening and utterly intolerable, as to be entirely different from any other pelvic pain with which I am acquainted. Its seat is referred to the upper and posterior part of the vagina, usually somewhat to the left side where the ovary can be felt, especially upon examination per rectum.

The direction in which we can best reach the painful spot will guide us to applying leeches per rectum or vaginam ; they pretty certainly produce much relief, although, so long as the displacement continues, it cannot be more than temporary. The grand object necessarily must be to restore the ovary to its natural position, and this, whether connected or not with retroversion, will be best obtained by the prone position. The patient soon learns by experience what amount of lying forwards will be required, but, as in cases of retroversion, it is generally found necessary to assume an extra degree of prone position for a few minutes (knees and elbows) previous to lying down prone upon the couch.—*Medical Times*, July 6, 1850.

TAPPING OF A FIBRO-CYSTIC TUMOUR OF THE UTERUS. BY P. HEWETT, ESQ.

An unmarried female, æt. 47, was admitted into St George's Hospital, under the care of Dr Wilson, with great swelling and distension of the abdomen. The symptoms, which had existed about twelve months, had been at first confined to the left iliac fossa, but had subsequently spread over the greater part of the belly. Fluctuation was very evident in various regions, and the disease presented all the characters of ovarian dropsy. Œdema of the legs was present, as well as pain in the region of the heart, and difficulty of breathing in going up stairs. The general health had not been much affected, but of late she had lost flesh ; the catamenia had been absent for the last six months ; the urine was scanty and highly acid. She was put on diuretics and good diet. After five days, it was found that she had decreased two inches in circumference round the abdomen, and that there was also much less swelling of the feet. Under this plan of treatment, she, at first, continued to improve slightly; but the symptoms and consequent distress having subsequently increased, Mr Hawkins tapped the abdomen, and drew off fifteen pints of thick fluid, of a reddish colour, and mixed, towards the last, with blood and some flakes of lymph. After the operation, it was observed, that the decrease in size had occurred principally on the left side, and two masses of solid substance were detected, which appeared to form part of a tumour, rising from the pelvis. The operation was, at first, followed by marked relief ; but, two days afterwards, symptoms of low peritonitis appeared, and the patient died on the eighth day after having been tapped.

Examination of the Body Eighteen Hours after Death.—The cavity of the peritoneum contained a large quantity of dark-coloured fluid, mixed with flakes of recently effused lymph, which served to glue together the convolutions of the intestines. In its lower two-thirds, the abdomen was occupied by a large tumour, which, rising out of the pelvis, had displaced the intestines, and become attached, by slight adhesions, to the anterior wall of the belly. The upper part of this tumour was composed of large membranous-looking cysts, with thin walls, the interior of which was inflamed, and filled with a quantity of thick, dark-coloured fluid. It was one of these cysts which had been tapped during life. Towards its lower part, the tumour was principally formed of a more solid substance, and filled with an enormous number of cysts, varying from the size of a pin's head to that of a large orange. These cysts, which were all lined with a thin, smooth, delicate-looking membrane, were filled with clear fluid, containing a large quantity of albumen. The diseased mass was, at first, thought to be connected with one of the ovaries, but both these organs were found to be lying behind it, and quite healthy. On further inspection, the tumour was traced to the right side of the fundus of the uterus, to which part it was connected by means of a pedicle, two inches in breadth, and an inch and a-half in length, formed by the fibres of the uterus, which were traced upwards some distance, and then lost. Among these fibres were several vessels of a large size. Here and there, in the lower part of the tumour, were scattered some spots of fibrous tissue, hard, dense, and without any cysts. In the body of the uterus, deeply imbedded in its structure, there was a common fibrous tumour, of the size of a bean. There was no affection whatever of any

of the glands. The other viscera contained in the abdomen, as well as those in the thorax, were all quite healthy.

Microscopic examination proved that the tumour was essentially of a fibrous character.—*London Journal of Medicine*, July 1850.

ON A NEW STETHOSCOPIC SIGN INDICATIVE OF THE SEPARATION OF THE PLACENTA. BY M. CAILLAULT.

M. Caillault alleges, that on applying the stethoscope to the hypogastric region, after the expulsion of the child, and before the expulsion of the placenta, he hears, with each returning uterine contraction, a sound, feeble at first, but the intensity of which increases in proportion as the uterine contraction becomes more energetic, and then subsequently diminishes and disappears as the uterine contraction subsides. The sound is composed of a series of rapid small scratching noises, somewhat like those produced by drawing the nails at right angles across the seat of a sofa. It returns with each uterine contraction till the placenta is expelled from the uterus, when it ceases. M. Caillault believes that it depends, not upon the uterine contractions, but upon the mechanical disunion or separation of the placenta; and hence he proposes to term the sound "bruit de decollement placentaire."—*L'Union Medicale.*

Part Fifth.

MEDICAL NEWS.

NOTE ON DR GREGORY'S PROCESS FOR PREPARING PURE CHLOROFORM. BY PROFESSOR CHRISTISON.

The process proposed by Dr Gregory in the May Number of this Journal for preparing a perfectly pure chloroform, has given occasion to various notices in contemporary journals, some of them laudatory, and others the reverse. Among the latter may be mentioned the statement of a late writer in the "Pharmaceutical Journal," that chloroform prepared in Edinburgh, avowedly by the process in question, was found, in Liverpool, to have undergone decomposition, to have become loaded with chlorine, and to be quite unfit for use. In Dr Gregory's absence on the continent, I have felt it to be my duty to make inquiry into the truth of this statement; and the result is, that no less than three manufacturers in Edinburgh have tried the process on a considerable scale, and that all have failed to obtain a permanent article. A chloroform of fine quality is obtained in the first instance; but it does not keep many days; and ere long it becomes so loaded with chlorine that its vapour cannot be inhaled.

I have not had time to investigate fully the cause of this unexpected phenomenon, or to ascertain whether it is an inherent fault in the process, or depends rather on some manipulation having been practised, or left out, which Dr Gregory may have neglected to particularise. But this much seems certain,—1. That chloroform purified by one treatment, with pure sulphuric acid, free of nitrous acid, keeps perfectly; 2. That such chloroform, if left in contact with ordinary sulphuric acid, containing, as it generally does, an impregnation of nitrous acid, will, in less than twenty-four hours, undergo decomposition, and afterwards rapidly evolve chlorine; 3. That if a drop or two of nitrous or common nitric acid be added to an ounce of pure sulphuric acid, the decomposition goes on with far greater speed; 4. That if the sulphuric acid be

carefully freed of nitrous acid, so as not to affect solution of green vitriol, no change may occur in the chloroform for an entire week ; 5. But that, nevertheless, even with pure acid, decomposition begins at last, so that, in four weeks, the odour of chlorine is intolerable. It therefore seems probable, that the best chloroform cannot resist the protracted contact of sulphuric acid ; and that even the repeated action of that acid,—for example, when used to effect thorough purification, according to the method of Dr Gregory, produces some change, which, though inappreciable at first, gradually leads to decomposition in the end. Why this happens, whether it always happens, or in what circumstances, are questions which we are not prepared to answer. It is probable, however, that Dr Gregory will be able ere long to do so satisfactorily ; for there is a specimen of chloroform in his laboratory, which was prepared six months ago by his process, and which is still quite unaltered. Meanwhile, it is obvious that manufacturers ought not to adopt that process until farther explanations shall have been given. It may be added, that the Edinburgh manufacturers have recalled from their customers the chloroform which had been manufactured by the repeated use of sulphuric acid ; and that the spurious article is very easily known by the pungency which is added to the purely etherial odour of the uncontaminated preparation.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The late meeting of the Association in Edinburgh was, as regards the numbers of those who attended it, the most successful which has been held since the former meeting in the same city in 1834. As was to be anticipated, it became necessary to institute a Physiological Sub-section, which, during the two days on which it was open, was very crowded. Indeed there can be little doubt, that had it been instituted earlier, it would have constituted a principal feature of the meeting.

The proceedings at some of the sections struck us as exhibiting less than formerly that high scientific and original character which would satisfy rigid *savans :* there seemed to be a tendency to popularise, and an unwillingness to hazard criticism ; while the memoirs brought forward were in various instances mere re-statements of papers and views long published, or laid before different learned societies, and hence not new to the scientific world. The miscellaneous nature of the auditory may, in part, account for the predominance of the popular element in some of the communications made to the sections ; and we question much if such crowds of fair associates would have been attracted thither had the nature of the entertainment provided for them been more severely intellectual.

Although the still rather unsettled state of the Continent prevented such an influx of foreign visitors as we had expected, it was still gratifying to see the chief seats of learning in Europe represented by some of their brightest ornaments ; and the reception which these gentlemen met with, while it could not but be pleasing to them, was, we are proud to think, not unworthy of our national character for hospitality. Public entertainments were given by the College of Physicians at Barry's Hotel, and by the President of the College of Surgeons, Mr Syme, at his villa at Millbank House ; while many other members of the medical profession distinguished themselves by the exertions which they made to promote the comforts of the strangers who became their honoured guests.

MR D. R. RANKIN AND THE "MONTHLY JOURNAL."

In a letter which appeared in the "Lancet" of the 17th August, we are bitterly reproached by Mr Rankin, as guilty towards him of breach of "courtesy" and "morality ;" and he kindly warns our contributors that we are not to be trusted. We are unwillingly compelled to notice the cause of Mr Rankin's amusing wrath, which private communications have not tended to appease.

Mr Rankin sent to Professor Goodsir an account of a case of labour rendered difficult by the presence of a tumour. The tumour itself, which was expelled from the uterine passages thirty or forty days after delivery, was sent also to Professor Goodsir, who readily detected it to be a well-marked specimen of the common fibrous tumour of the uterus. The history of the case also, as well as the structure of the tumour, showed it most certainly and indubitably to be a uterine tumour, and not, as Mr Rankin had incautiously fancied it, an ovarian growth. In printing Mr Rankin's communication, we ventured to alter the designation of the case, and one or two expressions in the text, in accordance with this view. We altered none of Mr Rankin's *facts;* but merely corrected his evidently erroneous *opinions* as to the nature of the tumour, and in consequence of thus kindly preventing him from speaking incorrectly and absurdly of the nature of the interesting case he had met with, we are, forsooth, guilty of a want of "morality," &c. &c.

Without setting right Mr Rankin's pathology in this very important particular, we could not have permitted the case to appear in the "Monthly Journal." With this exception, then, the communication, as published in our July number, is Mr Rankin's own. The facts we repeat are unaltered. Some of the introductory and concluding observations were assuredly suppressed, as having nothing whatever to do with the case, and being, in several points, far too crude and puerile to inflict upon our readers.

It is certainly to be regretted, that Mr Rankin did not, when he sent his case and specimen, accompany the gift with a request that a proof should be sent him. This request is usual with our country contributors, especially with those who contribute for the first time, and it is invariably complied with; and still we are sorry that a proof was not sent to Mr Rankin, for we believe it might have opened his eyes to the true nature of his case, of which he was previously ignorant, and of which we despair now of convincing him.

"Rejection," he remarks, "was easy." It certainly was so; and the hint thus conveyed will not be lost upon us and others, in the improbable event of similar contributions emanating from the same quarter.

VARIETIES.

Homœopathy in the House of Commons.—On the 2d of August Lord R. Grosvenor, President of the Homœopathic Association, presented a petition to the House, from that body, complaining of the conduct of the deputy-coroner for Middlesex. Mr Wakley said that the House of Commons was not the proper place for such complaints, but it was considered a better and a cheaper place for advertising the quackery of the Homœopathic Institution, than the courts of law. He hoped, however, that honourable members would set their faces against this impudent proceeding. He called it "impudent," for he believed that a more audacious set of quacks did not exist, and could not be found upon the surface of the globe, than were to be found in the Homœopathic Institution. It consisted partly of noodles and partly of knaves. The noodles formed the majority, and the knaves used them as tools. It was perhaps quite right that they should have a dread of coroners, and they had a natural dread of them; and now their object was not only to advertise their association by means of this petition at the cheapest possible rate, but to terrify coroners from the faithful discharge of their duties to the public. He knew not how far the House would allow them to succeed in their first object, but he knew that they would not succeed in their second. (Hear, hear).—*Lancet*, August 10th.

Naval Assistant Surgeons.—By an Admiralty order, issued on the 17th July, and which has appeared in all the weekly medical periodicals, some concession is made to improve the condition of this long ill-used class of officers. Still it seems, that the privilege of admission to the ward-room mess can only be attained after three years passed in the drudgery of the old system,—and that a separate cabin is to be put at the disposal of the *emancipated*, and of them only—"*when the accommodation and space on board will permit*," from which

expression we are entitled to infer, that the cabin will not always be forthcoming. This is a mode of doing things by halves, which will please nobody. Captain Boldero must again take the field.

Inauguration of the Statue of Larrey.—This statue, the work of M. David (d'Angers), has been recently erected in one of the courts—*la cour d'honneur*—of the Parisian Military Hospital—the Val-de-Grâce. At its inauguration, on the 9th August, deputations from the principal public bodies of the capital attended, and a number of orations were delivered in honour of the illustrious surgeon, and of the service to which he belonged. None of the orators seem to have so completely charmed the auditory as M. Dupin, the President of the Assemblée Nationale, whose brilliant little improvisation, reported in the "Gazette des Hôpitaux," conveys, even to our minds, the perfection of eloquence and fine feeling.

Honours Opened up to Medical Men.—In the "London Gazette" for August 16, there is a formal notification, that the Order of the Bath will in future be given to distinguished military and naval medical men. The honour has already been conferred on seventeen members of the profession.

Hydrophobia.—A case of hydrophobia, the particulars of which we hope to publish at length in a future number, has lately proved fatal in the practice of Dr Sidey of this city.

Sangrado Redivivus.—The Spanish journals narrate the case of a man, aged 70, born at Majorca, of sanguine and apoplectic temperament, who, according to an approximative calculation, has, during a period of fifty-five years, been *two thousand times* bled at the arm, and on each occasion at least a pound of blood has been abstracted. From the age of 15 this man was obliged to have himself bled every month, in order to combat his tendency to apoplexy. At the age of 20, he was compelled to have recourse to the operation twice every month; at 25, thrice a month; afterwards, thrice every fifteen days; finally, he has sometimes been bled fourteen times in a month. Even still, he is bled twice or thrice a fortnight to prevent his tendency to apoplexy. —*L'Union Médicale*, 15th August 1850.

London Apothecaries' Company.—This Company have lately successfully prosecuted a chemist and druggist for practising as an apothecary ("Lancet," August 24th, p. 243). In doing this we conceive they are more worthily occupied than in prosecuting well-qualified members of the medical profession, as in the case of Dr Burt.

PUBLICATIONS RECEIVED.

A Manual of Materia Medica, Pharmacology, Toxicology, &c. By Edward Chapman, M.D., &c. Edinburgh: Maclachlan and Stewart. 1850.

Principles of Surgery. By James Miller, F.R.S.E., Professor of Surgery in the University of Edinburgh. Second Edition, 8vo. Adam and Charles Black, Edinburgh. 1850.

On the Management of Infancy, with Remarks on the Influence of Diet and Regimen. By Charles Hogg, M.R.C.S., L.A.C., &c. London, 1849.

On Animal Chemistry, in its Applications to Stomach and Renal Diseases. By H. Bence Jones, M.D., &c. London, 1850.

Monthly Report on the Mortality and Public Health of Oxford, for March 1849.

The Baths of Rhenish Germany. By Edwin Lee. London, 1850.

On Murrain, or the Vesicular Epizootic. By Mr Finlay Dun, V.S., Lecturer on Mat. Med. in Edinburgh Veterinary School. Edinburgh, 1850. (A prize essay from the Journal of Agriculture, &c).

Journal für Kinderkranheiten; von Behrend und Hildebrand. Band xiv. Heft 1, 2, 3, 4.

Oppenheim's Zeitschrift für die gesammte Medicin. Band xliii. Heft 1, 2.

Zeitschrift für Erfahrungsheilkunst; von Bernhardi und Löffler. Band iii. Heft 1 and 2.

University of Edinburgh. Lithographic Likenesses of Drs Traill, Alison, &c. Schenck and Macfarlane, Edinburgh.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr Tilt of London, Dr M'Cormac of Belfast, Dr Hall of Montreal, Dr Nicholson of Antigua (with preparation), and Professor Schœpf-Merei of Pesth. Others have been privately acknowledged.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Reflections on the Causes which Advance or Retard the appearance of First Menstruation in Woman, with a Synoptical Table, showing the mean age of first menstruation in* 10,422 *women, in hot, temperate, and cold climates.* By EDWARD JOHN TILT, M.D., Senior Physician to the Paddington Free Dispensary, and Physician to the Farringdon General Dispensary and Lying-in Charity.

(*Read to the Physiological Section of the British Association, at Edinburgh,* 1850.)

THE period of first menstruation is alike interesting to the medical man, to the natural philosopher, and even to the statesman; for, as the fecundity of woman dates from that epoch, its knowledge is an indispensable element of many problems in medicine, in natural history, and in the science of population; and although it may seem that the precise determination of this epoch has no practical bearings, we shall soon find that a knowledge of its different dates in different circumstances throws considerable light on the nature and intensity of the powers which advance or retard the period when woman is first able to conceive.

The problem is bristled by all those difficulties which complicate every question of vital statistics; for, in our prognosis of the period of first menstruation in a girl, we must bear in mind the race to which she belongs, her abode in town or country, whether she is hardened by privations and out-of-door labour, or whether, being nursed in the lap of luxury, her nervous susceptibilities have been strung to the utmost,—various conditions which, like the extremes of temperature, retard or advance considerably the appearances of menstruation.

The causes which modify the period of first menstruation may be divided into those which are intrinsic and those which are extrinsic. By intrinsic causes, I mean those which are inseparably inherent to the female's constitution—as,

I. The race or stock from which she descends.

II. The parents from whom she inherits, physiologically as well as pathologically.

III. National customs, from the dominion of which neither herself nor her family can ever obtain complete emancipation.

I. *Race.*—This is an element of the problem which has scarcely been alluded to. All differences in the period of first menstruation have been ascribed to the agency of heat or cold, but it is necessary to ascertain whether the advance or retardation of the faculty of conception does not depend on certain original peculiarities of those races which inhabit hot or cold countries. Mr Roberton, of Manchester, in his valuable contributions to the natural history of woman, brought forward reports of missionaries, in Jamaica and Barbadoes, in order to prove that the negress menstruated about the same time as the inhabitant of our own country; but the observations he was able to procure are not sufficiently numerous (86) to warrant our building anything solid on the conclusions to which they point; and if the same conclusions were deduced from some thousands of observations, instead of denying, with Mr Roberton, the influence of heat, which is proved by the unanimous voice of residents in India, as well as by statistics, I should explain the fact by admitting, that the negro race is one peculiarly constructed to support labour in tropical climates. The skin of the negro is evidently modified to withstand heat,—may not, then, that duplicature of the skin which constitutes the generative intestine, be likewise modified in order that it may not permit conception to take place so early as in women of other races? It has been stated that, in this respect, Jewesses are very precocious, and their eastern origin was made accountable for the fact; but in dispensary practice I have been struck by the number of Jewesses in whom menstruation was delayed to the seventeenth, eighteenth, and twentieth year, and this inquiry might easily be followed by others.

America is a country peculiarly adapted for investigating the effect of race on this function,—for there, the European, the African, and the native or copper-coloured race, live under the same climate. I have, therefore, addressed to many eminent medical men of that country a series of questions relating to the menstruation of the women of the three races; and I am happy to find that the inquiries thus originated are likely to be very carefully responded to, for my queries have been published in the medical journals, and distributed to all the delegates to the National Medical Convention, which has just met at Cincinnati (Ohio). Perhaps it will not be out of place for me to record my admiration of the earnestness with which the Americans follow up every medical improvement which they receive from us—sending us back at times very valuable suggestions, and, amongst the latest, the means of plunging into Lethean slumbers those on whom it would be otherwise necessary to inflict intense pain by surgical or obstetric operation,—an admirable secret, with which one of your first men has identified his name by popularising it amongst us. Our Transatlantic brethren have shown most originality in the obstetric branch of the profession; and a lover of final causes would say that it must be so, as long as the voice of man is still unheard in so many of the vast solitudes of the New World.

From this digression I turn to another modificator of the conceptive faculty in woman.

II. *Family.*—How far does the period of the first appearance of menstruation depend upon the period at which that function first appeared in the mother?

If the lineaments of the face and external forms of the body may be transmitted, it is reasonable to suppose that the structure of the internal organs is likewise transmitted; and that, therefore, peculiarities of functions may be inherited as well as peculiarities of features. I have sometimes met with instances of this in the menstrual functions, but we have not sufficient data to decide whether this is frequently the case; and I therefore proceed to a third cause.

III. *National Customs.*—In India, for instance, dishonour is attached to the parents of a girl who is not married when extremely young. It is, therefore, the custom to affiance children of 7, 8, or 9 years of age,—and during their subsequent residence in the family of their intended husband connection generally takes place long before it has received the sanction of a religious ceremony. This precocious use of matrimonial stimulus is, no doubt, calculated to advance the period of first menstruation, and it is to the influence of this perverse custom, strengthened by the hereditary transmission of periodical phenomena in the parent, that Mr Roberton ascribes the uncontested early menstruation of Hindoo women; but I think you will consider this view too exclusive, when you remember that Arctic travellers have repeatedly told us, that the snow of the Polar regions can no more assuage the lascivious burnings of the barbarous tribes, which there eke out a miserable existence, than the mountain ice of Hecla can restrain the throes of the volcano.

In sober English, notwithstanding a lasciviousness which is said to equal, and even to surpass, anything that can be met with in India, still the mean of age of first menstruation is the sixteenth year instead of the twelfth, as in India. Negro women are likewise known to be subjected at a very early age to immoral practices, and still Mr Roberton says that the average age of first menstruation in them is about 16 years.

Extrinsic Causes.—By extrinsic causes I mean those which are external to the female, and of which she is more or less independent—as,

I. Temperature.
II. Habitation.
III. Civilisation.

I. *Temperature.*—It has always been, and is still, generally admitted, that a high or a low temperature acts on the generative process in woman as it does on the generative process in plants, hastening or arresting the budding forth of that which separates from the female to prolong the species. I believe that in this instance the current notion is the true one, and that a glance at the diagram, which is the reason of my drawing your attention to this subject, is suffi-

cient to prove the opinion. You will see that, while the mean age of menstruation is in Calcutta 12 years 6 months, in London 14 years 9 months, it is in Copenhagen 16 years. I do not forget that the maturing influence of heat is complicated by the influence of race; and that, whilst I can only explain, by a peculiarity of race, the fact of the mean age of first menstruation of 86 negresses being 14 years 10 months, Mr Roberton might propose to explain the precocity of Hindoo women in a similar way. To solve the question, it would be necessary that those who are favourably placed should compare the mean age of first menstruation in the negress inhabiting her own country, with that of her sable offspring transplanted into other climes; and likewise the mean age of the same period in Portuguese women living in Portugal, with that of the descendants of the Portuguese who densely populate Goa and other parts of India. A medical man of the Jewish persuasion might give us an interesting solution of the question by ascertaining the mean age of first menstruation in an inextinguishable race, living as it does unalloyed by other blood under opposed influences in so many climates.

Edinburgh has not been remiss in prosecuting this as well as other physiological investigations, for it was in successive volumes of its first medical journal that Mr Roberton published some of our most important documents on the history of menstruation, but I have not been able to find any Scottish tables of first menstruation. Now it would be interesting if such tables were drawn up in Edinburgh, and in the most northern extremity of our island, so that the results might be compared with those found in London. In drawing up these lists I would suggest that the observers, instead of stating that there are so many first menstruations at the ninth or tenth year, should do as my friend Dr Rawn has done, and mark them as occurring between the ninth and tenth year, from ten to eleven, and so on, which is much more correct.

II. *Habitation.*—The influence of a town or country habitation has been shown by Dr Brierre de Boismont in his valuable work on menstruation. Thus he found that the mean age of first menstruation in the country being 14 years 10 months, in Paris it was 14 years 6 months. Pursuing the inquiry still further, he ascertained that there was a difference in the period of first menstruation in women inhabiting small towns or the capital of the country, Paris. Thus in women of small towns, the average was 14 years 9 months; while in women inhabiting the capital, it was 14 years 6 months. The influence of elevation above the level of the sea on the menstrual function should also be ascertained, by tables of first menstruation taken in mountainous districts, and in the lowlands of the same country—an investigation which might be easily carried out in Scotland.

III. *Civilisation* has an immense influence in hastening the advent of possible fecundity in woman. It can create a tropical climate in the mansions of the rich, however cold the country; and it can bring about the precocious fructification of the human germ in the same

way that the gardener in the hot-house does that of the vegetable tribes. M. Brierre de Boismont is, I think, the only one who has demonstrated the influence of civilisation in a limited number of cases (53); thus he found a marked difference in the mean period of first menstruation in the poor, the well-to-do working classes, and the rich and noble.

He found the mean age of first menstruation—

	Years.	Months.
In 171 of the poor to be.....	14	10
In 135 of the well-to-do working classes.....	14	5
And in 53 daughters of the rich and noble, it was	13	8

The influence of this important agent has never been shown on a sufficient extensive scale. Indeed, it has been so much neglected, and comes so little into play in the tables which have been published, that I have thought it most correct to call the table I communicate to you a table of the ages of first menstruation in women of the lower classes, of hot, cold, and temperate climates,—for, with few exceptions, the observations were made on those who attend dispensaries or on those who crowd our hospitals.

I will suggest that a knowledge of the mean age of first menstruation in Quakeresses and Moravians, on account of the peculiar strictness of their mode of educating girls, might throw light on the combined influence of late hours devoted to dancing, music, theatrical representations, &c., powerful excitors of the nervous system, from which these communities think it judicious to abstain. I have questioned my friend Dr Hodgkin on this subject, but he was not prepared to answer my inquiries.

The table contains various lists which have been published before, besides two very important lists derived from cold countries, and which are not known in England. The one was collected by Dr Frugel in the Lying-in Hospital of Christiana, and published by him in a work on the state of Norway. The other list, which I have lately received from my friend Dr Rawn of Copenhagen, is still more important, as it contains 3429 observations carefully made in that capital.

This list is made up of 3429 cases, published by Dr Rawn ("Bibliothek for Læger," Jan. 1850), to which are added 411 additional cases, taken by Professor Leog at the Royal Lying-in Hospital of Copenhagen, and this is the first time that this list is made public.

I have carefully calculated, and by the same process, the average period of first menstruation to be deduced from the column which represents each country, and it is not my fault if the averages which I find differ in many instances from those given by the authors who have furnished us with the lists. Diffident of my own familiarity with figures, I obtained the assistance of a good mathematical scholar, who revised my calculations, and who found that many of the lists as originally given were not even correctly added up.

TABLE OF THE PERIODS OF FIRST MENSTRUATION OF 10,422 WOMEN OF THE LOWER CLASSES, IN HOT, TEMPERATE, AND COLD CLIMATES.

By E. J. Tilt, M.D.

GRAND MEAN OF ALL COUNTRIES, 14 Y. 4 M. 18 D.

YEARS.	HOT CLIMATES. Number of Observations, 629. Mean Age, 13 y. 0 m. 16 d.			TEMPERATE CLIMATES. Number of Observations, 5775. Mean Age, 14 y. 4 m. 4 d.										COLD CLIMATES. Number of Observations, 4018. Mean Age, 15 y. 10 m. 5 d.			YEARS.
5	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	5
6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6
7	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	7
8	3	...	...	...	...	...	...	...	2	1	...	...	...	...	...	...	8
9	7	1	...	1	...	...	...	1	10	4	14	...	...	...	...	...	9
10	14	4	...	...	...	...	5	...	29	5	55	...	...	...	1	...	10
11	37	41	1	5	2	6	14	3	93	53	67	10	...	...	5	...	11
12	66	71	5	6	11	10	26	14	105	90	123	19	3	...	18	1	12
13	49	74	10	3	19	13	47	6	132	183	210	53	8	4	107	2	13
14	41	46	16	4	35	9	50	18	104	266	311	85	21	7	363	2	14
15	11	29	15	3	67	16	76	14	190	291	320	97	32	22	712	4	15
16	6	16	17	4	40	8	79	7	141	234	264	76	24	43	694	4	16
17	3	11	12	4	41	4	58	6	127	181	158	57	11	31	581	3	17
18	1	2	7	3	12	2	38	5	90	105	112	26	18	19	518	3	18
19	...	3	5	...	11	...	21	8	35	45	42	23	10	13	347	...	19
20	1	...	1	...	4	...	9	3	30	26	29	4	8	13	265	...	20
21	...	2	...	...	...	...	5	...	8	8	9	...	1	3	109	2	21
22	...	...	...	...	...	...	1	...	8	3	4	...	...	2	71	...	22
23	...	1	...	...	...	...	...	...	5	2	1	...	1	...	27	...	23
24	...	...	...	...	...	...	3	...	...	...	...	...	...	...	12	...	24
25	...	...	...	...	...	...	...	...	...	1	...	...	...	...	4	...	25
26	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5	...	26
27	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	27
28	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	28
29	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	29
No. of Cases.	239	301	89	33	242	68	432	85	1111	1498	1719	450	137	157	3840	21	No. of Cases.
Mean Age.	Y. M. D. 11 11 13	Y. M. D. 12 5 23	Y. M. D. 14 10 12[1]	Y. M. D. 13 3 7	Y. M. D. 14 10 14	Y. M. D. 13 7 7	Y. M. D. 14 11 19	Y. M. D. 14 0 0	Y. M. D. 15 5 9	Y. M. D. 14 2 8	Y. M. D. 14 3 4	Y. M. D. 14 7 22	Y. M. D. 15 2 5	Y. M. D. 16 0 24	Y. M. D. 16 0 13	Y. M. D. 15 3 7[2]	Mean Age.

Country.	Calcutta (Bengal).	Deckan (Bombay).	Jamaica.	Corfu.	Madeira	Marseilles, Toulon.	Lyons.	Paris.	Paris.	London.	London.	Manchester.	Gottingen.	Christiana.	Copenhagen.	Labrador.
Race.[3]	Hindoo-Gangitic.	Tamulian.	Negro.	Hindoo-Germanic.	Hindoo-Germanic.	Celtic-Hindoo-Romano-Germanic.				Gothic-Hindoo-Germanic.				Gothic-Hindoo-Germanic.		Esquimaux.
Authorities.	Dr Goodeve. Dwarkanauth Bossu.	A. H. Leith, and others.	Rev. J. Elliot, and others. Mr Bowen.	Dr Tarizano.	Dr Dyster.	Bouchacourt.	Petrequin.	Dr Marc d'Espine.	Brierre de Boismont.	Dr Grey.	Dr Lee and Dr Murphy.	Mr Roberton.	Osiander.	Dr Frugel.	Dr Rawn.	J. Lundberg.
Indications.	Mr Roberton: Med. and Surg. Edin. Journal, vol. 64.	Ditto, vol. 64.	Ditto, vols. 58 and 69.	Ditto, vol. 62.	Ditto, vol. 66.	Art. Menstruation, 19th vol. of Dic., in 25 vols. (2d edition.)	These. Paris, 25th August 1835.	Archives Gen. de Med., Oct. and Nov., 1835.	De la Menstruation (Prize Essay), 1835.	Medical Times, vol. xii.	Dublin Med. Journal, No. lxxvii., 1845.	Mr Roberton: Med. and Surg. Edin. Journal, 1832.	Denkwürdigkeiten für die Heilkunde und Geburtshilfe, 11 bre. bd. 2, sl. 1795, ms. 320.	A Government Report on Norway.	Bibliothek for Læger, list of 3429,— to which Dr R. has added 411 cases taken by Professor Leog.	Mr Roberton: Med. and Surg. Edin. Journal, vol. 63.

This average is deduced from too small a number of facts to counterbalance the contrary testimony of travellers.

[2] This average is likewise deduced from so small a number of observations that it cannot weigh against the opposing statements of travellers, amongst whom I shall mention Dr M'Darmid, who informed Dr Campbell that, in Esquimaux women, menstruation was often delayed till the 23d year, then only appearing in the warmest months of the year, and as a mere show.

[3] I follow my friend Dr R. G. Latham's nomenclature, as given in the late valuable work which has just issued from his pen ("Natural History of Man").

Under each column will be found the average period of first menstruation in different countries,—a period which is also indicated by the line which intersects each column. At the head of the table you will also find the averages of the three groups of hot, of cold, and of temperate climates, as well as that of the whole 10,422 observations, which gives an idea of the general mean starting point of fecundity in the less fortunate females of our race.

Before making any calculation, a glance at the various columns placed side by side would be sufficient to show that a line passing under the figures which indicate the 14th year, would be a kind of menstrual equator, dividing pretty equally the sum of first menstruation in temperate climates; and that while the same line would pass beneath the greatest amount of first menstruations derived from hot countries, it would pass above the greatest amount of first menstruations occurring in cold countries, thus giving a graphical demonstration of the influence of cold in the arresting of, and of heat in the ripening of, those germs of the human race which lie deposited in the ovaries. By heat I do not merely mean that force, in virtue of which the mercury rises when a thermometer is plunged in boiling water, but that imponderable compound which is heat, and light, and magnetism, the triune manifestation of one electric influence, whose inexhaustible focus seems to be the sun and centre of our planetary system,—three manifestations of the one power, which thus imparts to air, to water, and to earth the possibility of being tenanted by that which has life. It would indeed be strange if this solar influence—a power which has such wonderful effects on the transmutations and regenerations of the inorganic structures, which so sensibly actuates the phenomena of generation in the whole vegetable kingdom, and has no less visible effects on those of many animal organisms—should have none at all on the elaboration of the human germ.

From the manner in which it harmonises with what we have already learned to read of the laws the Almighty has written in his creation, the influence of heat and cold in the development of menstruation is rendered probable,—supported by analogy, the opinion gathers strength,—it becomes confirmed by the general voice of travellers,—and at last it receives the definite sanction of precise statistical data. Thus we again see what you have often seen, each in your particular department, that the true is beautiful, and that the beautiful is true.

ARTICLE II.—*Case of Mulberry Calculus, of Seventeen Years' Standing, Successfully Extracted by the Lateral Operation.* By JOHN PAUL, M.D., Fellow of the Royal College of Surgeons of England.

JAMES GORDON, æt. 22, became my patient about a fortnight before his admission into Gray's Hospital, Elgin, on the 12th April 1850. He was then in a most deplorable condition, being confined to bed, and his urine dribbling from him with the most horrible agony. When it was collected, there subsided a very large muco-purulent deposit. The urine itself was of average specific gravity, and about neutral. He had felt, he said, pain in making water since he was five years of age; still he pursued the usual occupations of boyhood, and learnt to be a currier, at which business he worked till last February, when, it would appear, chronic inflammation of the bladder had set in. He was then incapacitated for work, and confined entirely to bed. On my first seeing him, he was much attenuated. His pulse was greatly quickened. Tongue raw, red, and chopped. In a word, irritative fever, with all its accompaniments, was in rapid and destructive progress.

On sounding him, a large calculus was at once detected.

On the 16th April, he was brought fairly under the influence of chloroform, and the lateral operation performed. There was considerable difficulty experienced in introducing the forceps and seizing the stone, owing to the bladder being very firmly contracted round it. After the stone was laid hold of, it slipped, as, owing to its rough and tuberculated surface, it did not lie well within the blades of the instrument. It was, however, laid hold of more readily a second time, and probably more precaution used in maintaining the grasp, when, not without some resistance, a large mulberry calculus was extracted. Succeeding to the operation there was some depression, and in the course of a few hours he had about four ounces of brandy. As a diluent, he was ordered barley-water with the bicarbonate of potash, and was allowed beef-tea and arrow-root. In the evening he felt easy, but complained of thirst. Urine came pretty copiously by the tube.

17th.—Has slept well, and expresses himself as being wonderfully easy. Pulse 110; less thirst; urine by tube copious. To have four ounces of port wine daily. Continue the diluent, beef-tea, and arrow-root. A bag containing chamomile flowers, dipped in hot water, to be applied to the abdomen.

18th.—Has slept well; perfectly free from any uneasy feeling; pulse 110; no thirst. The tube is removed. There is copious wetting. To have eight ounces of wine daily.

19th.—Has slept well; copious wetting; along with the urine a few bubbles of gas were observed to escape; feels easy. Wound to be kept clean.

20th.—Feels perfectly well; tongue still very raw; pulse 96.

22d.—The wetting is copious, but portions of feculent matter are now passing from the wound. Some sloughing of the partition between the wound and rectum has taken place. The skin of the perineum is red and tender.

23d.—Pain in the wound has kept him from sleeping. Pulse 105. No abdominal pain.

24th.—Rather better; has had some sleep; pulse 86; appetite poor; bowels rather confined. To have infusion of senna with manna.

25th.—During last night felt annoyed from the action of the medicine; purging still continues, though gentle; pulse 108. To have brandy instead of wine, and an opiate.

26th.—Purgative given on the evening of the 24th still continues to act; is not so well; the opiate to be repeated; omit beef-tea and barley-water; to have a beef-steak; pulse ranging from 110 to 124 during the night; in the morning it fell to 96.

27th.—Slept soundly during the night for about seven hours; feels much better; tongue still raw and red; pulse 76; urine still flowing by the wound, which is granulating favourably.

29th.—Pretty well; wound healing. Felt a desire this morning as if he would pass urine by the urethra.

31st.—Much the same,—some feculent matter passing in the tract of the wound.

June 3d.—Discharged from the hospital.

In about three weeks after leaving the hospital he went to the sea side, where he improved rapidly in his general health, and began very soon to void the greater part of his urine by the urethra. At this time, 10th September, he is in vigorous health, only, when he voids urine, he loses a small quantity through a small fistulous aperture, which communicates with the rectum. The fistula is evidently the result of the partition between the wound and rectum being laid into one tract by sloughing, which was produced by the part being unavoidably bruised in extracting the stone. Whether this vesico-rectal fistula will close or not is more than I can say. The urethra is much contracted about the bulb; and to open it up I have begun to introduce, every three or four days, a silver catheter, which I allow to remain in the urethra for about an hour. By and by I intend to touch the part with the nitrate of silver; but, in the meantime, I shall carry into effect the full dilatation of the urethra, which may accomplish the healing of the fistula.

In reviewing this case there are several features in it not unworthy of consideration from the operating surgeon. In the first place, it is evident that the patient had suffered from symptoms of stone for seventeen years,—a time which is remarkable not only for its length, but for the particular period of life at which it occurred. Then the restoration to health by an operation, undertaken, it must be admitted,

under circumstances very unpromising, is not to be overlooked. The quantity of pus[1] in the urine, as well as the prostrate state in which the patient was, could not fail to be looked upon as unpropitious for operative procedure. But the diagnosis was, that it was the product of inflammation of the mucous coat of the bladder, and although no doubt much against a favourable result from lithotomy, ought not to be a barrier to the operation; whereas, if it had come from the kidneys, the removal of the stone could not have been undertaken with any prospect of success.

The calculus itself is a fine specimen of the mulberry variety, and is in the possession of Professor Syme. It weighs 1492 grs., and is probably one of the largest mulberry calculi which have been extracted by operation.[2] It is of slightly flattened oval form, measuring in length 2·4 and in breadth 2·1 inches.

ARTICLE III.—*On the Fœtus in Utero, as Inoculating the Maternal with the Peculiarities of the Paternal Organism; and on Mental States in either Parent, as Influencing the Nutrition and Development of the Offspring.* By ALEXANDER HARVEY, M.D., Lecturer on the Practice of Medicine in the University and King's College of Aberdeen, and Physician to the Aberdeen Royal Infirmary.

IN a paper, published in this Journal in October last, I brought together a considerable number of instances, which serve clearly to show, that the peculiarities of a male animal that has once had fruitful intercourse with a female, can sometimes be more or less distinctly recognised in the offspring of subsequent connections of that female with other males. I therein stated, that of this singular phenomenon two explanations used formerly to be offered,—one class of physiologists referring it to a permanent impression made somehow by the semen of the first male on the ova of the female,—another class ascribing it to an abiding influence exerted by that male on the imagination of the female, and recurring to, and operating on, her mind at the time of her connection, subsequently, with other males. At the same time, I expressed an opinion, that neither of these explanations could be regarded as satisfactory; and I endeavoured to show that an explanation offered by Mr M'Gillivray

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[1] As to the existence of pus there could be no doubt; for, when a portion of the deposit was agitated with an equal quantity of liquor of potash, a dense, almost solid, gelatinous mass was at once formed. This was done by Dr J. H. Robertson, the house surgeon, and also by myself.

[2] The largest of which there is a cast in the Museum of the Edinburgh College of Surgeons weighed 4½ 3 29, and was removed by Mr Fergusson. —ED.]

of Huntly, is the true one,—viz. that while, as all allow, a portion of the mother's blood is continually passing by absorption (and assimilation) into the body of the fœtus, in order to its nutrition and development, a portion of the blood of the fœtus is as constantly passing, in like manner, into the body of the mother;—that as this commingles there with the general mass of the mother's own blood, it inoculates her system with the constitutional qualities of the fœtus;—and that, as these qualities are in part derived to the fœtus from its male progenitor, the peculiarities of the latter are thereby so engrafted on the system of the female, as to be communicable by her to any offspring she may subsequently have by other males.

Since the publication of that paper, I have learnt nothing to shake, but much to support, the notion of *inoculation* therein suggested; and I have ascertained that many among the agricultural body in this district are familiar, to a degree that is annoying to them, with the facts then adduced in illustration of it,—finding that, after breeding *crosses*, their cows, though served with bulls of their own breed, yield crosses still, or rather mongrels; nay, that they were already impressed with the idea of *contamination of blood* as the cause of the phenomenon,—possibly, however, on the principle (adverted to by D'Alembert), that the doctrine so intuitively commended itself to their minds as soon as stated, that they fancied they were told nothing but what they knew before,—so just is the observation, that "truth proposed is much more easily *perceived* than without such proposal it is *discovered*." [1]

But, with regard to any one of the cases formerly given, as instances of inoculation of the fœtus, I would not now assert that it is an *unequivocal* instance; nor would I now affirm that the notion entertained by Sir Everard Home and others, of all of them being an affair of the imagination, is so absurd as it once seemed to me. The careful consideration, recently, of cases seemingly identical in kind with these, save and except that there never was coitus between the mother of the animals and the male animal which they resembled, has convinced me that it is not so clear a matter that they may not be referable to the agency of mental causes. A setter bitch, for example, takes a fancy to a cur dog, and without ever having access to him, produces once and again, to dogs of her own breed, whelps bearing a decided resemblance to the cur. I am still inclined to think, however, that there are good grounds for believing that the greater number at least of the cases are truly referable to the principle of inoculation; and it is very clear that such facts as those exemplified in the progeny of the setter are noways adverse to that theory.

It seems to me probable, indeed, that future inquiry will demonstrate, that there are in nature two sets of cases connected with the peculiarities of offspring, analogous, nay, identical, in their external characters, yet essentially different in their origin and cause,—the

[1] Archbishop Secker, Works (Ed. 1811), vol. iii. p. 237.

one referable to mental states in one or other of the parents, and oftenest in the female parent,—the other to a change effected in the constitutional powers of the female by a physical (organic) agency originally extrinsic to her, but inherent in a former fœtus by derivation from its male progenitor, and conveyed to her, in the way of inoculation, by that fœtus while in utero.

To establish this, and to determine also the relative proportion of the two sets of cases, together with the comparative efficacy of their respective causes, and the conditions that are essential to the agency of each cause, would be an achievement in physiology of great general interest, and, to breeders, of much practical value. It were idle, perhaps, to engage in so wide and difficult an inquiry with any hope of complete success. But as to some important points, and, in particular, as to the main question here at issue, experiments in breeding on the lower animals, so conducted as to be free from essential sources of fallacy, might, I am informed, easily be made; and as they could be carried on on the large scale, decisive results might ere long be obtained. Some suggestions as to the kind of experiments which might be instituted, and the mode of conducting them, will be offered in the sequel. As to their probable issue, it were, perhaps, wise at present not to hazard an opinion. But, "if reason can sometimes go farther than imagination can venture to follow," the latter can sometimes go in advance of, and pioneer the way to, the former. And possibly in this instance, if leaving *reason* to follow at her leisure in the broad day and clear sunshine, we hold on our way, warily, in the dim twilight, in company with *imagination*, we may, under her guidance, securely reach a post in advance, and have the satisfaction afterwards of finding reason treading in our steps.[1]

Before entering on the consideration of mental states in either parent, as influencing the nutrition and development of the fœtus in such a manner as to cause the latter to resemble a male animal not its progenitor, I purpose first, in this second communication, to submit some additional observations on the theory of inoculation, with the view of placing this doctrine on a firmer footing.

I. (1.) Without at present raising any question as to the nature of the phenomenon, I may remark generally, that, since the publication of my former paper, farther inquiry has satisfied me of the truth of an observation there quoted from Mr M'Gillivray,—viz., that cases of the kind given as instances of inoculation of the maternal system by a former fœtus, are, as regards *cattle* and *dogs*, "of every day occurrence;" and judging from the two remarkable cases presently

[1] "If hypotheses are introduced sparingly, and the grounds on which they rest fairly stated, they are admitted to be part of the process by which the knowledge of the truth is attained, even in the most strictly inductive sciences; and those who profess to reject and despise them, are not those whose opinions are the most exempt from their influence."—Alison, Preface to Outlines of Physiology. 3d Edition.

to be given of the like phenomena in the *sheep*, they are probably equally common in other classes of animals. I have not, indeed, seen any such myself, but opportunities for doing so have not come in my way; and the information I have received is only of a general kind, sufficient to excite to farther inquiry, but wanting in specific details. There is one fact, however, kindly communicated to me by Sir John S. Forbes of Pitsligo, which, as perhaps bearing on this subject, it seems proper to mention. It is the fact, observable within these few years at all our great cattle shows, that among the high-bred heifers and cows of the Angus polled breed, *fat is laid on in lumps about the tail*, by feeding of the most moderate description—a feature heretofore considered to be characteristic of the Teeswater breed, and which, as occurring in the breed in question, though usually attributed to improved management and high breeding, may, perhaps, have its true solution in Mr M'Gillivray's theory,—on the supposition that the mother, however pure her own original breed and that of the father, had previously borne offspring to a Teeswater bull.

The following are the cases illustrative of the effect on the procreative powers of the *ewe* of previous sexual intercourse with a ram of a different breed; the one communicated to me by my friend Dr William Wells, of the island of Grenada; the other, by Mr William M'Combie, Tillyfour, in Aberdeenshire.

1. A small flock of ewes, belonging to Dr Wells, were tupped a few years ago by a ram, procured for that purpose, from the manager of a neighbouring estate. The ewes were all of them *white* and *woolly*. The ram was of quite another breed, being (besides having other marks of difference) of a *chocolate* colour, and *hairy* like the goat. The progeny were of course crosses, bearing, however, a great resemblance to the male parent.

The next season Dr Wells procured another ram of precisely the same breed as his ewes. The progeny of this second connection showed distinct marks of resemblance to the former ram in *colour* and *covering*. And the like phenomenon, occurring under the like circumstances, was noticed in the lambs of some other adjoining estates in Grenada, and was the occasion of equal surprise and perplexity to the owners of the animals.

2. Six very superior pure-bred *black*-faced *horned* ewes the property of Mr Harry Shaw, in the parish of Leochel-Cushnie, in Aberdeenshire, were tupped in the autumn of 1844, some of them by a Leicester, *i.e.*, a *white*-faced and *polled* ram; others of them by a Southdown, *i.e.*, a *dun*-faced and *polled* ram. The lambs thus begotten were crosses.

In the autumn of 1845 the same ewes were tupped by a very fine pure black-faced horned ram, *i.e.*, one of exactly the same breed as the ewes themselves. To Mr Shaw's astonishment, the lambs were all without exception *polled* and *brownish* in the face, instead of being black-faced and horned.

In autumn 1846 the ewes were again served with another very superior ram of their own breed. Again the lambs were mongrels. They did not, indeed, exhibit so much of the characters of the Leicester and Southdown breeds, as did the lambs of the previous year; but two of them were *polled*, one *dun*-faced, with *very small* horns, and the other three, *white*-faced, with *small round horns only*. Mr Shaw at length parted with those fine ewes, without obtaining from them one pure-bred lamb.

(2.) Of the analogous phenomenon, in any of the aspects formerly specified, in the human species, I have procured no additional examples.

Inquiries are on foot, in one or two of the West India Islands, as to whether the offspring of negro parents, after the female has had children by the European, ever exhibit traces of the latter; but the results have not yet reached me. Dr Maunsell, of Dublin, however, has mentioned to me a case, bearing on the communication of secondary syphilis,—in too imperfect a form, indeed, to warrant its being adduced as an instance of the kind in question, but valuable as justifying the supposition formerly advanced, that a woman may somehow acquire syphilis, but have it in the *latent* form, and subsequently give proof of the reality of the fact by the birth of a syphilitic child, got by a perfectly healthy man. It is the case of a woman, who, as far as Dr Maunsell could learn, never herself exhibited any signs of syphilis, yet produced a syphilitic child in a second marriage, with a husband who never had the disease.[1]

With regard to the communication of secondary syphilis, in relation to Mr M'Gillivray's theory, Mr Paget, of St Bartholomew's Hospital, makes an important suggestion. "I would venture to suggest," Mr Paget writes me, "that you should try to find whether ever a woman derives secondary syphilis from her husband, *unless she conceives by him*. Facts bearing on this point might prove, that secondary syphilis is not communicated directly by the seminal fluid, but by the child begotten with it; and this mode of inoculation being proved would go far to prove the foundation of your [Mr M'Gillivray's] theory." I fear it will turn out, on inquiry, that secondary syphilis may be transmitted directly by the seminal fluid, independently of conception; but perhaps it may appear also, that its transmission in this way is *occasional* only and *uncertain*, while it is *very frequent*, or almost *inevitable*, when conception follows intercourse. And a comparative observation of this kind, if clear and undoubted, would be nearly equally decisive.

(3.) With regard to the singular observation made by the Count de Strzelecki, and referred to in the appendix to my former paper, —and from which it appears that the aboriginal female of certain countries, after fruitful intercourse with the European, becomes

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[1] Since the publication of my former paper, I find that Dr Montgomery of Dublin has been beforehand with me in this question as to syphilis; and that he seems virtually, though obscurely, to enunciate the doctrine of the *constitutional* character of the phenomenon exemplified in Lord Morton's mare and Mr Western's breed of pigs. Referring to these well-known cases, Dr Montgomery remarks:—"Such occurrences appear forcibly to suggest a question, the correct solution of which would be of immense importance in the history and treatment of disease. Is it possible that a morbid taint, such as that of syphilis, for instance, having been once communicated to the system of the female [by a conception], may influence several ova, and so continue to manifest itself in the offspring of subsequent conceptions, when impregnation has been effected by a perfectly healthy man, and the system of the mother appearing to be at the time, and for a considerable period previously, quite free from the disease? My belief is certainly in favour of the affirmative."—*Exposition of the Signs and Symptoms of Pregnancy*, p. 18.

sterile to the male of her own race, though retaining her fertility with the European,—the Count informs me, *that he has never met with an exceptional case,—i. e.*, to use his own words, "it has not come under his cognizance to see or hear of a native female, which, having a child with an European, had afterwards any offspring with a male of her own race." And I am assured by Professor Goodsir and Dr Carmichael, of Edinburgh, and by Dr Maunsell, of Dublin, that they have learnt, from independent sources, that, as regards the aborigines of Australia, Strzelecki's statement is unquestionable, and must be regarded as the expression of a law of nature.[1] If it is such, the fact still seems to me to indicate rather a constitutional than a local affection of the female system, and to fall within the compass of Mr M'Gillivray's theory; to be referable to some influence exerted by the European blood, through the medium of the fœtus in utero, on the general system of the female; and to be analogous in its degree to the constitutional effect of the vaccine virus, and of the specific poisons producing small-pox, scarlatina, measles, typhus, &c.,—the one rendering the system incapable, as a general fact, of being again affected by those poisons; the other incapacitating the ova from being again fertilized by the native semen.

Assuming that the fact is truly a law of nature, and that it holds as absolutely and extensively as Strzelecki's experience would lead us to infer, "it is," as Professor Goodsir observes, "a very remarkable one, and indicates a series of influences of high import in the natural history of the human race." What that import is, it may not be easy to comprehend, nor perhaps is it a short line that will fathom it. But it seems to indicate, how little account soever might be taken of it by the author of "The Vestiges of Creation," that there is in nature a principle of *degradation*, as well as a principle of *development*. And it may not be altogether out of place here to remark, that it may form part of the plan of Providence that certain races of men should hold given portions of the earth's surface till certain other races, and in particular our own Anglo-Saxon race, are ready to step in and occupy them,—those primitive races then disappearing; and that the law in question may be directly subservient to the extermination of these. The rapid increase of the Anglo-Saxon race during the last two centuries, its wide diffusion over the globe, and its superiority over every race with which it has come in contact, are in harmony with that supposition. It has recently been stated, with regard to this race, that while in

[1] "The intercourse of Kamehamha's men [people] and that of the whale ship's [manned by Europeans], which now began to anchor in their waters, was sadly disastrous to the native constitution and morals, poisoning the fountains of health, and inducing premature decay and *barrenness*." This observation, though by no means very definite, seems to point to the fact alluded to in the text. See an article, entitled "On the Sandwich or Hawaiian Islands," in the "New York Biblical Repository and Critical Review," for July 1849.

1620 it numbered only about six millions, and was almost exclusively confined to the United Kingdom, it now numbers sixty millions of human beings, planted upon all the islands and continents of the earth, and fast absorbing or displacing all the sluggish or barbarous tribes on the continents of America, Africa, Asia, and the islands of the sea; and that, increasing everywhere by an intense ratio of progression, it is estimated that, if no physical revolution supervene to check its propagation, it will number eight hundred millions of human beings in less than 150 years from the present time.[1]

It seems tolerably certain that Strzelecki's law does not extend to the Negro race—the fertility of the Negro female with the male of her own race not being apparently impaired by previous fruitful intercourse with the European male,—a kind of intercourse which is notoriously common in all the West India Islands, the Brazils, and the slave-holding States of North America. And for this immunity the reason may be, a designed appropriation of the Negro race to the cultivation of those regions of the earth, whether in subserviency to the European or independently of him, where, by reason of the high temperature, the European cannot toil in the way or to the degree which that cultivation requires, but where also, but for that provision, the intercourse between the two races might ultimately end in the extinction of the Negro. But it is yet, I apprehend, undetermined, and it must surely be interesting to ascertain whether the law applies to the Mongolian and pure Malay races, inhabiting China, Japan, Borneo, and the islands of the Eastern Archipelago. And, if it should, one cannot help considering, in connection with it, the footing which Britain has of late acquired in those regions,—the probable rapid increase of our countrymen there and in Australia, Van Diemen's Land, and New Zealand,—and the Anglo-Saxon *nation* rising up as with mushroom growth in California, to overspread the western coast of America,—and asking whether, through the instrumentality of that law, the native inhabitants of those parts of the East may not yet be exterminated?

But our knowledge of this law must be far wider and more precise than it yet is, to enable us confidently to speculate in regard to it. We do not even know for certain that it holds absolutely over the races specified by Strzelecki, or that the Negro race is wholly exempt from it. It is conceivable, in regard to the former, as Strzelecki himself frankly allows to me, that there may be exceptions to it, though he has met with none; and, in regard to the latter, that it may be in some degree subject to it. It is well known, indeed, that in the West India islands, under the system of slavery at least, the black population tends rather to diminish than increase, and that

[1] American Paper. Quoted from "Chambers' Edinburgh Journal," for July 1850.

it can only be adequately maintained by continual importations from Africa. Whether that diminution is in any degree owing to the operation of Strzelecki's law, it is perhaps at present impossible to say.[1]

(4.) In my former paper, I represent Mr M'Gillivray as holding that there is a direct vascular connection, and a continuous interchange of blood, between the fœtus in utero and its mother, and also as regarding that sort of connection to be essential to the validity of his theory of inoculation. Mr M'Gillivray, however, as I have since discovered, does not hold any such opinion, nor does he rest his theory on so insecure a basis as I fancied. His ideas regarding the structure of the placenta, and the connection between the mother and fœtus, appear, in fact, to be substantially those suggested by the late Dr John Reid and others, and now commonly received.

The mistake committed by me arose from some expressions dropt by Mr M'Gillivray in the course of his paper, taken in connection with a case given by him, and showing an *empty* state of the blood-vessels of a fœtal calf, whose mother had died of a hemorrhage from the lungs, of three days' continuance. As this case seems to me to afford a stronger support to his theory than I at first imagined, and to be besides (though not unique[2]) a valuable contribution to our physiological knowledge, I shall introduce it here, and take occasion to offer some remarks upon it :—

A cow in calf, and past the ordinary period of utero-gestation, had, from some unknown cause, a slight rupture of the left lung. A constant hemorrhage from the fissure proved fatal in three days. On a post-mortem examination, it was found that the hemorrhage had taken place partly into the cavity of the chest, and partly into the air-passages of the lung. A great part of the blood that entered the lung had found its way upwards, and, being swallowed by the animal, was passed along with the egesta. The animal survived until but a very small quantity of blood remained in the carcass.

The uterus, with its contents, was removed entire, and very carefully and minutely examined by Mr M'Gillivray. All the vessels of the chorion, amnion, &c., were white, flaccid, and empty. On making a section of the umbilical cord, no blood followed ; on applying a sliding squeeze on that portion of the cord, and in a direction from the fœtus, slight traces of blood appeared at the cut ends of the umbilical arteries, but there was no flow, not even drops. There was no blood in any part of the aorta or vena cava ; none in the carotids or jugulars near the head ; none in the external or internal iliac arteries, and large veins in the ilio-femoral region ; none in the left ventricle of the heart—the right containing a small coagulum which might amount to about half an ounce of blood. In short, there were not three ounces of blood in the calf, taken with all its membranes and placenta.[3]

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[1] It may be mentioned, in connection with this, that it has been stated to me, though not on sufficient authority, in regard to the *mare*, that, though not rendered sterile to the horse, she does not readily conceive by him after breeding with the ass, with whom she continues very fertile.

[2] There is a case, by Mery, in the "Mémoires de l'Académie Royale des Sciences," 1708.

[3] Abridged from Mr M'Gillivray's paper in Aberdeen Journal, for March 21, 1849.

On the perusal, formerly, of this case, I made but little account of it; and being impressed with the unqualified allegation of various physiologists, that, "when a pregnant animal dies of hemorrhage, the vessels of the fœtus remain full of blood,"[1] I too hastily concluded that some fallacy must attach to it, or at least that, however it might be explained, it was too slender a foundation whereon to rest the doctrine (erroneously assumed as that intended) of a direct vascular communication.

Mr M'Gillivray's real object in adducing the case, was to combat the opinion entertained by some physiologists, that, while the fœtus receives supplies from the mother through the placenta, it returns nothing from its own system to hers. "I am quite aware (he says) that many physiologists maintain that, in the highest species of animals, the blood cannot be returned from the fœtus to the mother during utero-gestation." That this opinion is very generally held by physiologists in this country, is quite certain. Dr Alison, for instance, after observing (on the authority of Magendie and of Dr David Williams, of Liverpool,) that camphor and oil injected into the blood of pregnant animals are soon detected in the blood of the fœtus; but that poison, injected into the umbilical arteries, although mixing with the blood on its way from the fœtus to the placenta, does not affect the mother; and that fatal hemorrhage in the mother does not apparently diminish the fulness of the vessels of the fœtus, —adds, "so that it would seem that the transmission of fluids is almost entirely from the mother to the fœtus."[2] Again, Dr Kirkes, referring to Professor Goodsir's observations as to the intervention of two distinct layers of nucleated cells between the fœtal and maternal portions of the placenta, speaks of the one being "probably designed to separate from the blood of the parent the materials destined for the blood of the fœtus," while the other "probably serves for the absorption of the material secreted by the other set of cells, and for its conveyance into the blood-vessels of the fœtus,"[3]—no idea, seemingly, being entertained of a converse process. Moreover, the view taken by most physiologists of the destination of that portion of the fœtal blood which is transmitted to the placenta, appears to be exclusively that of *renovation* or *aeration*, by coming into relation with the oxygenated blood of the mother,[4]—nothing being said as to *re-absorption* into the maternal system.

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[1] Magendie; Physiology by Milligan, 2d edit. p. 509. Dr D. Williams, Edin. Med. and Surg. Journal, vol. xxv. p. 102.

[2] Outlines of Physiology, 3d edit. p. 426.—In his History of Medicine, Dr Alison expresses himself even more strongly on the subject:—"The experiments of Magendie and others have proved that any substance which may be circulating in the blood of the mother finds ready access to that of the fœtus, but that there is little or no transference of fluids in the opposite direction."—Cyc. of Pract. Med., vol. i. p. lxxxiii.

[3] Handbook of Physiology, p. 643.

[4] Carpenter, Principles of Human Physiology, 2d edit. p. 718. Manual of Physiology, p. 474.

Mr M'Gillivray brings forward this exsanguine fœtal calf to depone in opposition to these exclusive views, and by its testimony to prove that of the blood which returns to the placenta, a *portion* is *absorbed* by the uterine veins, and re-enters the circulation of the mother. That it furnishes "undeniable" evidence of this (as he alleges) is more than can fairly be affirmed, because it is quite conceivable that, without giving back to the mother a single drop of its blood, the fœtus may, during the three days, have simply used up the blood within it. Whether this happened it is difficult to say. But it may be questioned whether the vital actions of the fœtus would not have failed long before its blood was actually expended to this extent; and it seems altogether much more probable that the nearly complete disappearance of that fluid was owing *in part* to its being abstracted by the mother.[1] That, under the circumstances in which the mother's system was placed during the continuance of the hemorrhage, a powerfully *derivative* effect had been produced on the general mass of her blood, withdrawing a large part of it from the several organs (including the maternal portion of the placenta) towards the seat of the hemorrhage, seems very certain from the observations of Haller and Spallanzani; and that her general function of absorption had been unusually active, the well-known experiments of Magendie sufficiently demonstrate. And we should scarcely conceive, *à priori*, either that the blood of the fœtus had not been brought within the sphere of action of those two influences, and, unless an obstacle interposed, been laid under contribution for the support of the mother's system; or that the double layer of nucleated cells, intervening between the maternal and fœtal vessels, and which habitually allows the passage of fluids from the mother to the fœtus, had then offered any impediment to their passage in the opposite direction.

The discrepancy between Mr M'Gillivray's case, and the statements of Magendie and Williams, as to the *fulness* of the fœtal vessels when a pregnant animal dies of hemorrhage, is probably only an apparent one. Those statements have reference to cases where the mother dies *suddenly* from *profuse* and *rapid* loss of blood, and where the emptying of the fœtal vessels in the only way in which they can be drained, viz., by absorption, is *anticipated by death;* while in Mr M'Gillivray's case, the hemorrhage being comparatively gradual, *time was given* for the exercise of that process, and circumstances concurred to render it *unusually active.*

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[1] This inference will, perhaps, appear the more probable, if it is considered that the supplies from the mother to the fœtus could scarcely wholly fail at once; and that the fœtus must, therefore, on the other supposition, have expended on itself not merely the blood within it at the time the hemorrhage began, but such farther supplies, also, as it continued to receive, up, perhaps, till within a short period of the mother's death. Nor is there anything incongruous in the supposition, that the fœtus should at once draw supplies from the mother, and be drawn upon by her.

In the absence of any *experimentum crucis*, whereby a positive solution of the question might be had, the anæmic condition of the fœtal calf may, I think, with great probability be ascribed, in part, to the expenditure of its blood in the nutritive processes going on within it, and in part, as Mr M'Gillivray supposes, to the re-absorption at least of its more fluid constituents into the system of the mother. And what seems to have been thus possible, and, under the circumstances, may very fairly be presumed to have actually occurred, and to have taken place pretty rapidly, may reasonably be held to imply, that, at all times, though ordinarily but slowly, there goes on within the gravid uterus a gradual removal by absorption, and reception into the maternal system, of such portions of the fœtal blood as are unfitted by reason of impurity for ministering to the nourishment of the fœtus, and for the *renovation* of which the placenta may be inadequate. It is not probable, indeed, that the amount returned is very great, and the experiments of Magendie are sufficient to demonstrate that it can be but small. If, however, it take place at all, be the quantity absorbed within a given time never so inconsiderable, it is all that is necessary for the support of Mr M'Gillivray's theory,—the essential principle of which cannot, perhaps, be better expressed than in the words applied to signify the influence of moral causes on human character—"a little leaven leaveneth the whole lump."

Wagner, indeed, expresses a decided opinion that a *mutual* interchange of fluids takes place in the placenta,—"*the blood of the mother abstracting matter from that of the fœtus*, and the blood of the fœtus taking in its turn matter from that of the mother."[1] Müller, also, is of the same opinion. "In mammalia, the vascular villi of the fœtus are received into the vascular sheaths of the uterine placenta, so that the capillaries of the fœtal and those of the maternal system come into contact with each other, *and suffer an interchange of the matters which they contain*."[2] Neither Müller, however, nor Wagner refer to any facts in support of their opinion. Probably they rest it on analogical grounds. And, indeed, all that seems necessary to give confirmation to it, is proof that poisons introduced into the umbilical arteries, or into the body of the fœtus, may pass into and affect the mother. That Magendie's experiments failed to show this, is well known; but, valuable as they may be allowed to be, they do not demonstrate that such absorption may not take place,—still less, that it is impossible. The only negative inference that can be drawn from them is, that within the time occupied by them, and under the circumstances in which they were performed, no obvious

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[1] Elements of Physiology, translated by Willis, p. 202.

[2] Elements of Physiology, translated by Baly, vol. ii. pp. 1604–5.—Dr John Reid makes an observation which seems to convey the very same idea:—"The blood of the mother contained in the placental sac, and the blood of the fœtus contained in the umbilical vessels, can readily *act* and *react* upon *each other*."—*Physiological, Anatomical, and Pathological Researches*, p. 326.

absorption occurred. And it is conceivable that they may have been too few in number, or too little varied,—nay, even too unskilfully conducted, to be of much value, and may demand repetition; or, that the circumstances under which experiments of this kind must necessarily be made, are such as to preclude the hope of our ever obtaining satisfactory information through this channel. Yet, even in this event, we may yet obtain the requisite satisfaction. Possibly, nature may herself yet furnish us, in her own way, with the evidence which we cannot extract from her by artificial methods of interrogation. Had we, *e. g.*, undoubted instances of the fœtus being tainted with syphilis through the father, and communicating the virus to the mother, we should have all the proof we need desire that matters can and do pass by absorption from the fœtus to the mother. Whether such evidence shall yet be had, remains to be seen; but it may be observed, that Mr Paget has suggested the criterion by which the value of alleged instances of this kind may be determined.

(5.) It will be necessary to defer, till after consideration of the next division of the subject, the several suggestions to be offered as to the conducting of experiments in breeding on the lower animals, with the view both of testing the principle of *inoculation*, as held and applied by Mr M'Gillivray, and, if a sound one, of giving greater breadth and precision to our knowledge of it. Meanwhile, assuming provisionally that it is a sound one, I would venture to submit, in illustration of it, whether, as suggested to me by Dr Carpenter, the agency of the fœtus on the system of the mother is not *dynamical*, rather than material or organic,—operating, in the first instance, on the vitality of the maternal blood, and through it on the vital powers and susceptibilities of the maternal solids? Such, I apprehend, is all that can be imagined of the influence exerted on the living human body by vaccination, or by an attack of small-pox, measles, scarlatina, &c., whereby an almost perfect security is given against the agency of their specific causes in all time coming,—in explaining the *modus operandi* of all or any of which influences, it may be confidently affirmed that the test-tube, the balance, and the microscope, will afford us no assistance whatever.

(*To be continued.*)

ARTICLE IV.—*On the Treatment of Ascites by Diuretics applied Externally.* By ROBERT CHRISTISON, M.D., V.P.R.S.E., Professor of Materia Medica in the University of Edinburgh.

SOME years ago a short paper appeared in a French periodical, on the treatment of ascites by means of digitalis and squill applied outwardly in the form of liniment; and three cases were given, illustrating the diuretic and curative effects of this treatment, after failure by means of diuretics administered in the usual way, as well as by

powerful purgatives.[1] At the time, I had under my charge a boy, ten years of age, who had laboured under simple ascites,—that is, without any œdema, even in the limbs,—for a period of five or six months, during which the effusion had slowly and steadily increased, notwithstanding the employment of brisk purgatives, various powerful diuretics, and mercurial action. There was great difficulty in referring this affection to any particular organic disease; but, on the whole, a previous chronic peritonitis was suspected to be the cause. When he came under my care, purgatives and diuretics internally were again tried, and especially digitalis and squill, but with no better success than before. On the contrary, the enlargement and tension of the belly became gradually very great, so that the boy was confined in a great measure to bed. The French suggestion came therefore most opportunely. A mixture of equal parts of tincture of digitalis, tincture of squill, and tincture of soap, was rubbed freely and diligently into the skin of the belly morning and evening. At an early period,—so early, if I do not mistake, as the beginning of the third day,—the urine began for the first time to increase; by and by a copious flow was established; the ascites quickly subsided, and in about fourteen days entirely disappeared; the boy at the same time gained flesh and strength under the use of simple bitters and chalybeates, and ere long his health was completely restored. When I last heard of him, four or five years afterwards, he continued well and strong.

The same treatment was tried afterwards in several cases more or less similar to this, but for a time with invariable disappointment. In all, however, the more ordinary measures had previously been exhausted to no purpose; and in most of them decided organic disease of some abdominal organ was ascertained to be the cause of the effusion. At length the following remarkable case revived the hopes excited by the first experiment. A boy, about nine years of age, had been affected with enlargement of the liver and ascites for nearly a twelvemonth. Dr James Duncan, his medical attendant, had employed all the most serviceable remedies, but to very little purpose. Mercury, iodine, purgatives, diuretics, both vegetable and mineral, had been faithfully given, occasionally with partial effect, but not with any satisfactory amendment. At length the ascitical distension became enormous, the integuments acquired a glistening translucent appearance, the respiration was impeded, and the tightness of the skin prevented the nature and extent of the disease of the liver from being ascertained. But there was no œdema, even in the limbs. In this state I saw him for the first time. Before resorting to the temporary expedient of tapping, it was proposed to make trial of the diuretic liniment; but the integuments were so irritable, that the method by friction could not be persevered with. Dr Duncan, however, proposed to substitute a strong infusion of digitalis, four times

[1] I have repeatedly endeavoured, without success, to recover the reference to this paper, and the name of its author.

the pharmaceutic strength, and to apply it continuously by linen cloths covered with oil-silk to impede evaporation. In a few days diuresis set in, and a profuse flow of urine quickly removed the whole dropsical effusion. The liver was soon felt to be greatly enlarged, lobulated, and rugose; which disease, it need scarcely be added, proved fatal a few months afterwards. But it is remarkable that the ascites never returned.

I have often used the same method since, both in simple ascites, and likewise where that affection formed a prominent part of a more general dropsy. The issue has of course been variable, and more frequently unfavourable than successful. But on the whole the results of my own observation, and the experience of others in Edinburgh, who have also tried the practice, lead to the conclusion, that digitalis not unfrequently succeeds in this way, when not only it, but likewise all other diuretics, taken internally, as well as the purgative method of cure, prove of no avail. It has likewise been tried with success in a few cases of obstinate excessive œdema of the limbs, in connection with general anasarca and Bright's disease of the kidneys, after diaphoretics, purgatives, and diuretics internally, had failed to give relief.

The late invention of the texture called Spongio-piline has facilitated the application of this treatment. A large sheet of spongio-piline soaked moderately with a strong infusion of digitalis, made with one ounce of powdered leaves to twenty fluid ounces of boiling water, may be applied to the whole abdomen, or to each limb, and worn constantly without any material inconvenience to the patient. I lately met with a case of Bright's disease, in the University wards of the Royal Infirmary, in which considerable general anasarca, developed to a great extent in the limbs, and concurring, as sometimes happens, with a natural diuresis, was for a time reduced almost entirely by the soaked spongio-piline applied to both legs, although every other customary remedy had been administered unsuccessfully. In another case, that of a dissipated middle-aged woman, who presented all the characters of the variety of Bright's disease which is supposed to depend on chronic inflammation of the kidneys, diuretics given internally removed almost entirely the general anasarca, after diaphoretics had completely failed; the patient thereupon was speedily relieved also of an obstinate chronic bronchitis, liability to vomiting, and tendency to diarrhœa. But the ascitical effusion, which had been predominant from the first, was not reduced at the same time. On the contrary, it went on increasing till the integuments became tense. The foxglove infusion was then applied by means of the spongio-piline, with the effect of augmenting the flow of urine in a few days; and then the ascites decreased, at first quickly, afterwards more slowly, till at length all dropsical effusion had disappeared, so that the woman, after seven months of treatment, left the hospital in a fair state of health to outward appearance, and with the urine much less albuminous, and much less loaded with the debris of tube-casts and epithelial cells.

I have tried to ascertain the description of cases in which this treatment is most likely to prove successful, but without any satisfactory result. The pathology of ascites indeed is still not sufficiently understood to allow of an accurate inquiry as to the influence of remedies. Simple ascites, or that along with œdema of the legs and lower half only of the trunk, is known to be a symptom, or rather a sequela, of various organic diseases, such as existing or previous chronic peritonitis, cirrhosis and other organic derangements of the liver, enlargement of the spleen, diseased pancreas, enlarged mesenteric glands, malignant disease of the abdominal lymphatic glands near the great abdominal vessels, or of any other organ or texture so situated that the tumour thus arising exerts pressure on the great veins. But the physician cannot always distinguish during life, among all these causes, what is the real source of the dropsical effusion; and, on the other hand, several at least of these organic diseases are known to occur not unfrequently without being attended with ascites at all. In these circumstances it is not surprising, that a difficulty is encountered in ascertaining the description of cases in which the ascites is most likely to yield to a particular mode of treatment.

An analogous method, by means of a poultice made with the *Marchantia hemispherica*, or common liverwort, was recommended in 1833 by the late Dr Thomas Shortt of this city, for the treatment of dropsies in general.[1] I have not found this remedy so generally serviceable as Dr Shortt seems to have expected, and his method of applying it is irksome to the patient.

Article V.—*On Uterine Hemorrhage.* By George Hamilton, M.D., Falkirk.

In the July Number of the Edinburgh "Monthly Journal of Medical Science," at the conclusion of a short extract from the *Journal de Médecine*, giving an account of a new vaginal plug, invented by Dr Gariel, of Paris, it is stated, that "a similar application of caoutchouc, as a simple and perfect vaginal plug, was, we know, made some time ago by Dr Hamilton, of Falkirk."

As this allusion gives no practical information, and as a short description of the instrument mentioned will easily enable its merits to be tested, I have thought that an account of the case, in the treatment of which I had to get it constructed, may prove not unacceptable to the profession.

In July, 1849, I prescribed for a lady in this neighbourhood, who had been subject for some months to a bloody vaginal discharge, which, however, she was very averse to have stopped, as she thought it gave relief to headachs, to which she had long been subject. In October following I again saw her, and,

[1] Edin. Med. and Surg. Journ. xxxix. 129.

feeling much alarmed at the state of exhaustion in which I found her, I with some difficulty prevailed on her to allow Professor Simpson to be called in. On making an examination, the Professor was immediately satisfied that there existed extensive malignant disease of the neck of the uterus, and there was found ulcerative destruction of its anterior portion, of a size sufficiently large to admit nearly the whole of the first joint of the index finger. From this, there could be little doubt, the hemorrhage proceeded.

The nature of the case rendered any other than palliative treatment impossible. The principal circumstance to which attention had to be directed was the hemorrhage, which had now continued, with short interruptions, for nearly five months, and had been latterly increasing considerably. The Professor recommended that this should be restrained by the introduction of pieces of sponge, and he introduced one piece into the os uteri and ulcerated portion of the uterus, and another into the vagina, to retain the first in its proper place. He also recommended that the sponge should be dipped in the decoction of matico, or in a solution of tannin or gallic acid, in case this should be found necessary.

At my visit on the following day, I found that the sponges had restrained the hemorrhage for only a few hours, and that a considerable quantity of blood had since then been lost. I applied fresh sponges dipped in matico, using also a larger piece for the vagina, with a like result. After some further trials, I abandoned the sponges, and used rags saturated with astringents, the vagina being regularly "plugged," and the os uteri and the ulcerated portion being carefully stuffed with charpie or rags, but with little better success; and, after several days unavailingly employed in this way, I became seriously apprehensive that the hemorrhage would cut off my patient.

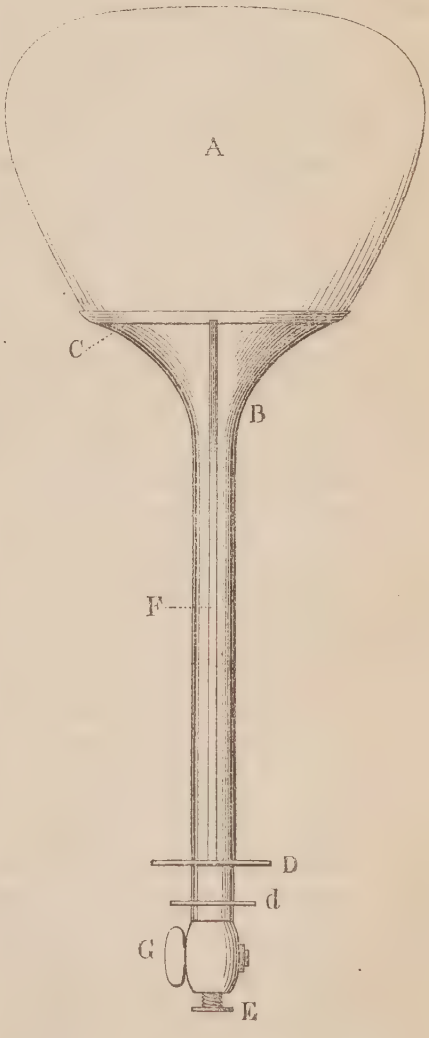
Under these circumstances, I employed the following simple means to stop the hemorrhage. I got a turner to shape me a piece of wood into nearly the exact form of Dr Weir's pessary,[1] making it three and a-half inches long. I converted this into a tube, by getting a hole bored through its length, and into its smaller extremity I fixed with shell-lac varnish a small stop-cock, such as is usually fitted into hydrocele canulæ. On the cup of the instrument I fixed a bit of sponge, and over both tied tightly to the upper part of the stalk (which slightly bulged below this, to prevent the thread slipping) the fundus of a strong pig's bladder. On the opposite extremity there were two buttons, or knobs—a smaller and a larger one—to give facility in attaching by one hole four straps of vulcanised India rubber, such as are used in gentlemen's braces. Before using the instrument, thus prepared, in the case I have mentioned, I introduced a piece of charpie, dipped in an astringent, large enough to fill the ulcerated portion of the uterus. The air was then squeezed from the bladder through the stop-cock, the stop-cock was turned, and the collapsed bladder introduced into the vagina, and, when there, about an ounce and a-half of water were injected from a syringe, through the stop-cock, into the bladder. When fully distended, the bladder was about the size of half of a small orange. The instrument was then gently pushed up against the neck of the uterus, and was retained in its place by tapes, connected with a band passing round the waist, which were also attached to the straps of India rubber.

In this way, after a very little practice, I found that the hemorrhage could be nearly perfectly restrained, by so adjusting the tapes as to direct the pressure from behind forwards. It constantly threatened to return, however, for nearly four months, whenever the instrument was for a few days dispensed with; and, on more than one occasion, to such an extent as to have nearly proved suddenly fatal before I could be in attendance to apply it. To prevent this danger, I taught the nurse how to apply the instrument, and she, for the most part, afterwards applied it when necessary, with the greatest ease, a few

[1] For a figure of Dr Weir's pessary, see page 198 of this Journal for September 1848.

hours generally sufficing to moderate the attack. This proved a great boon to my patient, as the protracted use of the instrument caused considerable irritation by excoriating the posterior commissure of the vagina. It could not be wholly dispensed with for nearly four months. After this there was no hemorrhage, but a large amount of fœtid discharge, under the effects of which, after several months of distressing exhaustion, the patient sunk.

The principal defect in the extemporaneous instrument, which I used in this protracted case, was the continual giving way of the bladders, which never lasted longer, when continuously used, than thirty-six hours, and generally gave way before this. Even when this did take place, however,—and when, of course, the water escaped,—I found that the bladder, and included sponge, alone acted as a pretty good plug. To remedy the defect mentioned, I wrote to Messrs Macintosh and Co., in Manchester, and got them to make for me a vulcanised India rubber bag, which, when attached to a silver stalk, forms the instrument referred to in the July Number of this Journal. Some delay took place before a properly shaped bag could be made; and, by the time this instrument was completed, the hemorrhage in the case narrated had ceased, and I have not since had another case in which I required to use it. It is essentially the same, however, in form, as the one which was used.



The annexed is a drawing of the instrument in its improved form,—reduced in size, however, nearly one-third :—

A is the inflated bag of vulcanised India rubber, which is attached to a silver tube at B with a silk thread, the cup part of the tube being passed into its interior. The vulcanised bag is prevented falling down by a circular wire C, which is fixed to three upright ones. One of these, F, is seen in the drawing. They are soldered to the stem at its lower portion. D d are the two buttons, between which are fastened the vulcanised straps. G is the stop-cock; and E is a screw which fits into the mouth of the tube, to guard against an escape of the fluid. Either air or water may be used for inflating, but water will be found less liable to escape. For more easily inflating, I ordered a vulcanised bag, similar to those used as breast-drawers, which I intended should be attached to a screw that was

made to fit into the tube of the instrument; but the bag sent me was unsuitable for the purpose. A common syringe, however, answers very well when water is used, or a breast-drawer could easily be adapted to the screw in the tube. Vulcanised India rubber is the best material I know of for constructing a bag such as I have described, but even it decays when exposed to fœtid discharges for some time. A bag of the size and shape of the one shown in the figure can be had by any one, by writing to Messrs Macintosh and Co., who have the pattern from which mine was made.

Such an instrument as has been described will be found, I apprehend, extremely useful in those cases where the "plug" is indicated. It is more easily applied or taken away than the means usually employed,—it is more effectual,—and, what is of the greatest importance in protracted or obstinate cases, it can with ease, when necessary, be applied by any nurse, or even by the patient herself. Compared with the instrument of Dr Gariel, it appears to me to possess these advantages:—1st. It does not unnecessarily distend the vagina, in order to produce pressure on the os uteri, and hence avoids the risk of "bearing down," and consequent displacement or expulsion, which I have found in such cases apt to take place when I have used differently shaped bladders for the same purpose. 2d. It allows a smaller or greater amount of pressure to be applied at pleasure, in the exact direction, and on the part, where it is required, without unnecessarily affecting the surrounding parts. Altogether, it appears to me to be a more precise and effective instrument than that of Dr Gariel.

At page 192 of this Journal, for February 1850, in giving an account of apparently the same instrument as has more lately been said to have been invented by Dr Gariel, it is remarked by the editor, that M. Diday's "proposed application of this plan to the arrestment of post partum hemorrhage, we are sure will, if tried, lead only to disappointment and danger. The application of the bladder to the mouths of the open uterine sinuses, will have no effect in stopping hemorrhage in an organ so capable of distension as the uterus after delivery."

The objections to the use of such an instrument as Dr Gariel's, in post partum cases, seem sufficiently obvious. I am not quite sure, however, that such an instrument as the one I have described, may not be found usefully to assist, in certain cases, a practice which I have adopted, with uniform success, in post partum hemorrhage, for the last twelve years. As I do not recollect of having seen the same practice recommended by any author in these cases, and as it is connected with the application of the instrument I have described, and is, it appears to me, important in itself, I shall here shortly mention it. In the treatment of post partum hemorrhage, I first, as is usually done, introduce the right hand into the uterus, and rapidly clear it and the vagina of blood and clots; but I do not then, as is usually the case, withdraw my hand from the vagina. On the contrary, I

retain it there, and grasp and press together the mouth and as much of the neck of the uterus as I can lay hold of, at the same time that I grasp, with my left hand, the body of the uterus, through the abdominal parietes, and push it downwards into the pelvis. Between the two I find I can compress the uterus, so as, merely by mechanical means, to defy hemorrhage. At least this has happened in every instance where I have adopted these means hitherto; and some of the cases have been of a very trying character. The application of the mechanical pressure for from five minutes to a quarter of an hour has generally sufficed, but occasionally I have had thus to grasp the uterus for nearly an hour. Of course, where the case has threatened to be in the least degree obstinate, cold cloths to the abdomen, and the other usual means, have been employed at the same time.

I have never had occasion to use such an instrument as the one of which a drawing has been given in such cases, but it lately struck me forcibly, while treating a case in which the patient was very stout, and in which I had to press the uterus against the pubis with both hands, while in the position mentioned, before the mechanical pressure stopped the hemorrhage, that this instrument might be useful in extreme cases. If we suppose a case in which the uterus continues flaccid, and in which hemorrhage is going on, notwithstanding the employment of all the usual means, I do not see anything to prevent the introduction of such an instrument as this into the uterus. Sufficient mechanical compression to restrain the hemorrhage might, it appears to me, without difficulty, be applied, by pressing the stalk upwards, with the right hand, in the vagina, while the left grasped the uterus externally; and should a general pressure over the uterus not prove sufficient, I imagine it would be found an easy matter to vary the direction of the pressure with the instrument internally, until the bleeding sinuses were found to be compressed. In such cases, one great advantage of retaining the hand in the vagina I have found to be, that the practitioner is constantly able *directly* to satisfy himself whether or not any hemorrhage is taking place, and consequently is able to regulate his means for restraining it, without waiting to take his indications from the effects produced by it on the general system.

ARTICLE VI.—*On the Adjustment of the Claims at present made by the Medical Profession on the Assurance Companies.* By Professor CHRISTISON.

CONSIDERABLE agitation has arisen in the medical profession within the last few years, in consequence of the relation in which its members have found themselves placed with regard to Life Assurance Companies and those who effect assurances with them. So long as the practice of effecting assurances did not extend widely,

and assurance companies were not particular in their inquiries as to the eligibility of the lives proposed to them, scarcely any medical practitioner felt himself so much incommoded by the references rarely made to him concerning the health of his patients, as to think it worth his trouble to aim at correcting the irregularities committed in the mode in which he was referred to. But within the last twenty years the practice of assuring has extended amazingly in this country; and every year sees it increasing in a progressive ratio. At the same time, most assurance companies have become much more particular in constructing their schedules of queries to be filled up by proposers and their referees; and special care above all has been bestowed upon the certificate which every proposer is required to present from his ordinary medical attendant. The natural consequence has been, that a good deal of trouble, and no little responsibility, have thus been occasionally imposed on medical men, and nevertheless without these purely professional services having been regularly, or even generally, requited. Hence much dissatisfaction has arisen among them, not so much perhaps for actual professional loss,—which cannot have been very material in the experience of any one,—as because of the unprofessional treatment to which they conceive themselves to have been subjected. This dissatisfaction has been all the greater, that there has been mixed up with the cause of it another ground of discontent,—which, however, is totally distinct and unconnected,—namely, the circumstance that the rejection of a proposal on the ground of ill health, unsound constitution, or doubtful habits, has not unfrequently created misunderstanding between the proposer and his medical attendant who supplied the information on these points, and has even occasioned irretrievable loss of practice to the latter, who has been discarded by his patient for telling the truth.

Provoked at length by these annoyances, some have brought their grievances before the weekly medical press, by which their cause has been fought with more energy than prudence, and occasionally with more ingenuity than candour.[1]

The tendency of all the propositions which have been hitherto advanced to settle the contest, has been to denounce the assurance companies as alone to blame in the matter; to treat them as selfish traders defrauding medical men of their legitimate gains; to concuss, by individual resistance or general combination, those who will not consent to pay certificate fees; to persuade others more inclined to submission by offering support and patron-

[1] Since these observations were put into the printer's hands, it has been announced in the "Lancet," that an English surgeon has raised an action against an assurance company, for refusing him a fee for his certificate as medical attendant of a proposer. The step appears a bold one, to say the least of it; but at any rate it will, sooner or later, settle the question as one of mere law.

age; and little has been spoken of in any quarter except the determination to have the money. It may be well to have recourse to such measures when other means more consonant with the character and habits of the members of a liberal profession have failed. But other resources of a less doubtful description may be pointed out. The assurance companies have not been addressed by the constituted medical authorities. They have not even been temperately appealed to by any of the special combinations of practitioners, which are said to have arisen in different parts of the country. No advantage has been taken of the advice and influence of the numerous members of the medical profession who act as medical advisers of the companies. These, however, are channels of communication, through which more is likely to be effected than by the efforts of individual opposition, or the censure of anonymous editors. Above all is it necessary, that those who think they can usefully bestir themselves in this question shall first make themselves well and impartially informed on the subject, and escape the one-sided views and obstructive prejudices of those who, unacquainted with the true relations subsisting between the practice of medicine and the system of assurance, will see no fault anywhere but on the part of the companies, and no difficulties not at once surmountable by a liberal board of directors.

It is necessary here to explain what has led me to come forward on this occasion, and to state my views in the present form. It has happened to me to have been not unfrequently consulted, for some years past,—especially by the younger members of the profession,—as to the proper line of conduct for them to pursue when referred to for their testimony by assurance companies in somewhat trying circumstances. In common with many medical friends in this city, I have received repeated complaints of the injustice,—as it is almost invariably called,—of the assurance companies who decline to remunerate the medical attendants of proposers for their professional certificates, and expressions of hope that something may be done at head quarters here to put an end to the grievance. On the other hand, having acted for nearly four years as medical adviser of one of the principal assurance companies in Scotland, the practical bearings of the question between them and my profession have been brought fully before me, and I have become acquainted with the sentiments entertained on the subject by more than one of the companies; and the result has been, that on both sides I have been requested to make the state of the case generally known, under the supposition that a basis of accommodation may thus be established, at all events in Scotland. Let me add, lest my judgment may be thought to have been biassed by my position as an assurance officer, that the views I now entertain are precisely those which were expressed many years before I was so circumstanced,—when I was Professor of Medical Jurisprudence in this University.

When a proposal of assurance is made to an assurance company, the proposer expressly declares that he is sound in health, constitution, and habits. Sometimes indeed there are defects, which he avows, and pays for. But it is unnecessary to consider here any other case than the simple one of a declared unexceptionable life. The company replies that he must produce evidence of the truth of his declaration, and presents him with queries to elicit the information which will be considered sufficient evidence. Among these queries are several which can be answered only by his ordinary medical attendant. In general no further steps can be taken till these conditions be implemented. He is then told that he must appear for examination by a medical man on the company's part; and this gentleman is usually instructed not to proceed before being provided with all the information elicited by the several schedules of queries. This preparatory information, all supplied directly or indirectly by the proposer himself, is the basis of the negotiation. It must be apparent, then, that the *onus probandi*, the duty of proving his soundness, must lie with him alone.

If so, it is equally evident that he must pay for the cost of the proof. The company, besides other means attended also with expense, are at the cost of employing a medical officer of their own to protect them against deception or omission. But it seems a somewhat startling proposition, that they should also be expected to defray, in all instances, the remuneration of the proposer's ordinary medical attendant. Nevertheless, this has been demanded of them almost universally by those who have handled the subject in the medical journals; and on not a few occasions it has been demanded peremptorily as a plain act of justice, that did not admit of a moment's discussion. The companies are rich, it is said, and can easily afford the outlay. They are the party who benefit by the assurance; and they ought to defray all charges. It is for their protection that the certificate of the medical attendant of the proposer is intended; and they ought not to grudge him his fee for his information. He may lose his patient by occasioning the proposal to be declined; and he ought to be reimbursed by the only party who gains in such circumstances.

These arguments are all valid enough, when applied to the companies whose business consists entirely of Mutual Assurance,—whose profits are all divided among the assured in the shape of bonuses; because here the company is neither more nor less than the persons assured. If a fee is to be paid at all, there is really no other party to pay it; and as it matters little whether the assured pay in their individual or corporate capacity, and there is undoubted convenience to all parties that they do so in the latter, the company should charge itself both with the expense and the trouble. The question in their instance is restricted to the narrow one,—whether the duty of the medical attendant of a proposer of assurance is, or is not, one of those public, or semi-public, taxes on his profession, which society has a right to exact from him as an eleemosynary service, or which inve-

terate custom has rendered on all occasions gratuitous. As I have never seen the affirmative side of this question argued, it is unnecessary to waste many words in support of the opposite view. The pretence of some companies of the kind—that they are merely philanthropic institutions—and that medical men should aid them as such, is almost too ridiculous to deserve being controverted. It is absurd to say, that a company, which grows rich by investing capital, and appropriates all its wealth for its own use, is to be treated as a philanthropic society.

The case, however, is different with those assurance companies which divide the profits, not among the assured, but among shareholders. The arguments referred to above do not so clearly apply there. For, in the first place, what can be more preposterous than the averment, that the company is the only gainer by an assurance? Would any man assure his life, if he were not satisfied that possibly all the gain may lie with him? And is he not actually often the gainer in the persons of his children or other representatives? But secondly, admitting that on the whole the company is a gainer by the total transactions—of which there can be no doubt—that is no reason why the company should be charged with the total expense of them,—with the expenses justly chargeable against the purchaser, as well as those which naturally fall on the seller. For if so, why should the company pay only the certificate-fee of the proposer's medical attendant?—why not also that of the registrar who certifies the proposer's age?—why not the government tax too? Or, if the gains of assurance business can be shown to be too great, so as to admit of the proposed liberality, it would be a fairer measure to effect a reduction of rates, leaving the cost of the medical certificate to be defrayed by him who truly owes it. Thirdly, and more speciously, it is urged that the effect of this certificate is to protect the company against loss. But that is a narrow view to take of its object. The primary object is to entitle the proposer to have his proposal entertained. The ultimate result may be, indeed, to prevent the acceptance of an ineligible risk, and thus often to protect the company from loss; but the proposer also sometimes shares that protection. Without an honest certificate from his medical attendant, he may recklessly embark in a contract, which, after a long term of yearly payments, turns out at his death to be invalid; when all these payments are forfeited. On the other hand, with such a certificate, he may, in doubtful circumstances, effect an assurance beyond all cavil. Some years ago I was referred to by a patient, who wished to effect an assurance upon his life, when upon the eve of marriage. But having attended him for epilepsy of two years' standing, which was said to have followed an attack of apoplexy on the Continent, I was obliged to certify that he was "liable to fits." The company, after fully informing themselves as to the nature of these fits, thought proper to accept the risk for a trifling extra premium; the levying of which was of course laid to

my account, and long resented. I need not say that, according to all the rules of medical science, the company acted very imprudently,—as time soon proved. For, after payment of two premiums, the gentleman died in a paroxysm; and, as there had been no concealment, but a full inquiry, his family received the sum assured, amounting to £5000. Had there been no medical certificate, the assurance would probably have proved invalid. This is a strong case. But instances approaching it more or less nearly are constantly occurring, to show that it is not the company alone which is protected from loss by the certificate of the proposer's medical attendant. Lastly, the companies are told they should pay the medical attendant his certificate, as the only way of reimbursing him for the loss of professional practice, when his patient resents the refusal of his proposal by transferring the care of himself and family to another practitioner. Here, too, a very narrow view has been taken of a very serious question. It is surely no fault of the company if a proposer, in such circumstances, turns out spiteful and ungenerous. A certificate-fee would be a sorry compensation for the loss inflicted by his malevolence. Nor is it at all clear that such conduct would be less frequent, were those who contemplate effecting an assurance to understand, that the certificate-fee of their own medical attendant was in all cases to be paid by the assurance company. The hardship incurred by medical men in the instance of such unreasonable patients would be intolerable, were it of frequent occurrence. And unfortunately it is not easy to see how the most willing company can help them to escape it. But this point will be resumed by and by.

It seems clear, then, that the medical attendant's certificate-fee cannot be justly made a charge to be borne by ordinary assurance companies. At the same time, no one can fairly dispute, that it ought to be paid, or that in general it is not paid. Some, indeed, have thought that the medical profession have been too sensitive and particular on the subject;—and I confess to have been at one time inclined to that opinion, on the ground that the utmost which any practitioner could add to his income from such fees must be a very insignificant proportion of it, and that the frequent pressing of his claims on unwilling assurance companies did not greatly tend to elevate the character of his profession in society. But this is an erroneous view. The certification of the state of health and constitution of a proposer of assurance is a highly responsible duty, which may often occasion the practitioner great annoyance at the time, much trouble in after life, and occasionally actual loss. If this duty is not to be paid for, the public may as well require also, that certificates for confinement of lunatics, reports in criminal investigations, medical evidence on criminal trials, and many other purely professional services of the like kind, shall go unacknowledged by the usual remuneration. It is true, that other certificates of various kinds are often granted by medical men without a fee; such, for example, as to patients unable

to serve as jurymen, or public officers incapacitated by illness. But it is also true, that in no other profession are so many professional services, public as well as private, rendered without requital; a circumstance which leads to the conclusion, that a charge of indelicacy or meanness lies much more with those who propose, than with those who resist, an extension of the practice.

Certificate-fees therefore ought to be paid, and they should be paid by those who directly require them, viz., the parties who propose to assure. The only question, then, is, how payment is to be effected. For experience shows that medical men cannot trust for this to the discretion of their patients. Few think of the matter at all; and not a few, when reminded, positively refuse remuneration.

It has accordingly been suggested, that the assurance companies should charge themselves with the duty of levying payment, as one of the expressed items of their first account with the assured. No just reason can be assigned why this should not be undertaken by the mutual assurance companies. But even in regard to other companies, it seems a reasonable proposition; and all difficulties which may be foreseen in carrying out such an arrangement will soon vanish, if the assurance companies will only see their true interests, and all combine together to adopt it.

I am informed that some companies, both mutual and ordinary, have proposed to escape their present embarrassments in another way. Treating the question with coldness, hastiness, or even illiberality, they have spoken lightly of the importance of the certificate of the proposer's medical attendant, ignorantly of the obligations of the medical profession, and sometimes disingenuously of their own position, as if their object was philanthropy, and not a dividend or a bonus. And in order to escape the difficulty, they have contemplated the abandonment, as a general rule, of this certificate, and that they should require it only in particular cases, the number of which would be so small as to allow of the payment of the fees by the companies themselves without any great sacrifice. But this would be a most dangerous practice. In the course of my experience as the medical adviser of an assurance company, I have observed it has been a common occurrence, that every particular of the usual documents was highly favourable until arriving at the medical certificate, which divulged some important defect not likely to be discovered through any other channel. Such defects may, indeed, be sometimes detected during the examination conducted by the medical referee of the company; but by no means always. Many defects depend on previous disease, which leaves no discoverable trace. Many existing diseases, or liabilities to disease, such as epilepsy, delirium tremens, spasmodic asthma, functional affections of the heart, stomach complaints, some hepatic diseases, many diseases of the bladder and kidneys, may present in their early stage, or at long intervals, no external signs to excite suspicion, and still less to prove their existence, during the usual medical examination of the company's referee. Besides, in

numberless other cases, the information supplied by the medical certificate of the proposer's attendant, although it may communicate no new facts, imparts a certainty and consequent security which cannot be attained in any other way. On the whole, I feel confident that the restriction of such certificates to particular cases of difficulty or suspicion, would be almost tantamount to the entire surrender of the principle of selection in the acceptance of lives for assurance.

The principal difficulties which have been stated to me as lying in the way of the companies charging themselves with the duty of levying the medical attendant's fee, are the following:—In the first place, in consequence of the late extension of the practice of assurance to a rank of life in which the encouragement of it is an object of great national interest and importance, many assurances are effected for so small a sum, that a competent medical fee would be a most disproportionate charge upon the proposer, and undoubtedly a serious obstruction to the spreading of provident habits among the well-disposed and provident class of upper workmen and minor shopkeepers. One company in Edinburgh, which transacts principally business of this kind, would be materially restricted in its range, were the medical attendant of the proposer to receive a fee at his patient's expense. I confess there seems no way of obviating this objection except by medical men understanding, that no fee will be expected by them when the sum assured does not exceed a certain amount. Nor is it likely that such an understanding would be found at all difficult to arrive at.

Another difficulty is, that the companies will not find it easy to effect payment of the medical attendant's fee by the proposer, when the proposal is declined. In such a case there is no other payment made, with which this one could be united. There is, besides, an evident awkwardness in asking a man to pay for being declined; and, most certainly, many people so situated would refuse if asked. This difficulty, I fear, cannot be got over, unless the companies see the propriety of conceding that they shall bear the expense when the proposal is rejected. And it appears to me that this much ought to be conceded by them; for there is no other party that so frequently, so largely, and so directly, profits by the divulging of the defects of an ineligible life.

Were these two questions adjusted between the medical profession and the assurance companies, the details might be arranged in other respects easily enough through the medium of the existing medical advisers of the companies; and I may observe, that, from what I have understood, there is a probability that several of the larger companies might be brought to an agreement on such a basis, the result of which would be, that all the minor establishments would soon find the necessity of following their example.

In the event of the assurance companies being induced to come to some arrangement by common consent, I should be sorry were the opportunity lost of also effecting other improvements, by which the position of the medical attendants of proposers of assurance would, I

suspect, be bettered even more than by the payment of their fees. They have a strong claim upon the companies for the adoption of any course which would diminish the risk of misunderstanding arising, as it often does at present, between them and their patients, from the granting of medical certificates. It must always be very mortifying to undergo rejection by an assurance company. It will discompose many to discover that their chance of life is thought worth so little. There are few whose philosophy will protect them from a certain grudge towards the friend who has, though unwillingly, brought the disagreeable fact before them in so plain and practical a shape. Nor is it to be wondered at, that in such circumstances the narrow-minded and selfish wreak their revenge in the only quarter in which it can be exercised. Something, however, might surely be done, to reduce the grievance thus justly felt by most medical practitioners.

When a man of prudence, intelligence, and honesty proposes to effect an assurance, and reflects that he has not always enjoyed uninterrupted good health, the first step he usually takes is to consult his medical attendant, whether anything has occurred in his previous medical history to affect the eligibility of his life. When this precaution is taken—and I know it is often done—it must in general be the practitioner's own fault, if he do not contrive to prevent any unpleasant feeling arising between him and his patient. Now, the companies might do something to extend this practice, and make it general; for example, by introducing a recommendation in their form of proposal, that, to prevent disappointments, intending proposers should first of all consult their own medical adviser. It may indeed appear surprising, that a man, who is conscious of having been at times seriously ill in the course of his life, should shut his eyes to the fact, and require to be told that his chance of longevity has been in consequence impaired. But every physician knows that nothing is more common. It is remarkable how unwilling most men are to think their own chance of life worse than that of their neighbours. And, in defence of their self-deception, it must be admitted, that many are on various accounts kept purposely in ignorance of their exact state by their medical attendants. Cases of self-deception and of heedless proposals of assurance, without consultation of the medical attendant, are particularly apt to occur when the object of the assuring party is not a family provision, but to facilitate a commercial transaction. Few persons reflect dispassionately on their healthiness or chance of longevity in such a conjuncture.—Another precaution is, that assurance agents might with advantage get a hint from head quarters on this subject. The practice of assurance undergoes great extension in country districts, through the judicious activity of a sensible agent. But some, in the excess of their zeal, are not sufficiently discriminating. Hence many cases from country agents, promising enough at the outset, are quashed in the end by the certificate of the medical attendant, who is referred to at the last, instead of at the first, as he might generally be. In country

places an intelligent agent can seldom be at a loss to discover from the medical practitioners around him, in what quarter he may safely take advantage of his opportunities of rendering his company popular.—Another consideration is, that companies should abandon the practice of promising to the medical attendants of those proposing assurances, that the information supplied will be kept secret. It is a well known fact, that, when proposals are declined, such information seldom is, and seldom can be, kept secret; and therefore medical men ought not to be deceived as to this. The certificate should pass through the hands of the proposer; and if there is anything to be really kept secret, it is plain that it must be arrived at in some other way than by queries in a printed schedule.—Another point is, that the schedule of the certificate for the medical attendant ought not to contain any query as to the proposer's habits. Most medical men have an invincible repugnance to certifying their patient's habits, and will not do so. Many excellent reasons could be assigned for this, were it necessary. But, for brevity's sake, these reasons may here be dispensed with; for several companies, in Scotland at least, have in recent times ceased to ask for this information from medical men; and all know that it may be obtained quite as well by the inquiries of their agents and local boards.—Perhaps, too, they ought to omit, as some already do, the general query, which some years ago always terminated the medical certificate, asking the medical attendant's opinion as to the eligibility of the proposer's life for assurance. Medical men now often decline to answer that query, as it is one which requires more information than the medical attendant possesses; and in that case he ought not to be exposed either to the risk of giving an unsound or imprudent judgment, or to the necessity of evading an answer altogether. This, and the previous alteration, would remove a fruitful source of the misunderstandings which are apt to arise between proposers of assurances and their medical advisers. In particular, it is evident that no practitioner can expect to retain the confidence and goodwill of his patient, if he honestly answer a query as to habits, when these are doubtful.

If the points adverted to above be settled by the assurance companies to the satisfaction of the members of the medical profession, it ought on the other hand to be clearly understood by them, that, in return, more care must be taken in answering the queries which may be retained in the certificate-schedule. At present some, through pique, answer in the most barren terms consistent with truth. Others, impelled by the natural desire to please their patient, are apt to forget that the *suppressio veri* is an untruth, though no direct falsehood be committed. Others, and this is a common case, are satisfied with an obscure hint of mischief. Others, while stating an important defect, attach to it no consequence in its relation to health and longevity, yet without assigning a reason. On these, and various other accounts, it is often necessary to appeal to them again for explana-

tions; under which they are, not unnaturally in the present state of things, apt to prove restive. But under a different arrangement, such as is proposed in this paper, they must be prepared to be more particular in their certificate, and also to encounter willingly the subsequent explanatory references, which are usually made to them through the medical referees of the companies.

To many there will appear to be no novelty in these suggestions. But they have been here put together, so far as I am aware, for the first time, and simply in the hope, that they may prove the basis of an accommodation between the assurance companies and the medical profession in their present inconvenient misunderstanding.

Article VII.—*Remarks on Scalp Eruptions.* By Henry M'Cormac, M.D., Belfast.

The difficulty attendant on the diagnosis of diseases of the scalp would render their easier discrimination no ordinary boon to the practitioner as well as to the scientific inquirer. As it is, the general practitioner is too often quite unable to discriminate one form of scalp disease from another, but lumps all up together, let us say under the term porrigo, entailing woful confusion in the issue and treatment. There is, therefore, perhaps, no class of diseases more completely handed over to an ill-discriminating empiricism. When we turn to the systematologists, the case is little improved. French and English, they all vary in their definitions. Either they apply the same names to different diseases, or different names to the same disease. Nothing would be easier than to prove this, were I merely actuated by the vulgar ambition of finding fault.

In the middle ages all, or almost all, cutaneous maladies were termed leprosy,—lepra, of course, among the rest. Of course, also, when every dubious disease of the scalp is termed porrigo or ringworm, porrigo itself will not fail to be of the number. One consequence of this laxity of diagnosis is, that every practitioner has, times without number, cured porrigo—a disease, in reality, very difficult of cure—by means of all sorts of applications. If people could only be got to use their eyes; but every body, though he will not own to it, uses every other body's spectacles, and reads in every other body's big book too, although the great book—the book of books—lies open to us all. It is not, perhaps, harder at bottom to discriminate scalp diseases than others incident to the human frame; but few care to look critically at them, and those who do, mount so many crotchets and quiddities, that too often they might just as well not have looked at all. It would be very desirable that beds could be reserved in hospitals for cutaneous maladies alone. All country incurables should be forwarded, the different forms of scalp diseases

would be fully and fairly recorded, and the diagnosis established on foundations unassailable.

It adds sufficiently to the difficulty of discrimination when we find that cutaneous maladies supervene, one upon the other,—that they may co-exist,—and that they undergo transformations, as from vesicular into pustular, from vesicles and pustules into crusts, and from pustules into scabs,—that they may be isolated or approximate, circular or irregular,—in short, totally, or almost totally, changing their form and aspect, in a manner inconvenient enough to builders of systems, but giving very little concern to old Dame Nature, on whose kibes we doctors are so fond of treading, and whom she often and so mercilessly snubs in return. If we were not just so clever,—but we are too knowing by half; and the good mother, who only cares to speak in the vernacular, and knows nothing of strange tongues, slinks off abashed in the presence of so much genius and learning.

Something might be done towards the elucidation of diseases of the scalp by the method of exclusion, rejecting names and alleged pathological conditions, unless the first could be shown to represent a fact, the second to constitute a fact. I would reject the terms favus, tinea, achores, and replace them by that of porrigo, which seems about the most usual denomination, else reject porrigo and choose one of the other terms. However, there can be no objection to employ them all *as synonymes*. The name is of little moment, only let us connect it with the same malady, and that without any needless multiplication of species. Porrigo once strictly circumscribed, the diagnosis of scalp affections becomes comparatively easy. Porrigo, fortunately, is rare, compared with other diseases of the scalp; nevertheless these are incessantly confounded with it. Private practitioners have rarely the means, the inclination, or, unhappily, the competence, to pursue such inquiries successfully. It is hardly to be expected they should prove in unison, when no two writers, perhaps, on cutaneous maladies, use the same phraseology, or recognise the same pathological distinctions. Names, facts, definitions, and assertions, have been bandied about till the subject has lapsed into a very undesirable state of confusion. There is but one porrigo. Porrigo scutulata, favosa, lupinosa, however portrayed in the magnificent folios of Willan, Alibert, and their successors, are one and the same affection. I would here earnestly urge the employment of the daguerreotype. This would give the exact forms and interlimitations of these affections. Afterwards they might be coloured from nature. As for porrigo furfurans, decalvans, larvalis, severally, they are not porrigo at all. The scalp is subject to scaly diseases, lepra and psoriasis for example, but never, so far as I have observed, unless when the rest of the surface is implicated. I may here remark, by the way, that psoriasis is very distinct from lepra, though in some respects it may resemble it; also, that it is vastly more frequent. I have known psoriasis to occur in three, if not four,

members of the same family. Pityriasis, dandriff of the head, rarely amounts to disease, unless neglected, or, as it is apt to be, irritated by the too frequent and assiduous use of the small tooth comb, tearing up the scalp and generating the branny scales which a long-haired brush would much better deal with.

The scalp is subject to vesicular eruptions, or rather to but one vesicular eruption, eczema, for neither herpes nor scabies (itch) is ever seen upon the hairy scalp. Even eczema either co-exists with impetigo, or is converted into it; and as papular diseases do not implicate the scalp, its only maladies, eczema always excepted, and of course the exanthems, might well be esteemed pustular. And as among pustular eruptions of the scalp, of course I am not including variola or its sub-denominations, there are but two, impetigo and porrigo, for acne and ecthyma rarely or never affect the scalp. Impetigo, by this time, should be tolerably well known, and yet it is continually mistaken for porrigo. Impetigo is entirely incommunicable, whereas porrigo is otherwise. Impetigo, it should be clearly and constantly kept in mind, occurs under two forms,—that in which the pustules are psydracious or agglomerated, and that in which the pustules are phlyzacious or scattered. In the former case, the pustules are smaller, and give rise to continuous crusts; in the latter, impetigo sparsa, they are larger, attended with much itching, and, so far as I have seen, pretty equally diffused over the head, coming in successive crops, and ending in small scabs, but not continuous crusts. In three instances, within the last two years, I saw it occur simultaneously in two members at a time, of pretty large families, severely,—without, however, extending farther. Professor Bennett, in his very instructive observations in the July Number of this Journal, seems to question the pustular origin of porrigo; but whether this can be established or not, the minute, yellow, depressed pustules or spots, with which the malady commences, with their central hollows, and circular, gradually enlarging crusts, presenting, on mutual approximation and pressure, somewhat of the hexagonal aspect of a honey-comb, and finally, the large, broad, more or less abnormal crusts or scabs, yield quite a sufficient basis of accurate discrimination. To this, however, we are enabled to add the cat-like odour of the disease, and the frequent, if not universal, presence under the microscope, of a cryptogamic growth,—which joint indications should, with sufficient attention, complete the diagnosis. Are these cryptogamic growths sufficiently established, however,—are they necessarily present,—or are they merely results of that well-known law by which low degrees of vitality in the living tissues, and even the excretions themselves, become the nidus of parasitic growths, plant or animal; for instance, the soft tartar of the teeth, which, not to mention other excretions, is found to be infested with a microscopic insect? As for the treatment of cutaneous eruptions of the scalp, porrigo in particular, it may be summed up under two indications, of which the first is to remedy

the constitutional taint of which the local affection may be deemed the expression, or, at any rate, the correlative; next, to supersede the diseased local action by the judicious alternation of one or more of the very numerous remedies,—the nitrate of silver, for example,—with which our apparatus medicaminum so abounds.

Part Second.

REVIEWS.

Tabulæ ad Illustrandam Embryogenesem Hominis et Mammalium, tam Naturalem quam Abnormem. Auctore W. VROLIK, M.D., in Athenaeo Amstelodamensi Professore Ordinario, &c.

Plates to Illustrate the Natural and Abnormal Embryogeny of Man and the Mammalia. By Professor W. VROLIK. 4to. Amsterdam. 1849.

DURING the last century no country contributed more to the advancement of anatomical science than the small kingdom of Holland. The works, for example, of Ruysch, Albinus, and Sandifort still afford abundant evidence of the great assiduity and success with which anatomical science was formerly pursued in the Dutch medical schools. And at the present day, among their living professors, are numbered several who have already distinguished themselves in the same line of research, as Schroeder van der Kolk, Donders, Harting, Hebertsma, and others. The author of the work more immediately under review, is Professor of Anatomy in the Athenæum of Amsterdam, and is already well known by his various anatomical writings and researches. The work itself is divided into two parts, the first of which refers to the natural development of the embryo. The second part comprehends the various forms and types of anormal development or monstrosities. The second part is the most extensive, and by far the most important portion of Professor Vrolik's beautiful work; and it contains, we believe, the best collection of figures which has as yet been published of the numerous and diverse varieties of malformation to which our organisation is subject. It is a work which ought to have a place in every public medical library, and is indispensable to any one specially studying the department of teratology.

The volume comprises one hundred quarto plates, and most of the plates contain four, six, or eight figures. Many of the drawings published by Professor Vrolik are original, and represent malformations contained, either in his own museum, or contributed by

friends. They differ from most previous plates of malformations, in presenting minute and accurate dissections of the malformed parts. Some of the drawings afford new and beautiful illustrations of the well-known law, that a large class of the malformations of particular organs consist merely of one of the various stages or types of development through which these organs pass during embryonic development. Among the numerous illustrations of this origin of malformations to be seen in Professor Vrolik's volume, no one has interested us more than a new malformation of the descending aorta, which he gives in his 88th plate, from a preparation contained in the rich museum of Schroeder van der Kolk, at Utrecht. Several years ago Dr Allen Thomson first showed, that, in an early stage of development, the descending aorta was at first double, or rather originally consisted of two separate vessels running parallel to each other, and which gradually approximated, and ultimately united into one tube, in consequence of the disappearance of their uniting sides along the line of junction. In the malformed aorta to which we have referred, the aorta was found double in its lower half in an adult dying of fever; and thus presented, in a permanent type, and as a malformation, a type of arterial structure, which normally is transient, and pertains only to a very early period of embryonic development.

The work abounds with illustrations of the congenital deformities of the bones and joints, and specially of the pelvis. Several figures show how much the transverse diameter of the pelvic outlet is increased in cases of congenital dislocation of the femora. Plate 74 is a good illustration of that very rare form of pelvic deformity first described by Robert, in which both of the sacro-iliac synchondroses are ossified, and the transverse and other diameters greatly altered and contracted.

One of the most interesting new contributions to teratology which the volume contains, is a series of sketches of what we believe to be the most perfect case of hermaphroditism which has as yet been detected in the human subject. (See plates 94 and 95.) The malformed individual was brought up as a female. At the age of 17 a copious beard began to grow, and a testicle descended through each inguinal ring. The female dress, however, was retained till the age of 38, when, by the advice of three physicians, who carefully examined the case, the malformed being was declared a malformed hypospadic man; and the male attire and habits were in consequence assumed and retained till the death of the individual twenty years afterwards. On dissecting the body, there was found co-existing the following combination of internal female and male sexual organs. Among the internal female organs were present a distinct uterus, furnished with broad ligaments, and the ligaments of Douglas. The uterus passed into a long and narrow vagina, which joined the hypospadic urethra. At the upper extremities of the uterus were the rudiments of the two Fallopian tubes. Professor

Vrolik further detected two bodies, which he believes to be imperfectly formed ovaries,—no Graafian vesicles being visible in them, but the bodies themselves being furnished with the usual pampiniform plexus of the ovary. On the other hand, of the male sexual organs were found two testicles contracted and small, and furnished with a tunica vaginalis. The internal structure of the testes was imperfect. A slender vas deferens ran down from the testis, and ended below on the right side in the vagina, and on the left side in the uterus. The testis and ovary of the left side were more imperfect than those of the right. There was no vestige of the prostate gland or vesiculæ seminales.

The late observations of various continental anatomists all tend to show, that even so complicated a hermaphroditic malformation as the above may possibly be the result merely of arrested development, or of the permanence and continued growth of early embryonic types of structure. Weber, of Leipsic, has shown that in the adult normal man we can trace a rudimentary female uterus; and Rosenmüller and Kobelt have more lately tried to demonstrate the rudiments of a male vas deferens and epididymis in the broad ligaments of the adult normal woman. Each sex thus possesses, in a rudimentary form, even in the adult state, some of the more important internal organs of the other sex; and when some or all of these rudiments become preternaturally developed upon the same individual, we have, as a consequence, a so-called hermaphroditic malformation.

The Principles of Surgery. By JAMES MILLER, F.R.S.E., Professor of Surgery in the University of Edinburgh, &c., &c. Second Edition. Adam & Charles Black. 1850. Pp. 805. 8vo.

THIS edition of Mr Miller's "Principles of Surgery" is so much enlarged and improved, not only in volume and typography, and in the addition of more than two hundred illustrative wood-cuts, but by so large an amount of new material, that it may almost be regarded as a new work, and now fully realizes the object for which it was first offered to his pupils, that of being a volume of "reference as well as a text-book." Whether viewed as an abstract of his university lectures, or as a concise exposition of the science of modern surgery, it is alike creditable to the author, bearing testimony to the advances made in our standard of medical education, and furnishing to the general practitioner a valuable epitome of the present state of surgical science.

Among the more useful additions made to the work in this new edition, we observe an entire new chapter, extending over seventy pages, on Surgical Medicine, or Constitutional Affections in Surgery. Although necessarily condensed, this section is nevertheless very complete, and we need not add important; for all who have ob-

served, with any care, the surgical practice pursued in our great hospitals, must be aware how much of the success of our most eminent surgeons is dependent upon a skilful management of constitutional affections, and a careful attention to the general health.

Mr Miller's account of the phenomena and theory of inflammation and of its results, is very excellent and complete. Without committing ourselves to an entire concurrence in the physiological and pathological views taken by the author,—for about many points there must still necessarily be some diversity of opinion,—we can yet fairly accord to him the merit of having given a clear and judicious resumé of the present state of medical science in relation to this fundamental disease, and its results. There is still an adhesion (though somewhat diminished) to the colourless-corpuscle theory of Drs Addison and Williams, which, in the next edition, will, we trust, entirely disappear, and make room for a more tenable doctrine.

In the chapters on ulcers we have, we believe, for the first time, an attempt to illustrate the characters of different kinds of sores by simple *wood-cuts*. Notwithstanding the want of colour, we have no hesitation in saying that the experiment is eminently successful, and that the illustrations convey, to our eyes at least, a very accurate idea of the familiar and distinctive characters of these sores.

The numerous wood-cut illustrations throughout the volume are, indeed, almost without exception, good, and well suited to facilitate the study of the subject. We must, however, except the delineation of destruction of the bones of the cranium, given in Figs. 50 and 51. Probably the introduction of an outline and more full description would remove the present rather mysterious character of these drawings.

The microscopic anatomy of tumours, and of various morbid affections and changes—as, for example, the disease of cartilage—is fully illustrated, in many instances by the same wood-cuts which adorned the original monographs on those important subjects. The numerous original illustrations are alike creditable to the draughtsman and the engraver.

An appendix contains Mr Miller's lectures on the use of chloroform in surgical practice, which, we presume, are already sufficiently known to most of our readers. Their republication here will tend to remove the prejudices against the use of this invaluable agent, which still linger in some quarters in spite of all that has been written on the subject.

To this edition Mr Miller has added, at the conclusion of each chapter, a very copious reference to all the best systematic works and monographs, in which the respective subjects have been discussed. This must tend greatly to enhance the value of the book, particularly as a volume of reference for the practitioner.

In a systematic work, such as this, there are necessarily many points which invite discussion. But it would be unfair to enter upon the arena, when we have the author at the disadvantage of merely giving a compendious view of principles or truths generally acknowledged, or, if offering opinions or theories which are doubtful, only indicating the grounds on which they are based. It only remains for us to recommend the work to the favourable notice of the profession. W. R.

Corpulence: or Excess of Fat in the Human Body. By THOMAS KING CHAMBERS, D.M., Fellow of the Royal College of Physicians, and Gulstonian Lecturer for 1850. 12mo, pp. 166. London, 1850.

WE welcome this little work of Dr Chambers as a highly acceptable addition to the medical literature of an important and too much neglected subject. It consists of a reprint of the Gulstonian lectures delivered in 1850, and originally published in the "Lancet." We know that many members of the profession perused these lectures with interest, even when doled out to them, at hebdomadal intervals, in double columns of small type; and few will regret the fortunate misunderstanding between the printer of the "Lancet" and our author, to which (as we learn from the preface) we are indebted for the complete and polished treatise now before us.

The first three chapters are devoted to general observations upon the structure of the fat vesicles, upon the mode in which fat is deposited within them, and upon the chemistry of its formation and assimilation. The author believes, that by the consumption of suet, and other fatty articles of diet, a certain amount of oleaginous material is conveyed into the body ready formed; but that the whole fat in the system is collected by so simple a process, is, he thinks, quite improbable. If it were so, "the quantity of fat fixed or secreted by an animal will be represented almost exactly by the substances soluble in ether and alcohol, but insoluble in water, which makes a part of the forage consumed" (p. 24). He agrees, then, with Liebig, Boussingault, Bernard, Frerichs, and other modern physiological chemists, that besides the *oleaginous*, other proximate principles of diet, and possibly *all*, are concerned in the production of fat; and that starch, sugar, &c., may undergo conversion into fat, by losing a part of their oxygen, under the influence of the vital force, or by being acted upon by some of the secretions of the body. He states his opinion: "That there is no evidence adduced to show that it (fat) is formed from other corporeal tissues, but that in unhealthy states of constitution its increase may coincide in point of time with their decrease" (p. 32). One common expression will include all the circumstances in which fat is produced. "It is ne-

cessary, *that the materials be digested in a greater quantity than is sufficient to supply carbon to the respiration*" (p. 33).

In the fourth chapter, attempts are made to account for the different energy of the intuitive desire for oleaginous food, manifested by different portions of the human race, and at different periods of life. The author remarks, that a strong predilection for fatty articles of diet is shown by girls when they arrive at the age of puberty; and he accounts for it by supposing that, during the period of life at which menstruation continues, a less amount of carbon is exhaled by the lungs,—and that when, from any cause, menstruation becomes arrested, the activity of the respiratory function becomes increased. We confess ourselves unconvinced either of the fact, or of the validity of the chemical evidence on which its explanation is attempted. The researches of Andral and Gavarret, upon the relative proportions of carbon excreted, in different circumstances, from the lungs, however probable their results may seem, still require confirmation by other observations (conducted, moreover, on a very large scale) before we can admit that uniformly vicarious depurating function of the uterus and lungs which Dr Chambers here assumes.

In the two succeeding chapters, attention is directed to the approximate mode of determining the amount of fat in the body, by means of the balance and the application of a standard of healthy weight adapted to the height of different individuals. It is stated, and we believe correctly, that any considerable increase in the weight of an individual, if unaccompanied with corresponding increase of muscular strength and activity, may be considered as due to deposition of fat. The author suggests that, for the purposes of life assurance, it would be desirable that the height and weight of every party desirous of effecting a policy should be ascertained, and that any considerable deviation from the standard of health should be regarded as accompanied with proportionate risk. He likewise insists upon the valuable indications which the physician may derive from weighing at intervals patients who are the subjects of chronic diseases.

At page 73 there is introduced an interesting table by Dr Hutchinson, exhibiting what may be regarded as the mean standard weight of healthy males in the prime of vigorous life in this country. The table rests upon 2650 observations, made on men of all ranks in society,—men capable of, and accustomed to, use great muscular exertions. Contrasting curiously with this table, we find some ingenious observations by Mr Brent upon the weight of the human body, which would correspond to some of those antique statues most remarkable for symmetry of form.

"The calculations of what the statues would weigh if they were of any fixed stature, was made by the ingenious Mr Brent, and in a way simple and comprehensible enough. He immersed in a bath accurate copies of them, and found what quantity of water was displaced. He thus obtained the weight in water

of the statue at one height. The weights of the other heights were easily arrived at by the common geometrical formula, that 'the bulk of symmetrical bodies is as the cube of any of the diameters.[1] The specific gravity of ordinary water and that of the human body, alive and with its hollow organs filled with air, is as nearly as possible the same, so that the calculated weight of water represented what a living man of that bulk would weigh. To this $\frac{1}{17}$th is added to allow for clothes, which we are accustomed to include in weighing ourselves, and the comparison between the statues and their prototypes is complete. Thus, if a figure 61 inches high weighs 6 stones 6 lbs. (or 90 lbs.), it would, if 62 inches high, weigh

	Stones.	lbs.
$\left(\frac{62}{61}\right)^3 \times 90 = 90{\cdot}049$, &c.	= 6	$10\frac{1}{2}$ nearly.

Add $\frac{1}{17}$th for clothes, and disregarding decimals, we get the numbers below,

Stones.	lbs.		Stones.	lbs.
6	6	$+ \frac{1}{17} =$	6	11
6	$10\frac{1}{2}$	$+ \frac{1}{17} =$	7	3

and so on, through the whole of the subjoined table."

The five statues to which Mr Brent has applied his calculations, are the Bronze Tumbler, the Dying Gladiator, the Theseus of the British Museum, the Bronze Hercules of the same collection, and the Farnese Hercules (colossal and reduced).[2] It is interesting to observe how nearly the proportionate weight of the Gladiator corresponds with Dr Hutchinson's table:—

Height.		Dr Hutchinson's Table of Standard Weight.		Dying Gladiator (calculated by Mr Brent).	
Ft.	In.	St.	lbs.	St.	lbs.
5	1	8	8	8	7
5	2	9	0	8	12
5	3	9	7	9	4
5	4	9	13	9	11
5	5	10	2	10	2
5	6	10	5	10	9
5	7	10	8	11	3
5	8	11	1	11	10
5	9	11	8	12	2
5	10	12	1	12	11
5	11	12	6	13	4
6	0	12	10	14	0

These numbers are extracted from a much larger table, given at p. 77, in which all Mr Brent's calculations are embodied.

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[1] We are here forcibly reminded of the calculations of Gulliver's tailors in Liliput; and of the time-honoured Celtic formulæ of "Shemus an Snaht," for which see "Waverly."

[2] The Farnese Hercules is a colossal statue, and its upper quarters are, therefore, exaggerated by the artist, to allow for the distance at which they are beheld by the spectator.—Note, p. 78.

In the next three chapters of his work, Dr Chambers treats of the pathology, causes, and effects of obesity, and illustrates his observations with a table of thirty-eight cases, in which the sex, age, height, weight, attributed cause of the disease, and various other important particulars regarding each patient, are recorded.

The treatment of that rare form of obesity "which commences at birth, and goes on increasing during infancy and childhood," is, the author thinks, not likely to prove successful. Such cases commonly terminate fatally in early life, and may be regarded as species of monstrosities, complicated for the most part with other disease. But the polysarcia of patients who have passed the period of childhood is much more amenable to treatment, and is the more controllable the later in life it has commenced, provided old age does not interfere with the active employment of suitable remedies. All supply of fatty articles of diet is to be withheld; but, as all substances used as food contain some oleaginous material, and as fecula, sugar, &c., may undergo the transformation into fat, especially when mixed with a little of that substance, Dr C. advises that the appropriate means should be taken to prevent the food from lying long within the stomach, and passing there into fatty fermentation. Hence early and spare meals, with moderate allowance of fluids,—exercise adapted to the powers of each patient,—early rising, bathing, and stimulation of the cutaneous circulation by friction, become most important means of cure, and, unless these are resorted to, drugs will be found useless.

The author does not advise purgatives in the plethoric form of the disease, in which the action of the bowels is for the most part regular; "but in the asthenic obesity of old people, where the abdominal walls are weakened by long pressure of an unnatural weight, it is necessary to employ them."—P. 131.

Of the virtues of alkaline remedies he speaks highly. He thinks that a trial of aqua potassæ should never be omitted, and states that several of his patients have derived unequivocal benefit from this remedy, given in milk thrice a-day, and in doses varying from ʒss. to ʒjss.

"The chemical affinity of alkalies for fat, point them out as appropriate alteratives in this complaint, and experience proves that they are suitable to the state of the digestive organs."—P. 131.

Bleeding, we quite agree with the author, is bad practice, except in cases complicated with plethora.

Bitter tonics may occasionally be useful, by increasing the facility and rapidity of digestion :—

"The increase of appetite which they cause does no harm; for when patients are getting better, they are usually more obedient to their medical man, and can be taught to control it. Gratitude for the benefit they have received makes them follow advice, however hard."—P. 135.

Other remedies which have been recommended, such as vinegar, diuretics, and iodine, are not approved by analogy or experience, and Dr Chambers adduces sufficient reason to show that their use may even be pernicious.

A short Appendix on "Emaciation," with which the volume concludes, contains some peculiar views upon the tubercular cachexia, and on the *modus operandi* of cod-liver oil. To these we shall probably take another opportunity of directing the attention of our readers.

On the Management of Infancy; with Remarks on the Influence of Diet and Regimen, &c. By CHARLES HOGG, M.R.C.S., L.A.C., &c. London, 1849.

WE have not for a long while read anything so amusing as this book. We do not mean to asseverate, that the hygienic management of infancy is intrinsically a mirthful subject, still less that there is anything decidedly facetious in scrofula, skin diseases, and liver complaints, to each of which Mr Hogg devotes a separate chapter. The amusement lies not in the matter, but in the manner, of the book, which contains, exclusive of an appendix, 188 paragraphs,—no consecutive couple of which appear to have any very decided connection with each other, and which, taken individually, are in almost every instance full of comical absurdities. To be sure, there are numerous quotations from Paris, Liebig, Dr Conquest, Dr Underwood, and others, which are no doubt sensible enough sentences as they stood in the works in which they originally appeared; but even they, mixed up as they are with the original matter of the present book, assume a quizzical aspect from the strange company in which they find themselves.

When we looked into the author's preface, and learned from him that his qualifications for authorship consisted of "watchful observation over a period of twenty years, and a constant practical experience, perhaps unusual both in kind and extent, especially in cutaneous diseases and scrofula," we were led to expect some useful information, and hoped to get some good practical hints from a writer who reports "the results of treatment, the benefits of which have been incontestably proved;" but, alas, we could gather nothing more from that part of the book which refers to infancy, than that a great many children are lost by bad management during early life, and that the proper article for nourishing infants is biscuit-powder. This last article is the author's great standfast, his *piéce de resistance*,— whenever anything is specified in the way of diet, it is sure to be biscuit-powder in some form.

Dr Paris, who appears to be a favourite authority with Mr Hogg says, that an author who comes forward with a new book, "must be able to satisfy the public tribunal, that he is either prepared to

extend the general stock of information, or to correct the errors into which preceding writers have been betrayed." We do not know exactly on which of these grounds Mr Hogg ostensibly builds his claim to become author; we infer from his preface that it is on the former of the two, his extending our stock of knowledge; for we do not find that he does much in the way of criticism to correct the errors of other writers, as almost all the passages which he quotes are stuck down without note or comment. But, though we cannot say which may be his ostensible reason for becoming author, we think we see very distinctly what is the real motive. He lets out his secret in his very first sentence—"our object is not to write a purely scientific treatise,"—*i. e.*, the book is not intended for the profession, but for the laity; it is, in fact, an advertisement of the author. Now, had the book borne on its title page merely the author's name, we should have set it aside at once as unfit for criticism in our pages—as being, in short, the production of some of those irregular practitioners whose dissertations appear in the newspapers, and pay advertisement duty; but when we find that the author adds to his name the titles of Member of the Royal College of Surgeons, and Licentiate of the Apothecaries' Company, we feel constrained to take some notice of him. It is not as an individual book that we notice this production, it is as a specimen of a class of works which are constantly issuing from the press, especially of London, where, from the enormous bulk of the population, there is a great concentration of gullible people, who buy and read such books, and, on the *ignotum pro magnifico* principle, look upon them as wonderful authorities; then, finding that they end in no practical benefit, toss them away in disgust, throw the blame of their disappointment upon the profession from which they are supposed to emanate, and talk of the uncertainty and instability of medical science and medical art. It is time that something were done to get rid of this nuisance. We call upon all who have any respect for the character of our professional literature to wage war upon this kind of authorship; and we vow on the handle of our steel-pen (as solemn an adjuration as ever Templar took on the cross of his sword) to "knock the powder out of the puff" of all these advertisement book-makers who fall into our hands.

In pursuance of this doughty resolution, and as the surest means of accomplishing it in the present case, we extract some passages from the book before us, upon which our comments will not require to be lengthy, as the excerpts themselves will do for us all that we desire.

We begin with the following sentence, which is the exordial paragraph of the book:—

"1. In commencing a work on diet and regimen, it may be profitable as well as interesting to the general reader to enter into an examination of the digestive capacities of different animals, showing the relation which the cavity [WHAT CAVITY?] bears to the kind of food destined by Providence to afford

nourishment and support; and, as our object is not to write a purely scientific treatise, we shall offer a few general remarks on that subject, in the hope that they will be found to be useful and important in practice."—P. 1.

Two things in this passage perplexed us: first, we could not make out how an examination of the digestive capacities of different animals, say from an elephant to a polype, could be practically important in relation to the management of human infants; and, secondly, we were at a loss to comprehend why an author's intention "not to write a purely scientific treatise," should be a reason for his commencing it with so purely scientific a discussion, as the comparative physiology of digestion. But our author appears, on second thoughts, to have come pretty much to our conclusion, that all this was extraneous matter; for, notwithstanding the very positive promise contained in this sentence, there is not in the whole chapter a word about the digestive capacities of different animals, unless we can regard as such a statement to the effect, that man can live both on animal and vegetable food, and that—

"Spallanzani has satisfactorily shown, that animals may be brought to live on food of the most opposite kinds, provided the change is gradually accomplished,—for example, a pigeon on flesh; an eagle on bread, &c."—P. 2.

Forthwith our author plunges into physiological chemistry, and commences with a long quotation, chiefly about phosphate of soda, from Dr Gregory's translation of Liebig's "Chemistry of Food," of which, as it has nothing whatever to do with the subject of the management of infancy, we need take no notice here. But in the 6th paragraph we find the following opinion expressed, which is so curious in itself, and supported by such a remarkable argument, that we cannot resist quoting it:—

"We do not think it is of such weighty importance, in reference to the nourishment of the body, whether the food is nitrogenous or not, seeing that the ultimate effect of all aliment is the same. There is but little difference between the flesh of carnivorous and herbivorous animals."—Pp. 3, 4.

The seeing that the ultimate effect of all aliment is the same, is a kind of vision quite peculiar to Mr Hogg. We had always been led to suppose that the ultimate effect of different aliments, in reference to the nourishment of the body, was entirely different, just according as they contained nitrogen or not. We had always thought, that unless the food contained some nitrogen, there would be a difficulty in forming blood and muscle, and other azotised constituents of our bodies; "not at all," says Mr Hogg, "the herbivora have flesh just like the carnivora, so that nitrogen in the food is of no consequence," from which we infer, that the sentence, if it means anything at all, means this, that the herbivora take no nitrogen in their food. Mr Hogg is fond of quoting Liebig, though it does not appear to us that he always understands what he quotes; but we think that, if he will look into the Baron's "Chemistry of Physio-

logy," he will find that grass, hay, and oats do contain nitrogen. And if he will consult Magendie, we think he will find that when animals are fed on food containing no nitrogen they die, so that we suspect, in spite of his learned argument from the flesh of the herbivora, that the presence of nitrogen in the food must be of some importance.

At page 6 we have a disquisition on the history of medicine, which, we have no doubt, will open the eyes of some of our readers to the lamentable state of young physic.

"12. The ancients believed that health formed the principal part or basis of human happiness; and they were thus induced to investigate the causes of human suffering, and the most simple means of relieving it. They found that diet was the most potent remedy to prevent, remove, or mitigate most diseases, and that led them to the study of the vegetable kingdom, as applicable for food and medicine; and experience, aided by sagacious reasoning, induced them to select as diet, that which enabled them, with a few simples, to remove with ease many infirmities otherwise found to be invincible by human art.

"They believed disease to be an adventitious occurrence, and not an inherent principle in our nature; and when it arose in the system, they never thought of attributing it to derangement in the harmonious actions of the constituent parts of the body, but to some noxious substance which had been taken into the stomach. While these sensible views guided them in their treatment, we need not wonder that diseases were few in number, and the means of cure simple and successful. But since diet has been considered a matter of *taste*, instead of principle, and disease an effect without a cause, disorders have increased almost beyond calculation, so that their names have become a burden to the memory."—P. 6.

We presume that those curious gentry, whom our author designates as "the ancients," lived in the "good old times," of which we read so much in novels and ballads, but of which no one has even been able to fix the chronological epoch. We need not, therefore, be ashamed to confess that we do not know when such opinions prevailed, or who it was who held them. Mr Hogg tells us that they believed that all disease was owing to some noxious matter taken into the stomach, and he applauds this as a sensible view, and thus adopts it. It does certainly appear from this that in one respect the ancients had the better of us moderns. They regarded disease as having at least one cause; we, it seems, generally look upon disease as an effect without a cause. We did not know that there could be such a thing as an effect without a cause, much less that this was generally held to be actually the case with respect to diseases. Verily, we must correct our philosophy, as well as our pathology, if we seek to hold sensible views regarding diseases.

Chapter II. discusses the appropriate food for infants, and contains quotations from Cato the Censor, Dr Conquest, Dr Primrose, and Dr Dick. The appropriate food is, of course, biscuit-powder, so we need say no more as to the substance of the chapter; but we lay before our readers one sample of its style, from which

they will be able to judge of the lucidity of expression which characterises it.

"The great object of digestion is the formation of *chyle*, consequently that food which supplies the greatest amount of that substance must be the most nutritious, and cæteris paribus."—P. 13.

We will be obliged to any body who will render this into English.

Chapter III. relates to sanitary conditions; and we extract from it a portion of the result of the author's twenty years' watchful observation, which is of rather an astounding nature.

"The importance of this subject [unwholesome air] was deeply impressed on the author's mind when a student in Manchester. Having at one time had the superintendence of a densely populated district, under the direction of the Lying-in-Hospital, called Little Ireland, from the majority of its inhabitants being natives of the sister isle, we have frequently attended women in labour, living in a room with five and six and even more inmates. Wholesome air was there out of the question. On many occasions, when the child was ushered into the world, there was not a rag to cover it. Such wretched poverty could not be otherwise than the parent of disease. Our astonishment at the time was, and still is, that vitality in so many instances overcame such formidable obstacles. We verily believe there is more of Godfrey's cordial, and other opiates, used in Manchester than any two cities of similar size in the kingdom. We were acquainted with a chemist and druggist, with whom it was usual to sell *gallons* on a Saturday night. It is a common practice for mothers to give large doses to their infants, in order to procure four or five hours sleep, while they are employed at the factory. When remonstrated with, the usual reply is, they cannot afford to have a person to look after the children. (We shall say nothing of the frightful burns of children from falling into the fire during the mother's absence.) The importance of fresh air is so self-evident, that it would be a waste of time to enlarge on it further. Nevertheless, we cannot avoid inserting the following few observations, in the truth of every word of which we perfectly agree."—P. 24.

We print this exactly as we find it at page 24. We had long believed that the air of Manchester was none of the purest; and we had also a very decided impression on our minds, that Godfrey's cordial was in pretty extensive use there; but we never thought that this or any other opiate was used to such an extent as to vitiate the atmosphere of the locality. We commend this specially to the watchful observation of the sanitary commission, and also to Mr Smith, when he next writes on the air and water of towns.

Chapter IV. refers to bathing, and is chiefly occupied with a strong pleading in favour of the vapour bath, against which, it appears, medical men entertain prejudices, which arise solely from ignorance of its effects. That our readers may be able, so far as it is in our power to help them, to enjoy this luxury in perfection, we extract the following descriptive passage:—

"The one patented some years ago by Mr Charles Whitlaw is unquestionably the most perfect and useful that has yet been invented. It consists of water boiled with a furnace, and having a brass tube attached to the dome, which conveys the vapour into a cistern three and a-half feet in length, two in

breadth, and about fourteen inches in depth, and covered inside with tinned copper. This is covered with a lid, composed of a frame and copper plated with tin, which is perforated with small holes. In the inside of this is a round tin box, also perforated, and having a plate running transversely across, which is attached to the tube coming from the boiler. A pole, about six and a-half feet long, is placed at each corner, with a frame at the top, which is covered with thick calico curtains, and has a thermometer suspended to the frame, and which completes what is now called Whitlaw's patent medicated vapour bath."—P. 47.

This description has at least one qualification, which is, we believe, of no small importance in the specification of a patent,—viz. that it shall be so unintelligible that no human being could avail himself of it if he were so inclined.

The mere vapour bath, however, turns out after all to be but a poor affair. To be truly efficacious, it must be medicated; and as it appears that there is a rampant infidelity abroad in the world, as to the utility of medicated vapour baths, we have a very strong argumentation in behalf of the practice, of which the following is a part:—

"If the utility of medication is denied, what becomes of the doctrine of infection, or the means recommended for destroying it? And if the air of a room can be deteriorated to such an extent as to affect a healthy person subjected to its influence for a short time, and give rise to an identical and fatal disease, why should not a medication have an equally salutary effect in counteracting the principles of disease already established in a constitution? Do not the fumes of turpentine give rise to a peculiar odour in certain secretions of the body?—and does not the smell of paint bring on colica pictonum and paralysis?"—P. 53.

We are puzzled to know why our denying the utility of medication, as applied to vapour baths, especially with such substances as Mr Hogg recommends, should destroy the doctrine of infection. We are also puzzled to know what disease is that which is identical with deteriorated air; and we are still more puzzled to know where he learned that the smell of paint causes colica pictonum and paralysis; and where he finds the medical men who are conversant with such a fact.

The most curious thing, however, about Mr Hogg's medicated vapour baths, is the substances which he uses in the form of vapour.

"We have frequently seen the most striking and almost immediate results produced by suspending iodide of potassium in the vapour bath in scrofulous ulcers; and repeatedly we have been told by patients that they have perceived the taste of senega in the saliva at least twenty four hours after the bath. Could such a circumstance take place, if medication were useless?"—P. 54.

How iodide of potassium, which cannot be volatilised, except to a very inconsiderable extent, at a full red heat, manages to get into the form of vapour even in Whitlaw's patent vapour bath, "which consists of water boiled with a furnace," we do not know. It must, however, surely be admitted, even by the most sceptical anti-medi-

cationist, that the perceiving the taste of senega in the mouth twenty-four hours after a bath, fully proves the utility of medication.

There then follow three chapters on cutaneous eruptions, scrofula, and liver complaints; but they do not appear to refer specially to these diseases as occurring in children, for most of the cases quoted in illustration of the subject, and in laudation of Mr Hogg's practical success, are in adults.

As our readers may wish to be informed as to what is the prevailing opinion on the Continent, in regard to the pathology of scrofula, we give it as follows, on the authority of Mr Hogg:—

"The theory of the origin of scrofula, which has recently come into much favour on the Continent, especially with those favourable to the use of cod-liver oil, makes the pathology of the disease turn upon a different secretion of bile; the chylous digestion is supposed to be defective from the absence of the fatty principle of bile. In support of this fact, we have frequent occurrence of fatty liver in persons who die of scrofulous disease."—P. 85.

If we do not, however, find any notabilia in the way of pathology and therapeutics, we find some curious things in regard to materia medica. Thus, at p. 56, we find a formula for what Mr Hogg is pleased to call a lotion; but which most people, we should think, would designate a liniment. We copy the formula as it stands:—

"Ung. hydr. nitratis, one ounce; Ol. coc. palmat., two ounces; Liq. calcis, three ounces."—P. 56.

We thought we knew our pharmacopœia pretty well; but as we never had heard of the oil of Calumba, and were, in fact, under an impression that there was no such oil, we were rather astonished to find it set down in this business-like way in a formula for a liniment. We were enlightened, however, by the succeeding sentence, which tells us that this mysterious *ol. coc. palmat.* is cocoa-nut oil!

With the orthography of our materia medica our author makes wild work. There is a mis-spelling of some unfortunate drug in almost every page.

Thus we have *taraxicum for taraxacum*—passim; *Inf. chiryitæ* for *Infus. Chirettæ*—passim; *Mistur. Amygdall.* for *Mist. Amygdal.* p. 68; *Potassa iodidi* for *Potassii Iodidi*, pp. 69 and 71; *rumax aquaticus* for *Rumex aquaticus*, p. 62; *Tormantil* for *Tormentil*, pp. 72 and 90; *Belladona* for *Belladonna*, pp. 104 and 113. Nor is the mis-spelling confined to the names of drugs; other vocables come in for a share of it. Thus at p. 105, we have *patela* for *patella*, and at p. 85, we have "the bone had been exposed in a *caries* state," &c. &c.

We gladly pass on, however, from Mr Hogg's materia medica to his Appendix on the materia alimentaria, in which we are happy to find one statement which quite accords with our own opinion. He says at p. 128:—

"Oysters have long enjoyed the general good opinion of mankind."

Deservedly, say we,—they are most amiable molluscs; and with this comfortable conclusion, we end our extracts from a book, which we found from its blunders and absurdities, so entertaining, that we absolutely allowed a new number, both of "Copperfield" and "Pendennis," to remain unread till we had recovered our wonted gravity a little after its perusal.

We have read it, we have laughed over it, but we have become serious again. In sober earnestness, is it not melancholy to see such a book emanating from the British press, with the titles of Member of the Royal College of Surgeons and Licentiate of the Apothecaries' Company appended to the author's name? What will be thought of the state of medical education in Britain, if such a book falls into the hands of any foreigner of average intelligence, and moderate acquaintance with the English language?

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

EXAMINATION OF THE PATIENT.

It is absolutely necessary that an examination of patients at the bed-side should be conducted with order, and according to a well understood plan. I have observed that some students, on being called upon, in their turn, to interrogate a case, have felt great embarrassment, and have been unable to proceed. Others have put their questions, as it were, at random, without any apparent object, and wandered from one system of the economy to another, in a vain search for a precise diagnosis, and a rational indication of cure. But continual practice, and the adoption of a certain method, will remove all difficulty. No doubt, the questioning a patient, to arrive at a knowledge of his condition, requires as much skill in the medical practitioner, as examining a witness does in counsel at the bar. They make it an especial study, and you must do so likewise. You should remember that, in proportion as this duty is performed well or ill, is the probability that your opinion of the case may be correct or incorrect; and that, not only will the reputation you hold among your colleagues greatly depend on your ability in this matter, but that the public itself will promptly give its confidence to him whose interrogations reveal sagacity and talent.

The method of examination differs greatly among practitioners, and must necessarily vary in particular cases. Men of experience gradually form a certain plan of their own, which enables them to arrive at their object more rapidly and securely than that adopted with, perhaps, an equally good result by another. In a clinical class, however, and in order that every one present may follow and understand what is going forward, the method adopted must be uniform. I hold it to be a matter of great importance, that every one standing

round the bed should take an equal interest in what is proceeding, and this he cannot do unless he is fully aware of the manner and object of the examination. The plan which appears to me the best, and which we shall follow, is the one I learnt when myself a clinical student in the wards of Professor Rostan, of Paris. Its object is to arrive, as quickly as possible, at a knowledge of the existing condition of the patient in a way that will insure the examiner that no important organ has been overlooked or escaped notice.

For this purpose, we ascertain, in the first instance, the organ principally affected, and the duration of the disease, by asking two questions, "Where do you feel pain?" and, "How long have you been ill?" Let us suppose that the patient feels pain in the cardiac region, we immediately proceed to examine the heart, functionally and physically, and then the circulatory system generally. We next proceed to those organs which usually bear the nearest relation to the one principally affected—say, the respiratory system,—and we then examine the lungs functionally and physically. We subsequently interrogate the nervous, digestive, genito-urinary and integumentary systems. It is a matter of little importance in what order these are examined,—the chief point is, not to neglect any of them. Lastly, we inquire into the past history of the case, when we shall have arrived at all the information necessary for the formation of a diagnosis.

The following is the arrangement of symptoms and points demanding attention under each of the seven heads into which the examination is divided:—

I. Circulatory System.—*Heart.*—Uneasiness or pain; its action and rhythm; situation where the apex beats; extent of dulness determined by percussion; its impulse; murmurs—if abnormal, their character, and the position and direction in which they are heard loudest. *Arterial pulse*—Number of beats in a minute; large or small, strong or feeble, hard or soft, equal or unequal, regular or irregular, intermittent, confused, imperceptible, &c. If an aneurismal swelling exist, its situation, pulsations, extent, and sounds, must be carefully examined. *Venous pulse*—If perceptible, observe position, force, &c.

II. Respiratory System.—*Larynx and Trachea.*—Voice, natural or altered in quality, hoarse, difficulty of speech, aphonia, &c.; if affected, observe condition of epiglottis, tonsils, and pharynx, by means of a spatula. *Lungs*—State of respiration; easy or difficult, quick or slow, equal or unequal, laboured, painful, spasmodic, dyspnœa, &c. Expectoration, trifling or profuse, easy or difficult; its character, thin or inspissated, frothy, mucous, purulent or muco-purulent, rusty, bloody, &c. Hemoptysis; cough, rare or frequent, short or long, painful or not, moist or dry. External form of the chest, unusually rounded or flattened, symmetrical or not, &c. Movements—regular, equal, &c. Resonance, as determined by percussion, increased or diminished, dulness, cracked-pot sound, &c. Sounds determined by auscultation, if abnormal, their character and position.

III. Nervous System.—*Brain.*—Intelligence—augmented, perverted, or diminished; delirium, stupidity, monomania, idiocy; sleep, stupor, coma. *Spinal cord and nerves*—General sensibility, increased, diminished, or absent; special sensibility—sight, hearing, smell, taste, touch, their increase, perversion, or diminution; spinal irritation, as determined by percussion; motion, natural or perverted, fatigue, pain on movement; trembling, convulsions, contractions, rigidity, paralysis.

IV. Digestive System.—*Mouth.*—Teeth and gums. *Tongue*—Mode of protrusion, colour, furred, coated, fissured; condition of papillæ, moist or dry. *Pharynx and œsophagus*—Deglutition—if impeded, examine the pharynx with a spatula, the cervical glands, neck, &c. *Stomach*—Appetite, thirst, epigastric uneasiness or pain, swelling, nausea, vomiting, character of matters vomited, borborygmi, flatuosity, eructations. *Abdomen*—Pain, distension or collapse, tumours, constipation, diarrhœa, character of dejections, hemorrhoids. *Liver*—Size, as determined by percussion, pain, jaundice. *Spleen*—Size, as determined by percussion. If enlarged, examine blood microscopically.

V. Genito-Urinary System.—*Uterus.*—Condition of menstrual discharge, amenorrhœa, dysmenorrhœa, menorrhagia, leucorrhœa, &c. If pain, or much leucorrhœal discharge, examine os uteri and vagina with speculum; uterine or ovarian tumours. *Kidney*—Lumbar pain; quantity and quality of urine, colour, specific gravity; precipitates, as determined by the microscope, and by chemical tests; action of heat; nitric acid, &c.; action on test papers; stricture.

VI. Integumentary System.—External surface generally; obesity; emaciation; colour; rough or smooth; dry or moist; sweats; eruptions; temperature; morbid growths or swelling; anasarca; emphysema, &c.

VII. Antecedent History.—Age; constitution; hereditary disposition; trade or profession; place of residence; mode of living as regards food; epidemics and endemics; contagion and infection; exposure to heat, cold, or moisture; irregularities in diet; commencement of the disease; rigors.

Such are the principal points to which your attention should be directed during the examination of a case. A little practice will soon impress them on your memory, and in this manner habit will insure you that no very important circumstance has been overlooked. At first, indeed, it may appear to you that such a minute examination is unnecessary; but we shall have abundant opportunities of proving that, whilst a little extra trouble never does harm omission of any part frequently leads to error. It is surprising, also, how rapidly one thoroughly conversant with the plan, is able to examine a patient so as to satisfy himself that all the organs and functions have been carefully interrogated. Remember that the importance of particular symptoms is not known to the patient, and that, consequently, it is not in his power voluntarily to inform you of the necessary particulars. It is always your duty to discover them.

In carrying out the examination, the following hints may be attended to:—

1. It should never be forgotten that you are examining a fellow creature, who possesses the same sensitiveness to pain and the same feelings as you do, and that everything that can increase the one or wound the other should be most carefully avoided. Prudence, kindness, and delicacy, are especially injoined upon those who treat the sick.

2. The questions should be precise, simple, and readily comprehended. When an individual has a limited intelligence, or is accustomed to a particular dialect, you will not arrive at your object by becoming impatient, or talking in a loud voice, but by putting your interrogations in a clear manner, and in language proportioned to the intelligence of the individual.

3. It is often necessary, after asking the first question, "Where do you feel pain?" to tell the patient to put his or her hand on the part. An Irish peasant applies the term "heart" to an indefinite region, extending over great part of the chest and abdomen; and a woman, in speaking of pain in the stomach, often means the lower part of the abdomen.

4. When pain is referred to any circumscribed part of the surface, the part should always be examined by palpation, and, if possible, seen. Rostan relates very instructive cases where the omission of one or the other of these rules has led to curious errors in diagnosis.

5. Although the question, "How long have you been ill?" is sufficiently plain, it is often difficult to determine the period of commencement of many diseases. In acute inflammatory or febrile disorders, we generally count from the first rigor. In chronic affections, a lengthened cross-examination is frequently necessary to arrive at the truth.

6. In endeavouring to ascertain the cause of the disease, great tact and skill in examination are necessary. We must guard ourselves against the preconceived views of the patient on the one hand, and be alive to the possibility of imposition on the other. Sometimes, with all our endeavours, no appreciable cause can be discovered; and at others we find a variety of circumstances, any one of which would be sufficient to occasion the malady.

7. In forming our diagnosis,—that is, in framing a theory deduced from the

facts elicited by examination,—we should be guided by *all* the circumstances of the case, and be very careful that these are fully known before we hazard an opinion. Even then it is not always possible to come to a satisfactory conclusion, and in such cases the diagnosis should be deferred until further observation has thrown new light upon the nature of the disease.

8. In recording a case, it is, for the most part, only necessary to put down, under each head, the symptoms or signs present. If any system be quite healthy, it should be said that it is normal. In many cases, however, it is necessary to state what are called negative symptoms. This demands great tact, and exhibits a high degree of medical information. For instance, an attack of epilepsy generally commences with a cry or scream; but sometimes there is none,—when this should be stated. Symptoms which are usually present in the disease, but are absent in the particular case, constitute negative symptoms.

9. All mention of size should be, according to its exact measurement, in feet and inches. Extent should be determined by proximity to well-known fixed points. All vague statements, such as large, great, small, little, &c., should be carefully avoided.

10. In conversing on, or discussing, the circumstances of the case at the bedside, we should always use technical language. Thus, instead of saying, a man has a cavern at the top of the lung, we should speak of a vomica under the clavicle; instead of saying, a man has diseased heart, we should speak of cardiac hypertrophy, or of insufficiency of the mitral or aortic valves, &c.

Having formed a diagnosis, and prescribed for the patient, the further examination should be conducted at intervals, varying, as regards time, according to the gravity of the case. In addition to the changes which may occur in the signs and symptoms previously noticed, the effects of remedies should be carefully inquired into. Whenever a record of the case is to be kept, I cannot too strongly impress upon you the importance of noting these down in a book at the time, rather than trusting to the memory. For a long series of years the reports of cases, dictated aloud by the professor, and written down at the bedside by the clerk, has formed a leading feature of the Edinburgh system of clinical instruction, and constitutes the only trustworthy method of drawing up cases with accuracy.

When a patient dies, the examination is not completed. The time has now arrived when an inspection of the dead body confirms or nullifies the diagnosis of the observer. You should consider this as a most important part of the clinical course. It is invariably regarded with the greatest interest by those who practise their profession with skill. It is only in this manner that any errors they may have committed can be corrected; that the value of physical diagnosis can be demonstrated and properly appreciated, and the true nature or pathology of diseases, and the mode of treating them rationally, can ever be discovered.

But here, again, method and order are as necessary in the examination of the dead as of the living, and it is of equal importance that no viscus be overlooked. The three great cavities should always be investigated. Nothing is more injurious to the scientific progress of medicine than the habit of inspecting only one of them, to satisfy the curiosity of the practitioner, or to determine his doubts on this or that point. Many medical men direct their attention to a certain class of diseases, and are apt to attribute too much importance to a particular lesion. It has frequently happened to me, when pathologist to this institution, to observe, that after the physician has examined this or that organ, to which he has attributed the death of his patient, and left the theatre, that further examination, according to the routine I always practised, has revealed important lesions that were never suspected. Thus, a person supposed to die of Bright's disease of the kidney, may have a pneumonia that was latent and overlooked. Large caverns and tubercular deposits in the lungs may satisfy the physician, and he may leave the body, when afterwards intense

peritonitis may be discovered, arising from intestinal perforation. A man has hypertrophy, with valvular disease of the heart; he dies suddenly, and everything is referred to the cardiac lesion. On opening the head, an apoplectic extravasation or yellow softening may be discovered. I cannot too strongly, therefore, impress upon you the necessity of always making a thorough *post-mortem* examination, and for this purpose you should, if possible, obtain permission to inspect the body, and not any particular cavity.

The object of a post-mortem examination is threefold,—1st, the cause of death; 2d, an appreciation of the signs and symptoms; 3d, the nature of the disease. These inquiries are very distinct, but practitioners generally have only in view the two first. It frequently happens, that on the discovery of a lesion that seems to explain the fatal termination, they feel satisfied, and there is an end to the investigation. In medico-legal cases, this is the only object. But even here it is necessary to examine all the organs, to avoid a possibility of error, for how can any conscientious man form an opinion, that an abdominal disease has been fatal, if he be not satisfied by inspection that the chest and brain are healthy? Again, it often occurs that a particular sign or symptom is unusual or mysterious, and this, if explained by the examination, is sufficient for the practitioner. But it must be obvious that this throws no light upon the nature of the disease, or its mode of cure. To do this, morbid changes must be sought for, not in that advanced stage where they cause death, or occasion prominent symptoms, but at the very earliest period that can be detected. Hence we must call in the microscope to our assistance, and with its aid follow the lesion into the ultimate tissue of organs; we must observe the circumstances which produced it, as well as the symptoms and physical signs to which it gives rise, the secondary disorders, and the order of their sequence; their duration and mode of termination. This is the kind of extended investigation which can alone be serviceable to the advancement of medicine, and such, I trust, will be the object all of you will have in view, in examining dead bodies. At all events, such are the views that I shall constantly endeavour to place before you during this course of clinical instruction.

The following is an arrangement of the points which demand your attention:—

I. External Appearances.—Number of hours after death. General aspect and condition of the body. In cases of suspected death by violence great minuteness in the external examination is necessary.

II. Head.—Scalp; calvarium; meninges; sinuses; choroid plexus; brain, its form and weight; cerebellum, its weight; cortical and medullary substance of brain; ventricles, exact quantity of fluid in each, which should be removed with a pipette, its character; nerves, and arteries at the base of the brain; sinuses.

III. Spinal Column.—Vertebræ; Meninges; Cord; Nerves.

IV. Neck.—Thyroid gland; larynx and its appendages; trachæa; tonsils; pharynx; œsophagus.

V. Chest.—Thymus gland; lining membrane of bronchi; bronchial glands; pleura; parenchyma of lungs; large thoracic veins; pericardium; general aspect and position of the heart; its weight; right auricle; coronary veins; auricular septum; right ventricle, size of its cavity; thickness and degree of firmness of its walls; endocardium; tricuspid valve; pulmonary artery, its calibre; pulmonary veins; left auricle; mitral valve; left ventricle; thickness and condition of its muscular tissue; size of its cavity; sigmoid valves; coronary arteries; thoracic aorta, its structure and calibre.

VI. Abdomen.—Peritoneum and peritoneal cavity; portion of abdominal viscera; stomach; duodenum; small and large intestines; liver, its weight, form, and structure—its artery, veins, and ducts; gall-bladder and its contents; portal system; pancreas and its duct; mesenteric and other absorbent glands; spleen, its weight, size, and structure; supra-renal capsules; kidneys, weight of each; secreting and excreting portions; pelvis; ureters; bladder, with the prostate and urethra in the male; in the female, uterus, ovaries, Fallopian

tubes, vagina; abdominal aorta and vena cava; ganglia of the sympathetic system.

VII. Blood.—Appearance in the cavities of the heart, in aorta, vena cava, vena portæ, &c.

VIII. Microscopic Examination of all the morbid structures and fluids, the blood, &c. &c.

In carrying out the post-mortem examination, the following hints may be attended to:—

1. As I have already said, the head, chest, and abdomen should always be examined, but the spinal cord and neck need not be disturbed unless the symptoms indicate some lesion there.

2. You should seize every opportunity of opening dead bodies with your own hands, and acquiring dexterity in exposing the cavities, taking out the viscera, &c. Nothing is more painful than to see the brain cut into or contused, in removing the calvarium; or the large vessels at the root of the neck wounded in disarticulating the sternum, so that the surrounding parts are deluged with blood; or the cardiac valves cut through, instead of being simply exposed; or awkward incisions made into the intestines, whereby fæces escape; slipping of ligatures, &c. &c. Coolness, method, knowledge of anatomy, and skilfulness in dissection, are as necessary when operating on the dead as on the living body.

3. In examinations made at private houses, it is not always necessary to remove the viscera. The heart, lungs, liver, kidneys, &c., may be readily examined *in situ*. But here, where every facility exists, the viscera are always taken out, and after describing the morbid alterations they present, I shall always pass them round, so that every one present may examine them.

4. It is a good rule never to omit the examination of a morbid texture or product microscopically, until experience has made you perfectly familiar with its minute structure. This is generally done by the pathologist immediately after the examination.

5. Notes of the examination should always be made at the time. If organs are healthy, this should be distinctly stated, so that hereafter all doubt as to their having been carefully examined may be removed. Here negative appearances are often of as much consequence as negative symptoms.

6. In describing morbid appearances, we should be careful to state the physical properties of an organ or texture, such as the size, form, weight, density, colour, position, &c.; and avoid all theoretical language, such as its being inflamed, tubercular, or cancerous, as well as all indefinite language of small and large, narrow and wide, increased or diminished, &c. &c. Size should always be stated in feet and inches, and the amount of fluid in quarts, pints, or ounces.

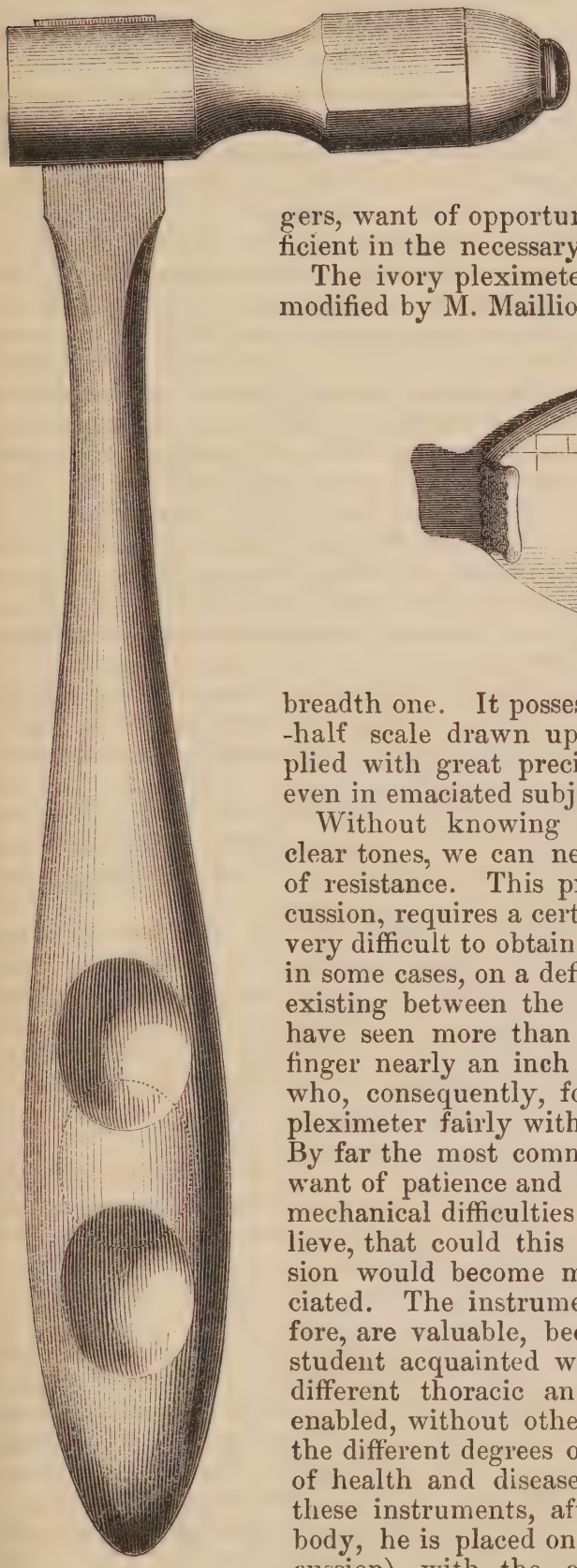
In carrying out the examination of a patient in the manner described, there are three modes of proceeding to which I have alluded that require more especial attention. These are percussion, auscultation, and the use of the microscope. I shall notice each of these *seriatim*,—and first, of

Percussion.

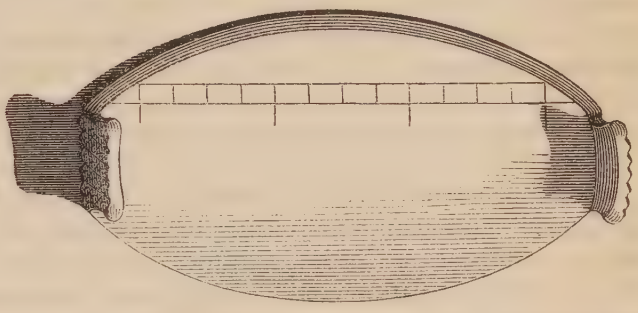
The object of percussion is to ascertain the density and size of organs. It may be practised directly, or through the medium of an interposed body (mediate percussion). Without entering into the numerous discussions which have arisen as to the superior advantages of one plan as compared with another, or of using this or that instrument, I may mention, that for the last nine years I have employed a hammer and a plexiimeter. These instruments I can confidently recommend to you as the readiest means of obtaining accurate results at the bedside by means of percussion.

The hammer is the invention of Dr Winterich of Wurzburgh, and may be procured at all the cutlers' shops in this city. The advantages it possesses are,

1st, That the tone produced, in its clearness, penetrativeness, and quality, far surpasses that which the most practised percussor is able to occasion by other means. 2d, It is especially useful in clinical instruction, as the most distant student is enabled to distinguish the varieties of tone with the greatest ease. 3d, It at once enables those to percuss, who, from peculiar formation of the fingers, want of opportunity, time, practice, &c., are deficient in the necessary dexterity.



The ivory pleximeter I use is that of M. Piorry, as modified by M. Mailliot. Its length is two inches, and



breadth one. It possesses two handles, and an inch and -half scale drawn upon the surface. It may be applied with great precision to every part of the chest, even in emaciated subjects.

Without knowing how to strike, and to produce clear tones, we can never educate the ear, or the sense of resistance. This preliminary part in the art of percussion, requires a certain dexterity, which some find it very difficult to obtain. The difficulty seems to depend, in some cases, on a deficiency in the proportions usually existing between the length of the fingers. Thus, I have seen more than one person who had the index finger nearly an inch shorter than the middle one, and who, consequently, found it impossible to strike the pleximeter fairly with the tips of two fingers at once. By far the most common cause of failure, however, is want of patience and perseverance to overcome the first mechanical difficulties ; and there is every reason to believe, that could this be surmounted, accurate percussion would become more universal and better appreciated. The instruments I recommend to you, therefore, are valuable, because with their assistance every student acquainted with the relative situations of the different thoracic and abdominal organs, is himself enabled, without other preliminary education, to detect the different degrees of sonoriety they possess in a state of health and disease. I may say, that by means of these instruments, after one hour's practice on a dead body, he is placed on a par (as regards the art of percussion) with the generality of experienced practitioners in this country ; and any of you, after one month's employment of them, will be enabled to mark out accurately on the surface of the body, the size and form of the heart, liver, spleen, &c.

Of the different Sounds produced by Percussion.—The sounds produced by percussion arise from the vibrations occasioned in the solid textures of the organ percussed. The different density and elasticity of these textures will, of course, more or less modify the number and continuance of the vibrations, and give rise to different sounds.

M. Piorry considers that nine elementary sounds are thus formed, which he has designated, from the organ or part which originates them, "*femoral, jecoral, cardial, pulmonal, intestinal, stomacal, osteal, humorique,* and *hydatique.*" I consider that all these sounds may be reduced to three elementary ones; that, in point of fact, there are only three tones occasioned by percussion, and that all the others are intermediate. These three tones are respectively dependent, 1st, on the organ containing air; 2d, on its containing fluid; and, 3d, on its being formed of a dense uniform parenchymatous tissue throughout. These tones, therefore, may be termed the *tympanitic,* the *humoral,* and the *parenchymatous.* Percussion over the stomach gives the best example of the first kind of sound; over the distended bladder, of the second; and over the liver, of the third. Certain combinations of these sounds occasion the *metallic* and the *cracked-pot* sound. The terms jecoral, cardial, pulmonal, intestinal, and stomacal, however, may be used to express those modifications of sound produced in percussing respectively the liver, heart, lungs, intestines, and stomach.

No description will suffice to convey proper ideas of the various modifications of tone occasioned by percussing over the different thoracic and abdominal viscera. To become acquainted with these, it is absolutely necessary to apply the pleximeter to the body, and then half an hour's practice with this instrument and the hammer will be sufficient to render any one conversant with those which may be heard in a normal state.

It must be remembered, however, that the tones even then may vary according to circumstances. Thus, immediately after a deep inspiration, the pulmonal sound will be rendered more tympanitic, and, after expiration, more parenchymatous. In the same manner, the stomach and intestines may give out different sounds according as they are more or less full of contents. In the left or right iliac fossa a clear tympanitic sound will be heard when the intestine below is empty, and a dull parenchymatous sound when it is full of fæces.

A study of the different modifications of sound, which various organs thus produce in a state of health, readily leads to the comprehension of the sounds which may be elicited in a morbid state. Thus, the lungs may occasion a dull or parenchymatous sound, from solidification, the result of exudation, or, on the other hand, become more tympanitic, from the presence of emphysema. The abdomen may give out a parenchymatous sound, from enlargement of the uterus or an ovarian tumour; or a humoral sound, from the effusion of fluid in the cavity of the peritoneum.

Of the Sense of Resistance produced by Percussion.—By the sense of resistance is understood the peculiar sensation resulting from those impressions which are communicated to the fingers on striking hard, soft, or elastic bodies. It is of the greatest service in determining the physical condition of the organ percussed. The sense of resistance bears relation to the density of the object struck,—hence firm and solid textures offer more resistance than those which are soft or elastic. Of all the thoracic and abdominal organs, the liver presents the greatest degree of resistance, and the stomach the least. The presence of fluid in the hollow viscera, offers a medium of resistance between the parenchymatous organs on the one hand, and those containing air on the other. But air much condensed, or fluid contained within the rigid walls of the thorax, may offer a considerable degree of resistance.

The sense of resistance should be as much educated by the physician as the sense of hearing, and it would be difficult for an individual, practised in the art of percussion, to say which of these two points is the more valuable to him. Both are only to be learnt by practice, and considering it perfectly useless to describe that in words which may be learnt in half an hour, by the use of the

pleximeter and hammer on a dead body, or the living subject, I shall now proceed to describe the

General Rules to be followed in the Practice of Mediate Percussion.—1. The pleximeter should be held by the projecting handles between the thumb and index finger of the left hand, and pressed firmly down upon the organ to be percussed. Much depends upon this rule being followed, as the sound and sense of resistance are considerably modified according to the pressure made by the pleximeter. A very easy experiment will prove this. If, for instance, the pleximeter be struck while it rests lightly on the abdomen over the umbilicus, and again, when it is pressed firmly down amongst the viscera, the change in tone will be at once perceived. In the first case, a dull sound is produced, from the muscles and integuments being alone influenced by the force of the blow; in the second case, a clear tympanitic sound is occasioned from the vibration of the walls of the intestine. In every instance, therefore, the pleximeter should be so held and pressed down, as to render it, so to speak, a part of the organ we wish to percuss.

2. Great care must be taken that no inequality exist between the inferior surface of the pleximeter and the skin. Firmly pressing it down will always obviate this when the abdomen is examined. As regards the thorax, the groove over the anterior mediastinum, the prominence of the clavicles and of the ribs, in emaciated subjects, may allow a hollow to exist under the instrument, by which a deceptive tympanitic sound is occasioned. By a little management, however, with the small and oval pleximeter I have recommended, this may readily be avoided.

3. The hammer should be held, as advised by Dr Winterich, between the thumb and the first and third fingers, the extremities of which are to be placed in the hollows prepared for them in the handle of the instrument. By some these are considered useless, but in all cases where slight differences in tone are to be appreciated, I have found this the best mode of employing it. Ordinarily, however, it will be sufficient to hold it by the extremity of the handle, merely in such a manner as will enable the practitioner to strike the pleximeter lightly, or with force, as occasion may require.

4. Care must be taken to strike the pleximeter fairly and perpendicularly. Unless this be done, vibrations are communicated to textures in the neighbourhood of the organ to be percussed, and fallacious results are the consequence. If, in percussing the lungs, for example, the blow be made obliquely, we obtain the dull sound produced by the rib, and I have seen considerable error in the diagnosis thus occasioned.

5. A strong or gentle stroke with the hammer will modify the tone and sense of resistance, inasmuch as the impulse may be communicated by one or the other to a deep-seated or a superficial organ. Thus a gentle stroke will elicit a pulmonal tympanitic sound just below the fourth rib, where a thin layer of lung covers the liver, but a strong one will cause a jecoral parenchymatous sound. At the inferior margin of the liver, on the other hand, where a thin layer of the organ covers the intestines, the reverse of this takes place, a gentle stroke occasioning a dull, and a strong one a clear sound.

6. By withdrawing the hammer immediately after the blow, we are better able to judge of the sound; by allowing it to remain a moment, we can judge better of the sense of resistance.

7. The integuments should not be stretched over the part percussed, as when the stethoscope is employed, for an unnatural degree of resistance is thus communicated to the hand of the operator from the muscular tension. In every case, especially where the abdomen is examined, the integuments and superficial muscles should be rendered as flaccid as possible.

8. It is always best to percuss on the naked skin. It is not absolutely essential, however; and in cases where, from motives of delicacy, it is desirable that the chest or abdomen be not exposed, it only becomes necessary that the co-

vering of linen or flannel be of equal thickness throughout, and not thrown into folds.

9. The position in which the individual examined should be placed, will vary according to the organ explored. In percussing the thoracic organs and the liver, a sitting position is most convenient. The stomach, intestines, uterus, bladder, and abdominal tumours or effusions, are best examined when the patient is lying on the back, with the knees flexed so as to relax the abdominal walls, and, if necessary, the head and neck bent forward, and supported by pillows. In percussing the spleen, the individual should lie on the right side, and when the kidneys are examined, he should lie on the breast and abdomen. In cases of effusion into the serous cavities, a change of position furnishes most valuable indications.

10. In percussing any particular organ, the pleximeter should be first applied over its centre, where the sound and sense of resistance it may furnish, are most characteristic. Two blows with the hammer are generally sufficient to determine this. From the centre, the pleximeter should be moved gradually towards the periphery, or margin of the organ, and struck as it proceeds with the hammer, now forcibly, now lightly, until the characteristic sound of the next organ be elicited. The pleximeter is then gradually to be returned towards the organ under examination, until the difference of tone and sense of resistance become manifest. In this manner having first heard the two distinct sounds well characterised, we shall be better enabled to determine with accuracy, the limit between the one and the other. This may be done exactly after having determined, whereabouts the line of separation is, by placing the long diameter of the pleximeter transversely across it, and striking, first one end of the instrument, and then the other, till the precise spot is determined. This spot should now be marked, by placing with a pen a dot of ink on the skin. The opposite and then other portions of the margin of the organ should be limited in the same manner, and these in turn should be marked with dots of ink, until the whole organ be completely examined. Then by uniting all these dots with a line of ink, we have the exact form of the organ drawn upon the skin. When it is thought necessary to render the first mark permanent, in order to see if any subsequent change take place in the size of the organ, or extent of the dulness, it may be rendered so, by carrying lightly a stick of argent. nit. over the ink line, while it is still moist.

Special rules to be followed in percussing particular organs.—The short rules and practical remarks about to be given, are derived partly from the precepts of M. Piorry, as I have heard him deliver them in the wards of La Pitié, and partly from my own experience.

Lungs.—Percussion of the lungs generally bears reference to a change in density, which is only to be detected by comparing the healthy with the morbid portions. The great practical rule here to be followed is, to apply the pleximeter to both sides of the chest in succession, with the same firmness, exactly in the same situation, and let the blow with the hammer be given with the same force. Care must be taken that the position of both arms be alike, as the contraction of the pectoral muscles on one side more than on the other may induce error. In short, every circumstance must be the same before it is possible to determine, in delicate cases, either from the tone or sense of resistance, whether change of density exist in the lungs. When circumscribed alterations are discovered in the pulmonary tissue, their limits may be marked out on the surface of the skin, in the manner previously indicated. In this way I have frequently succeeded in determining with accuracy the size and form of circumscribed indurations, arising from partial pneumonia and pulmonary apoplexy. Under the clavicles, the pleximeter must be applied with great firmness. Inferiorly, a thin layer of lung lies over the superior surface of the liver; and to determine the exact place where its inferior border terminates, the blows with the hammer should be very slight. Posteriorly, also, the pleximeter must be firmly applied, and the force of the blows considerable;

but they should decrease in force inferiorly, where a thin layer of lung descends over the liver much deeper than anteriorly.

In a healthy state, a distinct difference may be observed in the sonoriety of the lungs immediately after a full expiration and a full inspiration. This does not take place when the tissue becomes indurated from any cause; and thus we are furnished with a valuable diagnostic sign. Congestion of the lung, and pneumonia in its first stage, cause only slight dulness and increased resistance, which, however, are readily detected by the practised percussor. In the second and third stage of pneumonia, and in apoplexy of the lung, this dulness and resistance are well-marked, and even an impression of hardness and solidity communicated to the hand. When, however, the lung is studded with tubercles, the induration is most intense, and the greatest degree of resistance communicated.

Partial indurations from apoplexy, or simple cancerous and tubercular exudation, may be detected by percussion, even when deep-seated and covered by healthy portions of the lungs. In this case, by pressing with the pleximeter, and striking lightly, a tympanitic sound only is heard; but by pressing the pleximeter down firmly, and striking with force, the dull sound may be elicited and circumscribed. When indurations, however, exist inferiorly in those portions of the lungs which overlap the liver, it requires great practice to detect them with certainty. Caverns in the lungs, when large and filled with air, induce a tympanitic sound; but they are generally more or less full of viscous and fluid matters, and give rise to dulness.

Two or three ounces of fluid may be detected in the pleural cavity, by causing the patient to sit up. The height or level of the fluid is readily determined, and should be marked daily by a line made with nitrate of silver. If the effusion be only on one side, the increased dulness is more easily detected. It disappears on placing the patient in such a position as will cause the fluid to accumulate in another part of the pleural cavity, when the space which was previously dull becomes clear. When the effusion entirely fills the pleural cavity, no limit, of course, can be detected; but, even then, the dulness is distinguished from that of the liver by the diminished feeling of resistance.

When air is effused into the pleura, the sound is like that of a drum, and readily detected.

Heart.—To mark out the precise limits of the heart, constitutes the first difficult lesson in the art of percussion. M. Piorry commences by determining the clear sound at the upper end of the sternum, and bringing the pleximeter gradually downwards till the dull sound of the heart be heard. I have found it best to place the instrument first under the left nipple, where the cardial dulness is most intense; then to draw it upwards, striking it continually with the hammer until the clear sound of the lung be elicited; then, by bringing it down again towards the heart, we shall readily distinguish the line where cardial dulness commences, and thus limit the superior margin of the organ. The same method is to be followed in determining the situation of the lateral margins, only drawing the pleximeter outwards or inwards, striking more and more forcibly with the hammer, until the clear tympanitic sound of the lung only be heard. It is more difficult to determine the situation of the apex of the heart; for as this rests on the diaphragm, and this again upon the left lobe of the liver, it cannot readily be distinguished from them. The size of the heart, however, may be pretty accurately estimated, by limiting its superior and lateral margins. In females, the left mammary gland should be drawn upwards and outwards by an assistant.

The normal size of the heart differs in different persons. As a general rule, however, it may be considered that, if the transverse diameter of the dulness measure more than two inches, it is abnormally enlarged. It has been known to measure seven inches. (Piorry.) In hydropericardium, the dulness has been remarked to exist rather at the superior part of the sternum,

than on one side or the other. (Piorry, Reynaud.) In hypertrophy and dilatation of the right ventricle, the increased extent of the dulness stretches towards the median line, and sometimes passes over it. In similar hypertrophy of the left ventricle, the dulness extends on the left side more or less, according to the increased size of the heart. In concentric hypertrophy, there is little or no enlargement, but the density is greatly increased, which is readily detected by the feeling of resistance.

The presence of tubercles in the lungs surrounding the heart ; aneurisms or other tumours pressing upon, or in the neighbourhood of, the organ ; hypertrophied liver, extensive empyema, &c. &c., may render its mensuration difficult or impossible.

Liver.—Limitation of the size of the liver should be commenced by placing the pleximeter over the organ on the right side, where the dulness and resistance are greatest. It should then be carried upwards, until the clear sound of the lung be distinguished, when it ought again to be brought down, and the limit marked. This limit, however, may indicate either the inferior margin of the lung, or superior convex surface of the liver. Now, as a thin layer of lung descends in front of the liver, it will be necessary to determine where the tympanitic sound ceases inferiorly, by striking gently with the hammer, and where the parenchymatous sound ceases superiorly, by striking forcibly, so that vibrations may be communicated to the organ through the layer of lung. The space between these two lines thus marked on the surface is wider in some individuals than in others, and deeper and more extensive posteriorly than anteriorly. By carrying the pleximeter from the right side anteriorly, and then posteriorly towards the left of the patient, the whole superior margin may be thus detected, and marked with ink upon the surface, except where the liver comes in contact, through the medium of the diaphragm, with the apex of the heart. The inferior margin is for the most part readily detected. It must be remembered, however, that in the same manner as a thin layer of lung covers the upper margin, so a thin layer of liver descends on the right side over the intestine. It is, therefore, necessary to be cautious in determining the inferior margins, for a tolerably strong blow with the hammer will give rise to a tympanitic sound from the intestine, heard through the liver. The lower margin must be percussed in an inverse manner to the superior, and as we proceed downwards the force of the blow should be diminished. The inferior margin of the liver is in general readily detected, from the contrast which its dulness and density produced on percussion present, contrasted with the tympanitic and elastic feel of the intestines and stomach.

The superior limit of this organ is generally found about two inches below the right nipple, or corresponding with the fifth rib. Its inferior border descends to the lower margin of the ribs. The extent of the jecoral dulness in the healthy state is in general two inches on the left side, three inches in the hepatic region anteriorly, and four inches in the hepatic region laterally. (Piorry.)

Variations in the size of the liver, from congestion, inflammation, abscesses, hydatids, tumours, atrophy, &c. &c., may often be exactly determined by means of percussion. In icterus, the increase and diminution of this organ, as evinced by lines marked on the skin, will generally be found to bear a proportion to the intensity of the disease. When tumours are present, the inferior border often presents an irregular form. If the inferior lobes of the lung be indurated by tubercles or hepatisation, it becomes difficult or impossible to draw the limit between them and the liver. When fluid effusion exists in the pleura, the increased density of the liver still serves to distinguish it, through the humoral sound of the fluid ; and, by changing the position of the patient, its upper edge in the majority of cases may be limited. In cases of ascites, we must lay the patient on the left side, in order to measure the right lobe, on the right side to measure the left lobe, and on the abdomen to percuss it posteriorly. Sometimes the right lobe of the liver is so enormously hyper-

trophied, that its inferior margin extends nearly to the right iliac fossa. (Piorry.)

When the gall-bladder is much distended with bile, or contains gall-stones to any amount, it may readily be detected by percussion, and the dulness it occasions immediately under the inferior margin of the liver, anteriorly and somewhat laterally, be marked off. M. Piorry is enabled to do this in almost every case: but I must confess that I have often failed in detecting dulness caused by distended gall-bladder.

Spleen.—In percussing the spleen, it is necessary that the patient lie on the right side, and it is advantageous that the examination be made before, rather than after, meals. Anteriorly the sonoriety of the stomach and intestines causes the margin readily to be distinguished. Posteriorly, however, where the organ comes in contact with the kidneys, this is impossible. Its superior and inferior margins may be made out by striking the instrument with some force, and following the rule (No. 10.) previously given. This organ offers great resistance on percussion.

The general size of the spleen is about four inches long and three inches wide. (Piorry.) In diseased states it may be atrophied or enlarged. I have seen it measure upwards of twelve inches long and eight wide. A pleuritic effusion, ascites, pneumonia, or tubercular deposition in the inferior lobe of the left lung, may render a limitation of this organ difficult or impossible. If the dulness cannot be detected, we may infer that its dimensions are small. (Mailliot.)

Stomach and Intestines.—The sounds elicited by percussion of the stomach and intestines, are of the greatest service to the practitioner: 1st, As furnishing him with the means of determining the form of other organs, as the liver, spleen, or bladder; 2dly, As enabling him to distinguish the presence or absence of fæcal or alimentary matter; and, 3dly, As the means of diagnosing abdominal tumours. Hence it is incumbent on every physician to be able at once to recognise the difference between the tones furnished by the stomach, small and large intestines, under various circumstances. To arrive at this knowledge, it is necessary to be acquainted with the relative positions of the different abdominal viscera, and the regions of the abdomen to which they correspond.

In exploring the abdomen by means of percussion, the pleximeter should first be placed immediately below the xiphoid cartilage, pressed firmly down, and carried along the median line towards the pubes, striking it all the way, now hard, now gently, with the hammer. The different tones which the stomach, colon, and small intestines furnish, will thus be distinctly heard. The pleximeter should then be carried laterally, alternately to the one side, and then to the other, till the whole surface be percussed. In this manner, the different tones produced by the cœcum and ascending colon on the right side, and descending colon on the left, will be respectively distinguished from that furnished by the small intestines. The sounds and sense of resistance will be modified according as the different viscera are full or empty, as any one can determine on his own body by means of the pleximeter and hammer. When the intestines are full of fluid or solid contents, such portions may be circumscribed and marked out on the surface of the skin. I have thus often succeeded in determining the internal margin of the colon, in its ascending, transverse, or descending portions. Sometimes a portion of intestine is found lying between the abdominal walls and the stomach. The latter, however, may be readily limited, by pressing down the pleximeter, causing the patient to eat or drink, or by examining after dinner. The small intestines are almost never deprived of the tympanitic sound—a circumstance by which they may readily be distinguished from the stomach and large intestines. The distance of any particular knuckle of intestine from the abdominal walls, may be pretty accurately calculated, by the force necessary to be employed in pressing down

the pleximeter, and striking with the hammer, in order to elicit a tympanitic or dull sound.

It is unnecessary to point out the numerous circumstances, and morbid conditions, in which percussion of the abdomen may prove useful in practice. Displacements of the stomach or intestines, femoral and scrotal hernia, mesenteric, ovarian, and other tumours, peritoneal adhesions and effusions, may all frequently be diagnosed, and their limits determined, by a careful examination with the pleximeter and hammer. By means of percussion, even the nature of the tumour may often be arrived at; as for instance, whether it be fungus hematodes, scirrhous, encysted, osseous, &c., by the different degrees of resistance they possess. Care, however, must be taken not to confound with tumours an enlarged spleen or liver, a distended uterus or bladder, stomach full of alimentary matter, &c.

In a practical point of view, it is often useful to determine, by means of percussion, whether an enema or a purgative by the mouth is likely to open the bowels most rapidly. If, for instance, there be dulness in the left iliac fossa, in the track of the descending colon, that part of the intestine must be full of fæces, and an enema is indicated. If, on the other hand, the left iliac fossa sound tympanitic, and the right sound dull, an enema is of little service, as it will not extend to the cœcum, and purgatives by the mouth are indicated.

Effusion of fluid into the peritoneum may be determined with great exactitude by means of percussion, and the height of the fluid marked, as in the case of pleuritic effusion. In the same manner, a change of position furnishes similar results.

Bladder.—This viscus is only to be detected by percussion, when it is more or less distended, and rises above the pubis. It may then be distinguished, and its circular margin limited, by observing the tympanitic sound of the intestines, on the one hand, and the dull humoral sound furnished by the bladder, with increased resistance, on the other. When covered by intestines, it will be necessary to press down the pleximeter with tolerable firmness, but not in such a manner as to give the patient pain. In the infant, the situation of the bladder is not so deep in the pelvis, and a small quantity of fluid renders it cognizable by means of percussion.

A ready approximation of the state of the bladder will be found of great service in cases of fever, apoplexy, delirium, imbecility, paraplegia, &c. &c. In several cases it has been found dangerously distended, on percussing the abdomen to determine the state of the intestines.

I have here only noticed those points and circumstances in the art of percussion which may be readily accomplished, and which every one may master in a few months by care and attention. For a description of the more delicate points, such as percussion of the kidneys and fœtus, accurately limiting the left from the right ventricle, determining and marking out the ascending and transverse portions of the arch of the aorta, &c., I must refer you to the admirable works of MM. Piorry[1] and Mailliot.[2]

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[1] De la Percussion Médiate, &c., Paris, 1828. Du Procédé Opératoire, Paris, 1831. De l'Examen Plessimêtrique de l'Aorte Ascendante, et de la Crosse Aortique, &c. Archives Gén. de Méd., vol. ix., 1840, p. 431.

[2] Mailliot (L.) Traité de la Percussion Médiate, &c., translated into English, with notes, by Dr George Smith of Madras.

Part Fourth.

PERISCOPE.

MEDICINE.

ON SPASMS AND CONVULSIONS OF CHILDREN.—BY DR AUGUST SCHŒPF MEREI, PROFESSOR OF MEDICINE AT PESTH. 1849.

[The following practical observations were originally written in the French language, during the author's recent absence from Pesth. They were kindly placed at the disposal of the conductors of the Monthly Journal, under whose direction the English translation has been executed. Dr Schœpf Merei enjoys on the Continent a high reputation for his acquaintance with the whole subject of children's diseases, for the special study of which he has long possessed excellent opportunities, and on which he lectures with great ability.]

Among the diseases which decimate infancy, and whose treatment constitutes the most arduous task of the practitioner, the functional disorders of the cerebro-spinal system occupy an important place. It is especially in the tenderest period of childhood, the first year, that these nervous affections, so obstinate and destructive to life, abound; and the disposition to spasms and convulsions at this early age is so very general and intense, that even children of the best constitution are, without any appreciable cause, sometimes subject to the most severe nervous fits.

Many points in the special pathology of these affections are as yet very imperfectly developed. For my own part, I can but attempt to supply some data of experience and inquiry on this subject; the simple narrative of what I have seen, among the many thousands of patients treated in the children's hospital of Pesth, and in the course of an extensive private practice.

1.—*The Abdominal Spasms of Children newly-born or of tender age.*

I shall first treat of a *neurosis* not dangerous to life, but which constitutes the most painful scourge of tender childhood—*abdominal spasms.* This affection is generally abandoned to the domestic treatment of mothers and nurses. I have even known physicians, who esteemed the subject as more properly belonging to the department of domestic and popular medicine, than to that of scientific inquiry and practice. Nevertheless, the abdominal spasms of children seem to me well worthy of our attention. Indeed, there is such gradation, such variety and extent, between the minimum and maximum of pain—such frequency of complications with elements both material and purely nervous—such difficulty in appreciating what is due to mere predisposition and nervous derangement, and what to occasional local causes whose usual seat is in the bowels, that the survey of the subject, clear though we fain would make it, is not exempt from obstacles, and its accomplishment is a very difficult task.

I am almost convinced, that of every hundred children at the breast, from fifteen to twenty are more or less subject to these spasms, and consequently cause disturbance and affliction to their mothers, or to those in charge of them. Yet, as we very often find that these affections continue their attacks for a long time, and then pass away without danger, and without the use of remedies, they are consequently apt to be neglected; or it happens, that an abdominal inflammation is mistaken for a nervous pain, and by such a mistake the life of a child is often sacrificed.

Our attention is also drawn to this *neurosis*, by the dominion exercised by it over so long a period of the most tender age. While other forms of nervous

disorder are more or less confined to a certain short period of childhood,—for instance, trismus to the first few days after birth, spasmus laryngeus to the period between the fourth and eighth month, eclampsia by preference to the earlier periods of dentition,—we observe the abdominal spasms sometimes to begin with the first day of life, and to continue in fits more or less frequent and prolonged till about the end of the first year,—sometimes, though rarely, even till the eighteenth month.

Children affected with these nervous pains are observed to cry, often violently, and at the same time to struggle with their hands and feet. The breast does not appease them for any length of time ; and when they take it, their suckling is frequently interrupted.

A spoonful of infusion of chamomile or of seeds of fennel, frictions over the abdomen with warm flannel perfumed with fennel, with the *unguent. nervinum vel roris marini comp.*, of the old pharmacopœias, or with the etherial oil of rue,—a clyster composed of infusion of chamomile and a spoonful of fresh oil : such are the means by which the mothers in this country (Hungary) often succeed in appeasing the sufferings of their children. We also frequently observe the cessation of the fits without the use of physic. Their duration is of various length, from some minutes to several hours ; and the same may be said of the intervals. But I have seen great numbers of children, who have passed three-fourths of their first year, crying more or less violently from twelve to sixteen hours every day ; and, what is very astonishing, some of these, in defiance of their continual sufferings, have continued to grow very well—a certain proof that this kind of nervous affection remained limited to the spinal system.

Symptoms.—When the abdominal spasm attains its highest degree of violence, the infant, if lying on its back, and without clothes to hamper its movements, presents the following symptoms :—Continual and very violent crying ; increased redness of the skin ; lividity, or redness of the face ; eyes forcibly shut ; cheeks puffed out ; thighs drawn up to the abdomen ; forearms flexed upon the arms ; hands strongly closed and pushed into the mouth. The pulse beats from 80 to 140, but at this early age affords no trustworthy diagnostic indication. The crying consists of prolonged and very violent screams, alternating with efforts of inspiration (" reprises "), which are deep, more or less short, not sonorous, and consequently free. From this I infer, that the spasm implicates neither the lungs, the larynx, nor the glottis ; and this observation is important, for these spasms are not dangerous while the respiratory apparatus remains unaffected.

Terminations.—If, on the other hand, the child's cries are of an opposite character—*i. e.*, if the expirations or screams are short and faint, while the back-draughts or inspirations are prolonged, forcible, and sonorous, then there is danger ; for in this case the spasm of the abdomen has extended to the larynx and glottis, and by obstructing the air-passages it sometimes causes suffocation. This affection is, in fact, the spasmodic asthma of Kopp, which consists in spasmodic constriction of the larynx.

I have in some rare cases observed the transition from abdominal to laryngeal spasm in children under four months old ; but in almost all the instances in which I have witnessed it, the *neurosis* did not attack the larynx alone, but soon manifested its influence elsewhere, by convulsive movements of the eyes and of the muscles of the face ; and death by tetanus, or by general convulsions (eclampsia), commonly supervened. This fatal termination is most frequent in infants from two to eight weeks old, and of delicate constitution.

Sometimes the abdominal spasm passes at once into eclampsia or into tetanus, without the intermediate step of laryngeal spasm. I repeat, however, that these fatal transitions are in general observed only in infants of very tender age and delicate constitution, or at the commencement of dentition.

Causes.—The frequency of abdominal spasm is most considerable between the second and fifth month, after which it diminishes, so that of a hundred infants, probably not more than five suffer from this cause after they have completed the ninth month of their age. The cause of these proportions seems in great part

due to the multiplication of sources of faulty nutrition about the second month of infant life; for about this period many women commence to give to their children, for the purpose of fattening them, as they say, different kinds of pultaceous food, compositions of farinaceous articles with milk, cakes, &c.; digestion is thereby deranged, acidity and gas generated in the alimentary canal, and the irritation of the intestinal nerves thus induced provokes the spasms. If the child survives these derangements of the digestive system, which are sometimes very serious and continuous, and associated with vomiting and diarrhœa, then about the ninth or twelfth month, when he has already acquired some strength, in spite of the pernicious influences to which he has been subjected, the abdominal spasms become rare, or totally cease.

Let us inquire more narrowly into the internal and external causes of these spasms. There can be no doubt that the predisposition to abdominal spasm is in the first nine months of life very strong and very general. If there be an additional hereditary special susceptibility derived from a very nervous mother, this will constitute the most powerful of *internal* causes. Among external and occasional causes, we may enumerate violent passions, and in particular, fits of anger, and other influences which may, during the period of lactation, derange the wholesome quality of the milk, different errors in diet, and consequent disordered state of the digestion and of the excretions. Exposure to cold may, in my opinion, induce rheumatism or inflammation, but not abdominal spasm. What I wish to insist upon here is, that the very frequency of this affection is sufficient to show a general predisposition to it on the part of infants of tender age—a predisposition so strong, that we very frequently witness spasmodic attacks which we cannot trace to any appreciable external causes. It is therefore the extreme irritability of the abdominal portion of the spinal system of nerves at the early period of life, which I believe to be the chief cause of the affection in question. How often have I observed fits of abdominal spasm in very young infants whose digestive functions seemed normal; and on the other hand, in riper years, how frequently is a deranged state of digestion observed without the slightest spasm! Why, in fact, should spasm be rare after the eighteenth month, or in children of two or three years of age, when it is certainly out of all proportion rarer than deranged digestion is? And why should deranged digestion almost never induce these spasms in adults? After the eighteenth month, if there is gastric derangement, the pain is in general null or very transient,—diarrhœa or vomiting is soon excited and gives relief,—and proper regulation of the diet, if the affection be not rheumatic, soon effects a cure. But in young infants, on the contrary, in spite of evacuations, and in spite of the most severely restricted diet, we frequently see these *neuroses* persist for several months.

Pathology.—The pathological examination of a considerable number of infants, who during their life suffered from abdominal spasm, but whose death was caused by very different diseases, has satisfied me that abdominal spasm may persist for a considerable period, without occasioning alteration in the tissues of the alimentary canal, or in the spinal and splanchnic nerves. I remember one case, which occurred in 1844, and which was very remarkable. The infant was aged nineteen months, and died of diarrhœa and atrophy connected with scrofula, which unequivocally manifested itself about the ninth month. From the very day after its birth, this infant had daily paroxysms of abdominal spasms. With the assistance of several young physicians, a most careful examination was made of the spinal cord, of the sympathetic and other nervous branches distributed to the intestines, and no lesion was detected. Similar results have been obtained by me on many other occasions.

Necroscopic examinations have also demonstrated that abdominal spasm may develop itself in infants suffering from any chronic affection, not even excepting chronic inflammation, more or less extensive, of the mucous or serous

membranes of the intestinal canal. *But during the course of an acute febrile or inflammatory attack the abdominal spasms are suspended.*

I shall next endeavour to state the results of the dissections which I have conducted, arranging them as far as possible in accurate numerical proportions. Of infants, aged from one week to two years, who during life had suffered in a very marked degree from abdominal spasms, $\frac{5}{100}$ths died of eclampsia, without other disease; $\frac{25}{100}$ths, of chronic inflammation of abdominal viscera; $\frac{30}{100}$ths, of scrofula in some form, or of diseases induced by scrofula; $\frac{10}{100}$ths, of acute typhoid, or inflammatory diseases; $\frac{5}{100}$ths, of acute hydrocephalus; $\frac{25}{100}$ths, of different chronic affections.

The *nature* of the abdominal spasm may easily be judged of from what has now been stated. There exists in all cases of this affection an extreme irritability of the abdomino-spinal system; and in the majority some of the external accessory causes above alluded to are superadded. I have purposely tried, at some length, to demonstrate the predominance of the nervous element in this disease, because my remarks may convey a hint to the practitioner not to treat cases of spasms complicated with gastric derangement exclusively with remedies directed against the condition of the stomach. It may, perhaps, be asked, whether the affection in question is not, very frequently at least, rheumatic—a rheumatism of the abdomino-spinal nerves? When treating of the diagnosis, I shall have occasion to state my views on this head.

The *local manifestation* of the disease consists in a spasmodic muscular contraction of a certain portion of the stomach, of the intestines, or of the urinary bladder—or, to speak more precisely, of its sphincter. My opinion is, that what is commonly called "pain in the belly" only differs in degree from the disease just described. Infants can give no description of the nature of the pains which afflict them; still I am strongly inclined to believe that neuroses of the sensitive system—abdominal neuralgia—must in early life be very rare. In fact, the pains suffered by infants are almost always connected with constriction of the alimentary canal, or of the bladder, as is proved by the uniform retention of the evacuations observed when the pains are urgent.

There are, then, abdominal spasms purely nervous, and complicated,—and of complications the most usual are vomiting, diarrhœa, constipation, acidity, eructations, and flatulence; there are, besides, cases complicated with some chronic inflammatory affection of the abdominal viscera. The transition into, or complication with, an acute inflammation of the bowels or bladder, must, according to my observation, be exceedingly rare. And here I must make a slight digression into the field of therapeutics, in support of the pathological doctrines which I have enounced.

In the earlier years of my practice in the children's hospital, when I had to do with cases of very violent spasms, in which the severity and duration of the paroxysms rapidly increased, if the child was robust or even of moderately strong constitution, I used to have recourse to detraction of blood from the abdomen. This measure was often immediately followed by cessation of the spasms. But I cannot admit that there had been a transition from the state of spasm to that of inflammation. Indeed, I might cite several cases in support of this opinion; I shall content myself with two.

Case I.—Jean P., a child of one of my friends at Pesth, and four months old, suffered from very frequent attacks of intestinal spasm. One day I was thrice applied to, in consequence of its very violent and continuous attacks of pain. The child had, for eighteen hours, and almost without intermission, suffered from the most exquisite form of intestinal spasm, and that notwithstanding the use of the most efficacious remedies,—viz., purgative enemata containing oil, followed by two doses of castor oil given by the mouth, some drops of sp. melissæ, and finally a drop of tincture of opium given in a warm infusion of chamomile and fennel seeds. The train of symptoms corresponded with the description given at p. 360. But, as there was considerable increase of the temperature of the skin, and a pulse beating 135 per minute; and as similar symp-

toms had formerly in the same infant given way when blood was withdrawn, five weeks before, I ordered four leeches to be applied about the umbilicus. The spasms ceased while the blood was still flowing, and the infant seemed well. Fifteen hours afterwards I was again sent for, to witness a similar paroxysm, which in eight hours terminated by eclampsia and death.

On dissection, no injection of the serous or mucous membranes of the alimentary canal, or of the bladder, was discovered; neither was there congestion of any abdominal viscus.

Case II.—Jules R., a child aged five months, was robust and always healthy. The mother was of exceedingly nervous temperament. One night I was sent for to see the child, which was seized with a violent fit of pain. It had been crying violently for several hours. Its hands were closed, trembling, and thrust into the mouth; its lower extremities continually in motion; the abdomen very tense and hot; pulse 120. Purgative enemata, and warm cataplasms, containing narcotics, were used; and ¼ grain of Dover's powder was twice given in four hours. Slight narcotism supervened, but the symptoms were unrelieved. Three leeches were then applied to the abdomen. In fifteen hours after the commencement of the fit, the child died by general convulsions. A careful dissection was made, and no morbid appearance was detected.

I could give many cases where, after the local bloodletting, there was prompt amelioration. But what would that prove? It is a fact, that nervous pains, especially in robust individuals, sometimes cease when blood is withdrawn. This observation is not unfrequently to be verified in cases of periodic nervous headach ("migraine") occurring in females. The ancients used to say, "*Ubi dolor, ubi adfluxus;*"—pain, then, may be the cause of congestion, and this, if it does not pass into inflammatory action, may not after death be visible to the eye of the anatomist. This, however, is not the place for a lengthened discussion on such a subject. It is sufficient simply to state, on the authority of a large number of observations, that well-marked abdominal spasm very rarely passes into inflammation; and, when this termination does arise, it is connected with some external circumstance capable of engendering inflammation,—*e. g.*, the application of cold. I must add, that, in the great majority of cases, detraction of blood causes no relief to abdominal spasm.

The intensity and duration of the paroxysms are very variable. In general the duration of the fit is inversely as its intensity. This is illustrated in cases of local spasm of the stomach and urinary bladder, in which the duration of the fit seldom exceeds fifteen minutes. Intestinal spasm, however, is less painful, but its paroxysms are often very long.

Diagnosis.—The spasms seldom affect a very extensive portion of the abdominal viscera; in the majority of instances they localise themselves in a more or less circumscribed portion of one or other of the following organs,—viz., the stomach, the intestines, and the urinary bladder. It is not very easy to recognise and diagnose any one of these three forms of disease; for the above-described cries and movements of the child are often pretty much the same in all. The differential diagnosis is not alluded to by authors; for the subject of abdominal spasm has not acquired in medical literature the place which it deserves. I flatter myself that, in the remarks which I am about to make, some useful practical information will be conveyed.

I. *Spasm of the Stomach.*—Violent crying, as described at p. 360. The infant sometimes moves, as if it wished to raise itself,—*i. e.*, it bends the head and breast forward towards the abdomen. The violence of the symptoms usually continues pretty uniform till the complete cessation of the paroxysm. There then occur eructations, sometimes exhaling an acid odour; there may even be vomiting of the mucus secreted in the empty stomach. It is not rare to see children pass at once from a paroxysm of pain and tears into a state in which their expression and laughter indicate perfect ease. The best marked fits of this description do not in general last longer than from ten to twenty minutes;

and the intervals between the fits are longer and more perfect than in the other varieties of the affection.

II. *Spasm of the Intestines.*—Symptoms, common to all varieties, described at p. 360, less intense; some remissions during the paroxysms; legs strongly drawn up to the belly; paroxysms longer, and intervals shorter; fits terminating gradually, and very frequently succeeded by discharge of flatus from the bowels, or by diarrhœa. I have met with many cases of this description, in which the paroxysm lasted for several hours, with remissions more or less distinct, and in which the infant hardly enjoyed half an hour's rest before a fresh attack supervened.

III. *Spasm of the Bladder.*—The violence of the symptoms, the form of the paroxysm, its duration and termination, resemble the description given of spasm of the stomach; but the infant moves the pelvis much, and sometimes seems to wish to elevate it, by fixing the feet and upper part of the trunk, and then making muscular efforts. When the fit is over the urine begins to flow, at first drop by drop, and afterwards more freely. The child is then quite tranquil and happy. The urine in these cases very often leaves reddish-yellow stains upon linen,—a sign of an excess of acid in it. Paroxysms of this kind seldom last above ten minutes. The intervals are usually perfect, and the fits seldom occur oftener than from five to eight times in a day. Before the second month it is a rare disease.

These three varieties are, however, not constantly confined to the localities in which they are originally seated. In particular, when the sole cause of spasm consists in the child's extreme nervous susceptibility, without notable derangement of the digestive or urinary apparatus, one variety is frequently seen to pass into another, *i. e.*, spasm may pass from the bladder to the stomach, or *vice versa*. What we formerly said of the transition of abdominal to laryngeal spasm applies to the three varieties now under consideration. I have had occasion to treat some cases of vesical spasm, which passed first into spasm of the stomach, then of the larynx. The nature of these affections being identical, and consisting of derangement of the abdominal spinal nerves, it is easy to account for such metastases.

The physician may, with attention, attain the practical knowledge necessary for distinguishing, in all cases, the spasm of the bladder from the other varieties. But in many instances it is impossible to determine whether the stomach or the intestine is the seat of spasm. This uncertainty, or it may be fault, of diagnosis, is fortunately of little consequence as regards the treatment.

In distinguishing spasm from *gastro-enteritis*, we must attend to the following points:—In *acute* inflammation of any portion of the alimentary canal, there is fever of a continued character, more or less distinct; there is either vomiting of everything which the child eats and drinks, diarrhœa, or obstinate constipation. Continual plaintive cries without violent screaming—constant tenderness on pressure applied to the abdomen—a much lower temperature of the extremities than of the trunk—are likewise symptoms of acute gastro-enteritis. It is a rare affection.

In *chronic* gastro-enteritis, the fever is not of the continued type, but there are almost every day perceptible alterations in the pulse, and degree of temperature of the extremities. Pain is not a prominent symptom of this affection. Mucous diarrhœa is a very constant and obstinate symptom. There is progressive emaciation. The child is almost constantly very peevish. We have already alluded to the complication of spasms with chronic forms of abdominal inflammation. In these cases there exists a continuous state of calm (but not of freedom from suffering), interrupted from time to time with fits of violent spasmodic pain.

Peritonitis, whether acute or chronic, is always accompanied with tension and extreme tenderness of the abdominal parietes. Diarrhœa is almost always present. The other symptoms are identical with those of the corresponding forms of gastro-enteritis.

Cystitis is a very rare affection during infancy, and can be easily distinguished from spasm, by the continuous character of the symptoms, some of which are common to gastro-enteritis. The urine is at first of a dark colour, more or less red or bloody, and at a later stage of the disease becomes loaded with mucous or purulent discharges. Large blisters applied to the abdomen of infants easily produce irritation of the bladder, passing sometimes into inflammation. Acute cystitis I have never observed before the fifth month, and the total number of cases which I have met with in children in the first two years of life does not exceed ten. Pains connected with *calculus* are recognised by the use of the sound.

The differential diagnosis of *rheumatism*, it is much more difficult to define. Rheumatism of the alimentary tube, or of the peritoneum, may commence with or pass into a train of true inflammatory symptoms; but it may be confined to the nervous system, and furnish no evidence, symptomatic or anatomical, of the phlogistic state. What is, then, the difference between a simple spasmodic neurosis, and a rheumatismal neuralgia? It would be necessary to enter into tedious and minute details in order to express my views on this subject; and these views are far from satisfactory in a scientific and practical point of view. There is, in fact, such a similarity between rheumatismal neuralgia—that induced by the action of cold air upon the warm and transpiring skin, and so much influenced by atmospheric and thermo-electric changes—and the primitive nervous spasms, that it must often be difficult or impossible to distinguish between the two forms of disease in an infant of tender age. Diarrhœa, almost always very serous and profuse, is one of the most constant characters of abdominal rheumatism. There is also a degree of vascular excitement, which the nature of the dejections shows not to be symptomatic of inflammation; still, infants, the subjects of abdominal rheumatism, have for the most part accesses of slight fever. The lower the degree of the rheumatic affection, the more difficult does it become to distinguish it from spasms, and the less serious is a mistaken diagnosis in regard to the treatment; for the application of dry heat is indicated in both diseases. The same may be said of the use of opium; and it is only the effect of blisters, which is more unequivocally beneficial in rheumatic than in spasmodic disease.

Prognosis.—The prognosis is favourable if the child is strong and of good constitution, and if the spasms have been preceded, and are still accompanied, by disorder of the digestive system. If the spasms have commenced with the first few days of life without appreciable cause,—if the mother is very nervous, or herself a sufferer from spasms,—if the child is of very delicate appearance, the prognosis is more doubtful. If there are very severe and obstinate gastric complications, these, and not the spasms, are the source of danger. Regard must also be had to the period of dentition, and a cautious opinion given when the teething is irregular, for in such circumstances I have often seen the spasms pass into general convulsions, and occasion death. In general, if the abdominal spasms be strongly marked, with intervals more or less regular and perfect, immediate cure is rather improbable; but it is very likely that the attacks, however obstinate, will cease between the ninth and twelfth month, if no unfavourable circumstance arise. As to the danger of death, there are comparatively few cases of this disease which, if uncomplicated with some other dangerous condition, terminate fatally by passing into general convulsions.

Prophylaxis.—The best prophylactic means against abdominal spasm, are those attentions to physical well-being best calculated to promote the regular development of the organism and vital forces, and especially a healthy digestion. We cannot enter into details on this subject; we shall only allude to the principal points which it includes.

If after birth the alvine evacuations are scanty, and if, after the first twenty-four hours, they retain the dark colour of meconium, half a teaspoonful of castor-oil, mixed with sweet almond oil, should be administered, and the dose eated every three hours, till the stools acquire the normal yellow colour. I

never prescribe for newly born infants syrup of rhubarb or of manna ; very sweet articles are not good for the stomach, and these syrups, being difficult of digestion, may engender spasms by passing into acid fermentation.

If the mother suffers from spasms or convulsions (even if she is of apparently robust constitution, and during pregnancy has not been so affected), she ought to intrust her child to a healthy nurse.

The examination of a nurse's milk, whether physical or microscopical, in spite of the interesting investigations commenced by Donné, and carried on by several other distinguished physicians, does not yet supply us with means of safely deciding either upon the quality of milk in general, or upon its adaptation as nourishment for this or that child. The proportion of granular bodies is no sure index to the wholesome nutritious property of the milk, nor are the other microscopic elements more trustworthy guides.

The physician is often asked, if the nurse should be changed, because she has every month a more or less prolonged menstrual period ? In such cases, it is our duty to ascertain if, during these periods, she complains of any other derangement,—if, while the discharge proceeds, her strength or the quantity of her milk are diminished,—if the appetite or digestion are affected,—if she suffers from pains in the lower part of the abdomen. If nothing of the sort is discovered, and if, besides, the child seems thriving, our advice is to continue the same nurse ; and I do not recollect having witnessed any unpleasant consequence from the adoption of such advice.

The mode of suckling ought to be regular,—at first every second or third hour,—and before the third month no nourishment should be given except the breast milk, which is sufficient in general. But should the quantity of the milk be insufficient, or if it be impossible to give the child the breast, I know of no better substitute than fresh cow's milk boiled and diluted with a very weak hardly coloured infusion of star-anise (*Illicium Anisatum*).

I must here allude to a very common fault of nurses. In general, if the infant cries, they give it the breast, and if the child be suffering from strong and frequent abdominal spasms, they thus overload its stomach with an extra quantity of milk, which it cannot digest, and hence induce gastric derangement and flatulence, which may from day to day increase its sufferings. This habit may become more mischievous, and even occasion an incalculable amount of evil, if the mothers think to appease the sufferings of their children by giving them more substantial nourishment. This is too frequently the case. The influence exercised by the practitioner over the hygienic condition of infants is still too small. I cannot pretend to a knowledge of the customs of Britain and France in this particular, but in Hungary, and in many parts of Germany and Italy, the above-mentioned habit of giving different articles composed of milk and farinaceous food to infants of two or three months old, is still very general.

But to return to the subject of those fits of crying, which most mothers and nurses try to appease by giving the child the breast, or some form of nourishment, it is most essential that mothers should study and become familiar with the temper of their children, and the true causes of their cries. It is very difficult to lay down descriptive rules for their guidance ; but maternal love and intelligence should be able to read the language of tears. Many women believe, that when a child cries violently, and brings its hands near its mouth, the desire to suck is indicated. This movement of the hands is very common and inconclusive; but if the infant, at the same time, cries loudly, kicks with its legs, and has its hands firmly closed, and in tremulous motion, it is suffering very acute pain. The cries caused by pain are also distinguished by the uneasy and interrupted manner in which the child takes the breast ; if, on the other hand, hunger or thirst is the cause of its cries, it sucks quietly, and after its wants are satisfied usually goes to sleep.

Among prophylactic measures, we must include the temperature of the chamber, and of the baths ; but on this head nothing can be said beyond what is well known.

Some mothers, too solicitous in their cares for their children, in order to insure them tranquil sleep, make the chamber quite dark, and avoid the production even of the sounds familiar in domestic life. This excessive care is to be blamed, for it exceedingly augments nervous susceptibility and weakness. Such infants, if disturbed by the slightest noise when at rest, experience a sort of nervous shock, and it is in subjects of this kind that the predisposition to spasm is most easily developed.

Treatment.—The treatment of abdominal spasms is composed in part of what has been already indicated, in treating of the external and internal causes of the disease, and in part of the prophylactic means just enumerated. The first point in treatment naturally consists in hygienic measures adapted to remove the causes which concur in the development of spasm; the second point is directed against the complications; thirdly, we must direct certain remedies against the spasm itself.

The removal of exciting causes has been already considered under the head of prophylaxis. In general, the greatest difficulty is, in obstinate cases, to estimate aright what is due to the purely nervous susceptibility, and what to the material element or different complications. The more sudden and strong the fits of cramp, the more decided the remissions, the more satisfactory the general health, the more evident is the nervous character of the complaint. We need not waste words to prove this. Cases, which have their origin purely in the system of animal life, are very rare. Such mathematical simplicity is not to be expected in a complicated living organism. The skill of the practitioner consists in duly appreciating the value of the material element in complicated cases.

I have already (at p. 360) mentioned several domestic remedies very useful in calming abdominal spasm, and to these we here again refer. To establish the indications for certain remedies, I propose to take the gastric and intestinal forms of spasm into consideration together, for the affinity between these two forms is very strong. Of the medication proper in the vesical spasm I shall treat separately.

In the first place, if there be gastric derangement (which, in connection with spasm, merits our attention, and which does not yield to dietetic means), before attacking the spasm we naturally prescribe an emetic, a purgative, or remedies to improve the digestion. But it is right to be chary of the use of these remedies in the case of an infant delicate in constitution and of tender age. Neither the white tongue nor the inodorous eructations of an infant at the breast are indications for the administration of an emetic. Eructations are, during infancy, very frequent, but when inodorous, and unaccompanied with symptoms of disordered digestion, are of no great significance. (Even in adults, purely nervous eructations are not unfrequent. Women affected with gastralgia often suffer from them. I have known several individuals who could immediately excite inodorous eructations by pressing with moderate force upon the epigastrium. A lady of very nervous temperament, and subject to arthritic complaints, whom I had occasion to treat, presented the same symptom when friction was applied to the fingers. These, then, are examples of eructations unconnected with gastric derangement.)

Most infants at the breast, even the most healthy, have a white tongue, and are subject to eructation in a slight degree. But in young infants, if there be derangement of the stomach or liver—circumstances which may demand the use of an emetic—there is *always a yellowish fur on the tongue, the eructations are of a disagreeable odour*, and the younger the child the more surely is there fever, indicated by quick pulse and hot skin. If there be biliary derangement, it is recognised by a slight yellowish tinge of the conjunctiva of the eyelids, or even of the globe of the eye.

Emetics.—During the first week of life, if there is indication for emesis, it is best provoked by tickling the palate with a feather after giving the child a drink of tepid water. It is better to continue these attempts, by which forcible

and repeated acts of vomiting can be excited, than to give an emetic to a child of a week or two old.

I have seen the administration of one-quarter grain of tartar emetic in two ounces of water cause the death of a child two weeks old, by inducing intractable hyperemesis. But children of several weeks or months old sometimes die when an emetic is administered which is too strong or ill-timed. In the hospital at Pesth I had several cases of death from profuse diarrhœa, provoked by emetic tartar, viz., in children who, in spite of the administration of the emetic, did not vomit, but had twenty or thirty stools in rapid succession; the purging could not be arrested, and caused in a few hours death by collapse.

At the age of two to four months, vomiting, if indicated, may be induced with safety by the use of medicine, the dose being modified to suit individual peculiarities. At this age the following formula will be found useful:—℞. Aq. Destillat.; Oxymel Scillarum, āā ℥ss.; Antimonii Tart., gr. ¼; s. et adde Pulv. Ipecac. Alcoholisat.,[1] gr. x. M. Capt. cochl. j. minimum quater in horâ.

If the child is weak, delicate, or of nervous habit, the emetic should not be given, and certainly never repeated. In opposite circumstances, the emetic is one of the most important of remedies, and may be repeated, if necessary.

Purgatives.—As to the purgative, the indication is easily obtained. In the first month I usually employ a lavement of infusion of chamomile, with a little oil and sugar, or give castor oil by the mouth. After the first month we may give an infusion of prepared senna leaves,—the formula known in Germany and Hungary as the "thé de St Germain,"[2]—and may prescribe it as follows:—℞. Inf. Sennæ, ℥j.; Tart. Potass et Sodæ, ℥j.; vel. Syrup Mannæ, ℥ij. M. Capt. cochl. j. pro dose.

But we may here repeat the caution already given against the use of syrups in these diseases. It is to be wished, for the sake of children, that the redundancy of these medicines in our pharmacology should be somewhat abridged.

Mercurials.—The use of mercury as a purgative in infantile diseases is still too fashionable, particularly in Germany and England, and demands some notice here. The therapeutical theory on which the virtues of this favourite remedy rests is so accommodating that there is hardly a disease of childhood in which it is not "distinctly indicated." Thus it is used in abdominal spasms, not only as a "purgative," but as the "most powerful of alteratives." In some way or other, it is lavishly given in almost all obstinate cases. The cause of this abuse resides, I think, in two circumstances: in the obscurity of its action, and in the difficulty of salivating an infant even by its long-continued use. For my own share, except in syphilis, I can conceive no indications for calomel, except to act slowly on the liver, in moderate dose, or to procure abundant bilious motions, when given in larger quantities. If such indications are not present, its use as a purgative is to be condemned; and still more reprehensible is the plan of giving it in small doses, for weeks together, as an alterative; for in my experience of the consecutive effects of such treatment, I have become satisfied that no remedy is followed by such pernicious influence upon the digestion, the blood, and the bones.

Tonics, &c.—The indication for regulating and strengthening the digestive functions is a very natural one, when, besides the spasms, we find disorder of the stomach; but, in infants of very tender age, the indication is with difficulty fulfilled by the use of remedies. I shall here allude only to three very commonly employed,—viz., powder or tincture of rhubarb, carbonate of magnesia or calcined magnesia, and powdered fennel seeds.

Rhubarb in powder is not a good remedy during the first few weeks after birth. At this age powders are, for the most part, difficult of solution, and even

[1] We find no account of this powder in the Austrian Pharmacopœia.—Ed.

[2] In this preparation the more acrid matter is removed, by previously steeping the leaves in alcohol.—Ed.

when given in very small doses, are not well tolerated by the stomach. Infants cannot express their dislikes, but if attention be paid to the child after a dose has been administered, what I have just stated will often be obvious. Even at the age of two or three months, infants, by vomiting, and an unmistakeable expression of disgust, show the nausea excited by the ingestion of powders. If in any case it is thought expedient to prescribe rhubarb for a very young infant, as a tonic for the stomach or bowels, the dose should be from one-sixth to at most one-half of a grain twice or thrice a-day. The tincture is a far more suitable form. Two to four drops may be given twice or thrice daily, in a warm infusion of melissa or of chamomile. Three grains of the powder, or fifteen drops of the tincture, if given in a single dose, will almost always cause pains in the bowels, even if the infant is not usually subject to them. But it is important to state, that, although I have made thousands of experiments with rhubarb, I have never seen it produce immediate and unequivocal effects upon the digestion in infant life.

I have no more favourable opinion of *magnesia*. It is usually prescribed when there are symptoms of acidity at stomach; and as this complication often co-exists with spasms, practitioners give magnesia very frequently in such cases. Yet my observation has not shown the remedy to be so efficacious as is commonly believed. It is the proper changes in the mode of nurture which concur in effecting the cure, and often suffice without the remedy. I am convinced that the doses commonly given are too large. The stomach of an infant of two or three weeks may show more or less distinct symptoms of indigestion after doses of three or four grains of calcined magnesia. My experience teaches, that the medium dose for an infant aged two to four weeks is half a grain, repeated three or four times a-day; from the age of two to four months, the dose may be two to four grains.

Powder of *fennel* seeds is also a favourite remedy in abdominal affections. It is given to cause the expulsion of eructations and flatulence. I do not advise its use till the third or fourth week, and even then the dose should be small. The indication will be better fulfilled by giving to the little sufferer a spoonful of a warm infusion of chamomile and fennel flowers mixed, or one or two drops of sp. melissæ for a dose. At the age of two months and upwards, the dose of fennel may be from a half to one grain.

All that we have said of these three remedies must not be understood to apply to infants of four months and upwards; for at this age the stomach is able to bear larger doses, and hence the better effects observed. But if the abdominal spasms are very obstinate, and the infant of delicate frame, we must not, even though its age should exceed four months, insist too long upon an exclusively *antigastric* treatment; and least of all must we use evacuants.

Narcotics.—If the spasms are such as have been already described at p. 360, it is seldom that we can neglect to use some nervous calmative. It is unnecessary to enumerate a list of well known remedies. The cherry-laurel water is one of the most generally employed; but for my own share, although I have for several years prescribed it, I have never witnessed its prompt and decided effects either in cases of abdominal spasms or of any other pain. For the progress of medicine, nothing is more essential than to discard all remedies of feeble or uncertain virtue.

Opium is the only direct nervous calmative, which is almost always sure in its effects. There are, however, writers, particularly in Germany, who would totally proscribe its use in infantile diseases, because it is dangerous. I think, however, that far more mischief is done every day by the use of inefficient remedies, such as syrups, powders, and indigestible decoctions, than by the employment of drugs which, in small dose, have a powerful action. This is not the place for details on this subject. I must, however, mention emulsions of almond oil and of gum-arabic, with which many practitioners think to calm abdominal spasms, and which they administer to children even a few weeks

old, in doses of two to three ounces a-day. I have myself abandoned all such formulæ, satisfied that the quality and quantity of these remedies are both capable of disturbing the digestion of the breast-milk.

We must not be intimidated by the possible danger of opium. Its action is sure and salutary, and it is not difficult to learn the rules for its administration. I have given it to thousands of infants without ever having had occasion to regret my practice; and without opium I would not undertake the treatment of children's diseases. I shall, then, give a brief resumé of my experience of the drug in the Children's Hospital.

The proportionate dose of opium in very early life is nearly as follows:—To the newly born babe till it is a week old, I have prescribed it very rarely; from the second to the third week, the medium dose is $\frac{1}{120}$th of a grain; from three to six weeks, $\frac{1}{100}$th; from six to eight weeks, $\frac{1}{70}$th; from two to four months, $\frac{1}{40}$th of a grain of the powder. Of the simple tincture of opium fifteen drops are equivalent to one grain of the powder; and Dover's powder in ten grains contains one of opium and one of ipecacuanha.

The action of a proper dose of opium is manifested half an hour after its exhibition, and lasts from three to six hours. If narcotism is induced, it will be serious in proportion to the tender age of the patient. I have seen a single dose of $\frac{1}{4}$ gr. of Dover's powder (equal to $\frac{1}{40}$th gr. of opium), given to an infant of five weeks, cause narcotism, which lasted for thirty-six hours. The sleep during this period was continuous, profound, interrupted sometimes by startings, as if from fear. The case, however, terminated favourably. I have seen two cases in which the narcotism caused death. A moderate degree of narcotic action (lasting from six to ten hours) has never, in my experience, seemed dangerous, *when the use of opium has been distinctly indicated.*

The most suitable remedies for the narcotism consist in the application of cold water to the forehead, and of sinapisms to the nucha, a gentle purgative, and, in very obstinate cases, one or two small leeches to the temples, and ten to twenty drops of infusion of coffee made in the ordinary way, and given every hour. It is important not to give the above-mentioned doses of opium to infants at shorter intervals than from four to six hours, in order that their effects may be duly watched. While opium is being used, care must be taken lest an obstructed state of the bowels should be induced.

The most decided counter-indications are as follows:—If the pain have been long continuous, and accompanied with fever; if there be signs of inflammation, much heat of the head, with somnolence, and alvine obstruction.

In no form of spasm is the opium so efficacious as in the *intestinal.* It is not a radical cure; but calms pain, by resolving the spasmodic constriction of the intestinal tube which prevent the escape of its irritating gaseous contents, and thus assists the radical cure. On comparison, I have found that the intestinal pains have been more frequently relieved by opium than the gastric. These are often dissipated by a drop of sp. melissæ.

In cases of abdominal spasm, after the first month of life, I often, with advantage, prescribe the following powders:—℞. Pulv. Opii. gr. $\frac{1}{5}$; Magnes. Calcinat. gr. viij.; Pulv. Sem. Fœniculi, gr. iv.; Sacchari Albi gr. xxiv. M. Div. in chart. xvi., quarum j., 4tâ q. q. h. sumend. As to the salts of morphia, there is difficulty in portioning out the exact dose,—*e. g.*, $\frac{1}{400}$th of a grain for a child of a month old.[1]

To the remedies already mentioned, I may add the Emplast. Nucis. Moschat. applied to the abdomen, either alone or mixed with "Thériaque de Venise."[2] In general, however, the skin of a child under six or eight weeks old will not long endure such a remedy.

Aromatised warm baths sometimes alleviate the pains.

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[1] This objection cannot apply to the Sol. Mur. Morph.—Ed.

[2] Probably the complicated formula, containing a minute proportion of opium, to be found in the "Codex Medicamentarius Europæus."—Ed.

I have seen no good effects from either sinapisms or blisters. in cases of abdominal spasm ; it is in rheumatic affections that these act best as revulsives ; besides, the sensibility of the skin before the age of two months is too great to permit their application.

Cold lotions, mixed with a little alcohol (*e. g.*, 6 to 8 parts of spirit to 100 of water), are often to be recommended, when there is proclivity to nervous and spasmodic action. Their temperature should be gradually diminished, and they may be applied once or twice a day, for five minutes at a time, over the whole body.

Spasms of the urinary bladder are either purely nervous, in which case the urine is clear and abundant ; or connected with some gastric disorder, or with preternatural acidity of the urine, which, as has been already stated at p. 364, is then reddish-yellow. The two last varieties are the most frequent. Besides the anti-gastric remedies already enumerated, opium must be always prescribed in vesical spasm, which is of all forms of spasm the most violent. The *powder of lycopodium* is evidently useful in these cases, and I have, with advantage, ordered it in doses of two to four grains, twice or thrice a day. A proportion of opium may be added, and, in case of superabundant acidity, calcined magnesia ; but, for my own share, I prefer, for fulfilling this last indication, a solution of three to six grains of bicarbonate of soda in two ounces of distilled water, to be taken in the course of twenty-four hours.

PECULIAR AFFECTION OF THE LYMPHATIC VESSELS—ANALYSIS OF LYMPH. BY DR FETZER.

The rarity of cases in which it has been possible to collect lymph from the living body gives interest to the following case, more especially as the affection which caused the "*lymphorrhœa*" was spontaneous, whilst in all the previously known cases it has been traumatic.

A girl, aged 16, who had not menstruated, and was affected with double hernia, remarked for the first time, during the summer of 1847, that her belly was covered with little tubercular lumps. She took no notice of them at the time, and it was only in January 1848 that she showed them to Dr Fetzer. A brownish band, of the breadth of three fingers, extended from the umbilicus to the left side, and passed backwards to the spine, becoming narrower and paler. On this brown band there were seen, about an inch below the umbilicus, and two lines from the linea alba, about eighteen mamellated eminences, of the size of the human nipple, indolent, flaccid, compressible, but recovering their original size on the removal of the pressure. Occasionally, during the month of July, the patient experienced some lancinating pains along the brown band. On the 31st July, after a walk, there flowed from that region, according to the statement of the patient and her mother, about eight ounces of a milky liquid. This discharge reappeared in three days, and continued uninterruptedly till the 6th of August.

There was seen issuing from the two largest eminences a milky alkaline fluid in considerable quantity. This, when collected, separated at first in thirty minutes, afterward in fifteen, ten, and even four minutes into a turbid milky serum, which became clear on the addition of ether, and a transparent rather bulky coagulum, which reddened visibly in the air.

The author having dilated one of the apertures with the scissors, was able to introduce a canula into the vessel ; the liquid then flowed in a stream. The orifice was afterwards cauterised with nitrate of silver to stop this flux, which greatly weakened the patient.

From that time there has been no return of the discharge, but the number of nodosities has increased so much that the whole of the brown band above mentioned is strewed with them.

Microscopic examination of the fluid showed a great number of fatty vesicles, normal lymphatic globules, and other globules which were smooth, discoid, and yellow, and appeared to be blood corpuscles in course of formation.

The fluid was analysed chemically by Professor Schlossberger, who found in a thousand parts—

Water	930·63
Albuminous matter	47·26
Fat	0·29
Extractive matters	8·30
Salts	9·54

The quantity of fibrine was not accurately ascertained, because the filter containing the coagulum was lost after the first drying. Dr Schlossberger estimated it at 4 parts in 1000.—*Archiv. für Physiologische Heilkunde*, in *Gazette des Hôpitaux*, 20th August 1850.

SURGERY.

ON THE FORM, CONSTRUCTION, AND USE OF THE CATARACT NEEDLE. BY DR JACOB.

The following observations are abridged from an article in the "Dublin Medical Press," in which Dr Jacob has recently republished a paper on the above subject, which originally appeared in the "Dublin Hospital Reports," and to which he has made additions suggested by further experience. * * * * If the surgeon determine to adopt this operation of opening the texture of the lens through the cornea, he has next to make choice of the instrument which will accomplish this object most effectually, and with least injury to the organ. I conceive that all that is required for this purpose is a fine point; a cutting edge or knife being only required where the lens is to be cut in pieces. I also consider that the needle should be curved at the point, to enable the surgeon to open completely the texture of the lens if it should prove soft or friable, and it should be so constructed that the aqueous humour shall not escape. * * * To attain the desirable objects stated above, and to avoid the difficulties to which I have just alluded, I determined to try a fine sewing needle curved at the point; and after about forty operations I do not feel in the least inclined to repent of my choice. I am, on the contrary, every day more and more satisfied that it affords peculiar and unquestionable advantages. It rarely, if ever, leaves even the slightest mark in the cornea. I could produce examples where it has been three times introduced, and where not the slightest speck can be detected; and I have introduced it through the very centre of the cornea without any bad consequence. When fairly introduced into the eye, it is capable of accomplishing any object to be attained by a needle. The capsule can be opened to any extent; a soft or friable lens can be actually broken up into a pulp, by pushing the curved extremity of the needle into its centre, and revolving the handle between the fingers; large fragments can be taken up on the point of the needle from the anterior chamber, and forced back out of the way of the iris, or if sufficiently soft, may be divided by pressing them against the back of the cornea with the convexity of the needle,—a method which I have repeatedly adopted with advantage. When the lens has been displaced from the capsule, in consequence of the needle sticking in it in attempting to open its texture, I have, without removing the needle, placed the lens in the anterior chamber, and then extracted it; and in other cases have forced it back into the vitreous humour, out of the reach of the iris. From the fineness of its point, and the ease with which it can be turned and twisted in every direction, it enables the surgeon to deal most effectually with an opaque capsule; he may pick it with the point from any attachment it may have formed to the iris, or if it hangs flaccid he may entangle and detach it by pulling or twisting. In certain cases the pupil is found nearly closed, and adhering to a small cataract of nearly cartilaginous hardness; in these I have introduced the needle, and with the point picked up the adhesions between the margin of the pupil and this hard mass, which I have then placed in the anterior chamber, and removed through an opening

in the cornea, with a pair of forceps. It might be said that all this might be accomplished by a diminutive needle on the plan of Scarpa; but not, I conclude, with the same prospect of success, on account of the much greater size of even the smallest of such needles.

There is one difficulty attending the use of the round needle: it requires very considerable force to pass it through the cornea; so much, indeed, as frequently to embarrass those who use it for the first time. I can, however, safely assert, that very little practice enables the surgeon to surmount this difficulty. It is only necessary that he should be aware of the degree of force required; that force he is perfectly safe in employing. The surgeon who rejects an instrument which affords peculiar advantages, or refuses to adopt superior methods of operating, because difficulties in execution stand in his way, can never expect to obtain the character of a good operator. The greatest advantage in the use of the needle results from the very circumstance which causes the difficulty in its introduction: it is from its conical form firmly wedged in the cornea, which prevents the aqueous humour from escaping, and in consequence of being thus fixed, gives the surgeon a power of holding the eye that defies every effort on the part of an unruly patient, unless he actually pluck out the instrument with his hand. If the head be suddenly drawn back the surgeon has only to let the instrument rest loosely on his hand, and follow the motions of the patient. I have seen the needle, under such circumstances, slip from the hand of the surgeon, and hang from the eye without serious mischief, the handle being very light.

The size of the needle is known in the shops as number *seven*, being the forty-fourth part of an inch in diameter, about one half the size of the finest Saunders's needle which is made. The point can be turned to the requisite curve by means of a pair of cutting forceps, or the ward of a small key; of course without heat, which would destroy the temper. It must not, however, be expected that all needles are so soft as to be bent thus cold; there may not be ten in an hundred of this temper, but when once turned they retain the curve without any danger of bending or breaking, and certainly possess a degree of strength and temper never observed in needles separately forged and finished by the best cutlers. They should always be tried before use by passing them repeatedly through thick calf-skin leather. After they have received the requisite curve, the point should be cut flat on each side, on a fine hone, and carefully examined with a magnifying glass to ascertain that it is perfect. The extent to which the point should be curved may be left to the choice of the surgeon, reminding him that the greater the curve the more effectual the needle will be when introduced; but the difficulty of introducing it through the cornea will also be greater. I therefore recommend those who use it for the first time to choose one slightly curved. After the point has been turned, the needle, held in the jaws of a pair of pliers or a vice, is to be run down into a cedar handle, without cement, leaving only *half an inch* of blade, which I have found to answer every purpose. If the blade be left longer it will yield and spring when opposed to a resistance. The handle should be about the fifth of an inch in diameter, and four inches long. I use the handles made for camel hair pencils, and find that a metallic ferule, which increases the weight, is unnecessary and objectionable. A needle thus constructed, and preserved free from rust, will retain its point for a great length of time: I have used the same one a dozen times without sharpening.

The surgeon, provided with a needle, places himself in the usual position with respect to the patient, availing himself of whatever assistance he may find necessary to secure the lids.[1] He then brings the point of the needle within

[1] Some ophthalmic surgeons recommend that the left hand be employed to operate on the right eye, supposing the operator to sit in front of the patient. I operate on the left eye sitting opposite to the patient, on the right standing behind him with the head resting against my chest; this latter position I find by far the most favourable and convenient.

a very short distance of the eye, and when the cornea is brought into an advantageous position, he suddenly strikes the needle into it near its circumference. As I do not apprehend any opacity from the wound, I am not very particular with respect to the precise point where the needle pierces ; I generally, however, enter it sufficiently near the margin to obviate defect from this cause. The point of the needle once fastened in the cornea, the surgeon has complete command of the eye ; no action of the muscles can disengage it, and there is no danger of the needle slipping into the anterior chamber ; an elevator or ophthalmostat is therefore altogether useless. The operator now pushes the needle through the cornea, which frequently yields like wet leather, and the eye often turns so much toward the inner canthus that the pupil is hid, and he must rely upon his knowledge of the course which the needle necessarily takes, in order to conduct it to the lens. This is the principal difficulty to be surmounted. If the surgeon does not now steadily push the needle forward, whatever resistance he may feel, he will find, when the eye returns to its proper position, that the point of the needle is still merely entangled in the cornea. This also is the period of danger to the iris ; if the operator does not keep the flat of the needle to that membrane, with the point down and the convexity up, he will be very liable to injure it. Should it happen that the point of the needle has passed through the iris, it may be easily extricated by gently drawing back the instrument without removing it from the eye. After the needle has been fairly entered, and that the operator sees its point at the opposite side of the pupil, he brings the cornea forward merely by pulling it upon the needle, to which it is completely secured in consequence of the blade being wedged into its texture. He now turns the point directly back, and gently tears open the capsule, picking and scratching the surface of the lens with a rotatory or drilling motion of the instrument ; not with the lever or cutting movement, which is necessary when Saunders's needle is used. If the lens be soft and friable, the fragments fall like snow into the anterior chamber, and the surgeon may deal very freely with it, pushing the needle deep into its structure, and twirling the point round so as to mash it into a pulp. If, however, it proves hard, and that he attempts thus to deal with it, he fixes his needle in its tough and glutinous structure, turns it out of the capsule, drags it against the iris, and makes it necessary either to extract it or force it back into the vitreous humour. As I have already observed, if the cataract be hard, the capsule should be opened, and the centre of the lens cautiously scratched with the point of the needle, so as to expose its texture to the contact of the aqueous humour, by which it is softened and fitted for breaking up on a future occasion. In withdrawing the needle the surgeon has to encounter the same description of difficulty which attends its introduction ; it is tightly held by the cornea, requiring to be turned on its axis in order to extract it, as an awl is drawn from leather. It must not, however, be forgotten that this wedging of the instrument is attended with the great advantage of enabling the surgeon to operate on the most unsteady without an ophthalmostat or elevator. * * *

Since this paper was published, I have continued to practise this operation as described in it, and with the needle proposed, without interruption; scarcely deviating from the directions given, or resorting to any modification of the method laid down. Of the superior qualities of the needle I have not the slightest reason to change my opinion. It is, I am satisfied, by many degrees the best for the purpose. Its small size, great strength, and matchless temper, entitle it to a preference before all others as regards these qualities ; while its cylindrical stem and the curvature at the point, fit it for application to any peculiarity. But I find all these advantages in the instrument overlooked, if not despised, by some, because it has not the imposing appearance of a finely polished blade in an ivory or ebony handle with silver ferule ; so much so, that the cutlers will not make it, or keep it for sale, and some of them have actually forged a tool to resemble it, in proportions more worthy of the trade, when

called on for it by a customer. Now the truth is, that no cutler could forge, temper, and finish such a blade with the slightest probability of equalling this needle, which acquires its temper by chance in the tempering of thousands of needles together. I have stated in this paper that not ten, I might perhaps have said five or six only, in a hundred of needles, are found with the temper which admits of their being bent without breaking, and enables them to preserve the *set* or curve given to them by the pliers. As to the strength of the blade, diminutive as it is, no fears need be entertained. I never yet broke one or saw one break in an operation; and, as I have said, it may be passed without danger through the stoutest leather. I should, however, remind the surgeon, that the blade, before being finally pushed into the handle, should be dipped into a solution of gum lac or sealing wax, in spirit of wine, as otherwise it may rust at the wood if laid aside wet, and may snap there in consequence. I have also to warn those fond of "improving" surgical instruments to suit their peculiar notions, that I do not answer for the performance of any needle of this description unless it has been constructed precisely as directed. It must be the needle No. 7 (being the 44th of an inch in diameter), five-eighths of an inch long in the blade, from the wood to the point, and in a cedar handle of the specified proportions. If the blade be left longer it will spring in using, and if shorter, it will not reach as far as necessary.

STRICTURE OF THE URETHRA.

During the discussion which has of late taken place relative to the treatment of stricture, we have endeavoured to maintain, that the best means for this purpose was, in general, the simple metallic bougie; and that in those comparatively rare cases in which the contraction proves to be of such an irritable contractile nature as to resist dilatation, the most expedient course was to divide it by incision, upon a grooved director, passed through the narrow part of the canal. This operation we have represented as perfectly safe, certain to afford complete relief, and, there was reason to believe, generally, if not always, permanent in its good effects. Of the less eligible means employed for the remedy of this disease we have regarded as the most objectionable, cutting into the perineum in search of a passage without the assistance of an instrument conveyed through it; and attempting to destroy the obstruction by using escharotics, such as caustic potass. It would seem that the opinions upon this subject, entertained in the southern metropolis, are very different; since, in a late number of the "Mirror of London Surgery,"[1] there is related a case of stricture in which both the guideless probing and destructive caustication were employed, together with a third procedure, which, for rudeness and danger, must be allowed to stand unrivalled. This was thrusting a trocar—for it was neither more nor less, though disguised under the title of "Urethrotome"—through the seat of contraction. It is said, that before the fistula in the perineum was quite healed, "Mr Fergusson thought it advisable to allow the patient to recruit his health, by leaving the hospital (*where he had been four months*), and seeking the country air." This course, we think, was very prudent, for if the poor man had delayed his departure until after the fistulous opening was healed, his prospect of enjoying country air would, in our opinion, have been rather problematic.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

CASE OF FATAL HEMORRHAGIC DIATHESIS FOLLOWING AMENORRHEA.

BY J. B. DUNLAP, M.D., NORRISTOWN.

A young lady, 22 years of age, who had suffered for twelve months from more or less acute continuous pain in the right hypochondrium, with consider-

[1] "Lancet," September 7, 1850. P. 304.

able irregularity in her menstrual discharge, became completely jaundiced, and the catamenia entirely suppressed. A few weeks thereafter, she was attacked with severe pain across the loins, nausea, and a profuse discharge of blood from the mouth, which, in spite of the remedies employed, continued for five days, and then suddenly ceased. She remained in tolerable health in the interval; but the hemorrhage again returned at the end of four weeks, and ran exactly the same course. This continued for five or six periods. The quantity of blood lost each time was about six quarts.

Dr Dunlap, who was only called to attend the young lady during the two last attacks, says, "Upon opening the patient's mouth, I saw distinctly the blood oozing from the gums, in front of and behind the teeth, and as far back as the first molars, beyond which I am certain that no blood proceeded. On wiping it off the gums, the blood in an instant would again swelter out, covering them completely, and by making pressure on them, blood could be forced out in increased quantities." Dr Dunlap was endeavouring to restore the catamenia, and correct, if possible, the derangement of the liver, when, unfortunately, he says, "during my absence, on account of the severe pain in her side, and by the advice of a meddlesome friend, she sent for a cupper, who applied some half dozen cups to the right hypochondrium,—the consequence of which was, that the bleeding from the gums entirely ceased; but it was found impossible, by any means, to arrest the hemorrhage from the scarifications. She soon became pulseless, and in less than six hours died."

On examination after death, the various organs were found in their normal condition. In the gall bladder were found seven calculi, each about the size of a hazel nut, of an octangular form.—*New York Journal of Medicine*, May, 1850.

ON THE LEAVES OF THE RICINUS COMMUNIS AS A GALACTAGOGUE.
BY DR J. O. M'WILLIAM.

In the Cape de Verd islands the leaves of the bofareira, or common castor-oil plant, are used by the natives to excite lactation; and by their use the secretion of milk is not only, according to Dr M'William's testimony, accelerated and increased in child-bearing women, when that secretion is tardy in appearance or deficient in quality, but he also learnt that "this remedy could be successfully applied to produce milk in the breasts of women who are not child-bearing, or who have even not given birth to, or suckled, a child for many years." The following is the method of employing the remedy:—"When the appearance of the milk is delayed, in cases of child-birth (a circumstance of not unfrequent occurrence in these islands), a decoction is made by boiling well a handful of the *white* bofareira in six or eight pints of spring water. The breasts are bathed with the decoction for fifteen or twenty minutes. Part of the boiled leaves are then thinly spread over the breasts, and allowed to remain until all moisture has been removed from them by evaporation, and probably, in some measure, by absorption. This operation of fomenting with the decoction, and applying the leaves, is repeated at short intervals, until the milk flows upon suction by the child, which it usually does in the course of a few hours.

"On occasions where milk is required to be produced in the breasts of women who have not given birth to, or suckled, a child for years, the mode of treatment adopted is as follows:—Two or three handfuls of the leaves of the ricinus are taken and treated as before. The decoction is poured, while yet boiling, into a large vessel, over which the woman sits, so as to receive the vapour over her thighs and generative organs, clothes being carefully tucked around her, so as to prevent the escape of the steam. In this position she remains for ten or twelve minutes, or until the decoction cooling a little, she is enabled to bathe the parts with it, which she does fifteen or twenty minutes more. The breasts are then similarly bathed, and gently rubbed with the hands, and the leaves are afterwards applied to them in the manner already described. These operations are repeated three times during the first day. On the second day, the woman

has her breasts bathed, the leaves applied, and the rubbing repeated three or four times. On the third day, the sitting over the steam, the rubbing, and the application of the leaves to, with the fomentation of, the breasts, are again had recourse to. A child is now put to the nipple, and, in the majority of instances, it finds an abundant supply of milk."

In some cases the decoction of the bofareira is taken internally, with the view of assisting the action of its external application.—*Lancet*, September 1850.

JAPANESE REMEDY FOR AMENORRHŒA AND STERILITY. BY DR E. WILLIAMS.

A tincture of the leaves of a perennial tree of the natural order Terustrumaceæ (the *key-tu-sing*), is employed in Japan and China in cases of checked menstruation and sterility—and, Dr Williams thinks, with such success that the plant may become a valuable addition to our materia medica. A teaspoonful of a tincture made of the leaves with rice-spirit, given every hour, will, says Dr Williams, "invariably [?] bring on the menstrual secretion, which can be maintained by a dose or two daily for any length of time." The root, as well as the leaves, are used when the plant is employed to obviate sterility. [We lately procured some specimens of the Chinese emmenagogue plants and abortives through the kindness of Dr Balfour of Hong Kong; but they were accompanied by such information as to lead us to be extremely sceptical in regard to the alleged certainty of the common Chinese aphrodisiacs upon the cure of amenorrhœa and the production of artificial abortion.]—*Lancet*, 14th September.

ARTIFICIAL FRACTURE OF A COCCYX PROJECTING FORWARDS AND IMPEDING LABOUR. BY DR SUMMER.

The projection, in this case, of the coccyx inwards, had been the cause of death to the child in three successive labours. In the last the cranium was found to present a large gash, as if cut by a sharp instrument. On examination, the os coccygis was found to project in the form of a hook-like process. The patient was advised to avoid the risk of pregnancy in future; but failing in her resolution, Dr Summer was summoned in the course of time to her fourth confinement. Recollecting what had occurred on the previous occasion, he endeavoured to straighten the projecting bone, but failing in this, he broke it by main force. A living child was born, and the patient recovered. The same proceeding was rendered necessary in two subsequent labours.—*American Journal of Medicine*, and *Provincial Journal*, for September 4.

TOXICOLOGY.

POISONING OF THREE CHILDREN WITH CAMPHOR. BY DR SCHAAFF, OF STRASBOURG.

On Monday, 8th April 1850, an innkeeper's wife at Neudorf, near Strasbourg, gave to her two boys, one aged five, the other three years, and to her infant daughter, aged eighteen months, a dose of powdered camphor, equivalent to about half a teaspoonful for each, the whole representing about thirty grains. For several previous days the same drug had been administered, but always in smaller doses, and never on empty stomachs. It was intended as a vermifuge for the boys, and to remedy some intestinal derangement in the girl.

The first unusual phenomenon which the parents observed after the swallowing of the drug was an excessive paleness of face, with a fixed and stupid look. Presently there occurred a little delirium, and some heat in the throat and thirst were complained of,—next there followed nausea, vertigo, and slight twitchings of the face. A little later still there supervened vomitings and true convulsions, with loss of consciousness more or less prolonged, and also frequent desire to make water. This was reported to the doctor on his arrival,

which was half an hour after the camphor had been swallowed. These effects were manifested by all the children, and, with some slight variations, in nearly equal degree.

When first introduced to the scene, Dr Schaaff was forcibly struck by the appearance of the three little unfortunates, simultaneously affected with violent convulsions, with disordered expression of countenance, livid aspect, and tormented with constant retchings.

The oldest boy, more than the others, was affected with clonic convulsive movements, leaving between them intervals of a few seconds, sometimes of a minute. The arms were more convulsed than the lower limbs. Several times the body curled itself up into a ball, and was projected out again with great activity. The author had never seen such an appearance before. The face, alternately pale and livid, was the seat of incessant spasmodic movements. The eyelids in constant agitation, and half closed, showed the eyeballs turned upwards and outwards. There was a little froth about the teeth, which were firmly clenched. The skin appeared in general to be pale and moist, and the heat of the surface diminished. Sometimes there was complete loss of consciousness, at others the boy recovered his senses, and obeyed the orders given him. The respiration was short and noisy. The pulse uncountable, chiefly from the agitation of the body. There was frequent retching followed by vomiting from the remedies used, and repeated discharges from the bowels and bladder. The urine appeared to Dr S. to possess the odour of camphor, although this was denied by some of the bystanders.

The whole of these phenomena lasted, with more or less intensity, for three consecutive hours, then there came on a comatose sleep, which lasted till evening. The night was good, and beyond a little discomfort, the next morning showed no traces of the violence of the previous day.

As regards treatment, the author not knowing any positive antidote for camphor, commenced by taking every means for expelling the poison. He gave an emetic, laxatives, enemata, emollient, oleaginous, and narcotic,—and afterwards an opiate draught. In regard to this last, he inferred that as camphor has sometimes been employed to remedy some of the symptoms caused by opium, the latter might in its turn be able to subdue the violent action of the camphor. At all events, the formidable symptoms disappeared under its use with comparative rapidity. It is to be remarked that the first vomiting did not take place till an hour after the swallowing of the camphor, so that there was abundant time for its absorption.

The second boy presented the same symptoms as his brother, but in a less formidable degree. He vomited spontaneously and at an earlier period, to which circumstance, although feebler and younger than the other, he owed a more speedy restoration to health. In fact, an hour after the commencement of the symptoms, a profound sleep, accompanied by a profuse perspiration, soon brought everything to rights.

The following are the facts connected with the little girl, whose life was sacrificed to the deplorable mistake of her parents :—

She was delicate, and ill-developed for her age, had had a difficult dentition, cnd latterly suffered from an affection of the respiration of rather severe character, which had itself caused some convulsive attacks. But more recently her health had been restored, and the slight flatulency and want of appetite, which had been the pretext for giving the camphor, were quite unimportant. This child had been the first to show symptoms of poisoning. The convulsions in her were less violent than in her elder brother; but were prolonged without interruption from 7 A.M. till 2 P.M. From this period they presented intermissions of greater or less duration ; but after these the recurrences were more violent. The peculiarity in her case was, that in the face they affected only the right side ; but there they manifested themselves, even when the rest of the body was unconvulsed, and continued to cause hideous grimaces till the death of the child, which took place at 7 P.M. She never recovered conscious-

ness from the beginning to the end of her illness. There were also observed involuntary evacuations and vomitings, which, however, did not come on till a late period, notwithstanding the means used to excite them ; and there was also frequent discharge of urine. Further, there was a pallid countenance, occasionally blue and covered with cold sweat; the mouth slabbered, the eyes fixed and turned upwards, the features distorted by the spasms, the head thrown backwards, the respiration irregular and hurried. Towards the middle of the day there were some symptoms of improvement ; but these were deceptive, violent convulsions recurred, and the child died in one of the paroxysms.

The treatment which had been applied to the older boy was followed perseveringly in this case. With a view to producing powerful revulsion towards the skin, Dr S. used general friction with very warm mustard and water. The application of leeches, which he at one time thought of on account of the convulsions, was abandoned, because there were no symptoms of congestion about the head, whilst the constant agitation of the body prevented their being applied.

Dissection twenty-four hours after death.—The body generally was extremely pale—the abdomen much distended by gases—the mouth presented no traces of redness or ulceration—the stomach and all the intestinal convolutions were extremely distended. They exhaled a peculiar nauseous odour. The interior of the stomach showed no traces of redness,—on the contrary, both the gastric and intestinal mucous membranes were colourless. There were found in these organs, some detached whitish pellicles like fragments of croupy false membranes. One loop of the small intestine, to the extent of about four inches, presented the remarkable appearance of emphysema existing at once below both the peritoneal and mucous surfaces.

The large blood-vessels of the abdomen appeared unusually empty, and the liver and spleen, from their paleness, seemed to participate in this deficiency of blood. The parents did not permit the other cavities to be examined.—*Gazette Méd. de Strasbourg in Revue Medico-Chirurgicale,* July 1850.

[This is, we believe, the only recorded case of fatal poisoning by camphor in the human subject. Its effects are well known to be very variable ; but in experiments on animals, the tetanic spasms observed in the above cases have generally been a prominent symptom. From some introductory remarks of Dr Schaaff, it appears that camphor is a frequent remedy in domestic practice in France. It is not so in this country ; but it is obvious that though large doses have been taken without fatal effects, its use in such quantities is dangerous, especially in young subjects.]

POISONING WITH BITTER ALMOND. [COMMUNICATED BY DR CHRISTISON.]

Were not the obtuseness of the organs of taste in many individuals a matter of common observation, it might appear extraordinary that any one should eat a sufficiency of bitter almonds in the natural state to produce dangerous poisoning. There is no doubt, however, that several fatal cases of poisoning with this substance have occurred, besides others in which life was saved only by prompt measures of relief. Of the latter kind is the following, which has been lately communicated to me by Mr W. T. Jones, of Cork :—A lad of fifteen, son of a wholesale grocer, ate a quantity of bitter almonds with sugar. After a time,—but how long, he could not tell,—he felt a rather pleasing sensation, became suddenly giddy, fell down, and lost consciousness and recollection. It could not be ascertained how long he lay in this state. When Mr Jones was called to see him, he found him lying near a cask of bitter almonds. The extremities were extended and somewhat stiff, but not altogether rigid ; the pupils were dilated and insensible to light ; the respiration was not perceptible ; the heart's action was barely so, and the pulse was feeble and intermitting. A solution, containing half an ounce of hydrochlorate of ammonia, was introduced immediately with the stomach-pump, and instantly followed by a solution of carbo-

nate of potash. An instantaneous effect was produced. The jaw, previously closed spasmodically, so as to impede the introduction of the stomach-pump tube, became instantly relaxed; consciousness partially returned; and on being asked a question, he could answer, though confusedly. A mixture of two grains of tartar-emetic and a scruple of ipecacuan powder now occasioned vomiting, which was kept up by warm water occasionally for half an hour; and in that time he brought up about eight troy ounces of bitter almonds. After this he quickly recovered. He had no recollection of what passed until he vomited after taking the emetic.

Part Fifth.

MEDICAL NEWS.

ADMISSION OF MEDICAL OFFICERS OF THE ARMY AND NAVY INTO THE MILITARY ORDER OF THE BATH.

We cannot join in the triumphal strains which have been sounded so loudly and generally since our brethren of the public service have succeeded in obtaining the honour recently conceded to their persevering claims. Indeed, so far from regarding this concession as calculated either to reward the deserving or to stimulate the aspiring, we look upon it as the most mortifying degradation which has, in our time, been inflicted upon the members of a liberal profession. So long as the honour in question was withheld, the public were at liberty to form their own estimate of the merit due for distinguished medical service in the army and navy; but now the inferior degree of value attached to it, as compared with that of the destructive members of a military or naval force, has been stamped in terms which admit of no mistake.

The order of the Bath is composed of Knights Grand Crosses, Knights Commanders, and Companions. For admission into the last-mentioned very numerous body, the rank of Major in the army, or Commander in the navy, is requisite, together with some service which has been deemed worthy of mention in a public despatch. Now, with exception of three knights commanders who, being at the head of their respective departments, owe their elevation to the office and not to personal claims, all the medical officers of the army and navy selected for the late distinction, have been placed in this third or lowest rank. But when it is considered that some of these men have established claims, which can hardly ever be equalled, and certainly can never be excelled, it is plain that, in the opinion of her Majesty's advisers, the greatest amount of medical service constitutes no higher claim for honorary distinction than the smallest noticeable success in the fighting department.

One of the newly-created C.B.'s has been upwards of sixty years in the service, and, more than half a century ago, was the principal medical officer of a military expedition. In all the Peninsular campaigns he played a distinguished part; and at the battle of Waterloo he commanded the medical service of the British army. This extraordinary, we believe unparalleled, extent and importance of public professional life, has been rewarded by an honour which is deemed suitable for a major of the line, who, like Sir Frizzle Pumpkin, may be carried by an unruly horse into the seeming performance of a valorous exploit, or to the naval commander who boards the enemy to avoid being drowned in his own sinking ship. There has been much absurd discussion as to the relative degree of risk to which medical and fighting men are exposed in action, as if this consideration were to regulate the distribution of honours. If the sapient disputants had studied the writings of Deputy-Inspector-General

Marshall—which, though not thought deserving of honorary distinction, have done more than those of any living man to improve the condition of the British soldier—they would have learnt that the annual risk of life to which a regiment is exposed by service in the West Indies, exceeds in the proportion of nearly two to one the average mortality of Waterloo. When Dr M'William volunteered his services as principal medical officer of the Niger expedition, he encountered a far greater danger than the leader of a forlorn hope ; and when, in the course of that ill-fated voyage, captain, officers, and crew, were prostrated by death or sickness, the helpless vessel with its skeleton inmates would inevitably have fallen a victim to the cupidity and cruelty of the surrounding savages, unless by an almost superhuman exertion of fortitude, energy, and skill, he—the despised surgical functionary—had steamed and steered his way down the pestilential river, through all the intricacies of its deceitful shoals. When Napoleon discovered merit in the meanest of his soldiers, he did not think the breast of a brave man unworthy of the cross which he had worn on his own. But Dr M'William, though a full surgeon of her Majesty's navy and the senior officer of his department in the expedition, was not considered of a rank sufficiently elevated for honorary distinction. Such being the estimation of our department in the public service, it is not surprising that the naval Assistant-surgeon, though a doctor of medicine, five-and-twenty years of age, and a gentleman by birth as well as by education, should still be deemed unworthy of associating with a Second Lieutenant of Marines, who has not exceeded the mature age of seventeen years. Earnestly desiring a more just and rational state of things, we deplore the late distinct expression of inferior regard by rewarding the *highest* quality of medical professional service through admission to the *lowest* rank of the military Order of the Bath. It may not injure the gray-headed veterans immediately concerned, but it cannot fail to have a depressing effect on the whole departments to which they belong, and sink their junior brethren even lower than they were before.

ADVANTAGES OF GROUND WINDOW-GLASS FOR MICROSCOPIC INVESTIGATIONS.

F. C. Donders makes the following remarks in the "Nederlandsch Lancet," Nov. 1850 :—For the microscopic examination of transparent bodies by transmitted light, we prefer the sunlight reflected from white clouds ; but this illumination we cannot at pleasure procure, nor, when procured, is it always of the same quality, in consequence of the movements of the clouds. The changes in the field from clear to dark, and again to the blue of the sky, are very unpleasant to the eye. This irregularity is avoided by the use of ground-glass window-panes, by means of which the blue sky-light is improved. Ground panes (with the roughened side inwards) should be placed in part of the window-frame ; panes of the ordinary description may, and indeed ought to be left in the remaining compartments, in case the blue sky-light, or direct solar rays, should be required for any particular purpose. The advantages of the ground glass are,—1st, The light is more uniform. When the rays from a large expanse of the sky impinge upon the ground glass, they become spread out in every direction, and we are thus freed from the changes due to the passage of clouds. 2d, We may dispense with the ordinary apparatus for modifying and diffusing the light, such as whitened walls, a plaster of Paris mirror, white paper, white powder on the mirror, &c. 3d, Chambers, otherwise unsuitable for making microscopic investigations in, may, with this addition, be used. In narrow streets, surrounded with high buildings, the light, falling almost perpendicularly, cannot in some cases be taken up by the mirror at the requisite angle, or the observations must be carried on at a distance from the window. The light from the obscured panes can, however, be easily caught upon the mirror. Besides, a more uniform and abundant light is thus obtained.—*Schmidt's Jahrbücher.*

VARIETIES.

On the Introduction of Calomel into Medical Practice. [Communicated by Dr Christison.]—There is no trustworthy record of the first employment of calomel in medical practice in Scotland. But that it must have been first used about the middle of the seventeenth century, or at least was very little in use until some time afterwards, will appear from the following circumstances:—A few years ago, Sir Thomas Brisbane, the present President of the Royal Society of Edinburgh, found in an old repository, belonging to the family of Lady Brisbane, the following letter, which is here given *verbatim et literatim.*

"*For mine honored and luving Cusing, Mr Thomas Hay, Clerk to the Seshionne, Edinburgh.* Honored Cusing,—Your acceptable I did receave, and from my Heart sorie of our Cusing's continuing Fluxis. According to your Desyr and his I send two Dozis of Powder, whilk I hop is ane singular Remedie; and give (*if*) it were not to him I wold hardlie spare it. He will be pleased to use the ane Paper so sonne as he receaves it, after ane licht supper, halfe ane houre or he go to Bed, in ane Peace of rosted Appell; and the Quantity of Powder being in it, and after he has swallowed over, he can tak ane Glase of Wyne, and go to Bed, and sleape in God's Name. The other Paper he can use the fourth Day after the taking of the first; and the Nicht after he takes it and Morrow he must not go out, but stay in his Chamber. God give him his Blessing to it. But above all, when he useth this he sall tak no uther Physick from no man whatsomever. It is easy to tak. Albeit he were 80 years he nead not fear, and use it als I have written. I will entreat you to keap it so closse that the Air will not hurt to it; and keap it besyd you, that it come not in evrie bodie's Hands. And when he taks it, that nothing of the Quantitie go by the littill peace of Appell made a littill brod to receave it, and then put together and swallowed over his Throat. Give he do not use it send it back. I hop you will give me Notice what Effects it taks, for it can do not hurt to old or young. I wish he were hear: please God I sould cure him. Or give Mr Andro wer there there could be no Doubt.

"With Services to you all great and small, I wayt your weal and affectionate Cusing, to serve you till death. "J. Tomesonne.

"Glasgow, August 4, 1666."

The patient seems to have been content with the half of "ane Paper;" for both were marked outside 10 grs.; and one contained that amount, the other five grains only. The contents consisted of a few heavy, fibrous, white particles, of a pearly lustre, which proved on analysis to be pure calomel, very coarsely pulverised. The rank of the patient,—the clerks of session being usually in those days younger branches of families of some station,—the distance the remedy was transmitted,—the small quantity which Dr Thomson could spare,—his unwillingness to part with it,—the mystery with which he speaks of its action,—and his anxiety to get it back, if not used,—all show that in 1666 calomel was very little known in medical practice in Scotland.

Medical Legislation in Georgia, U.S.—An amusing debate took place in the senate of Georgia on the 9th February, on a bill being introduced "to *license* one Stephens, of Gilmore county, to practise medicine *without license.*" Dr J. R. Smith opposed the measure, on the ground that no man should be allowed to put himself upon a community as skilled in medicine, unless he had been found so, as by the very act he might, in critical cases, be trusted to the ruin and death of worthy and excellent citizens.

Mr Edmonson supported the bill. He knew nothing of Stephens' qualifications, but hoped the pleasure of his constituents (who were anxious for the passage of the bill) would be consulted.

Mr Smith—when a motion for postponement was lost, moved as an amendment, the interpolation of an additional section, "that all laws requiring applicants to undergo examination in order to practise, be abolished."

Other amendments having been proposed, Mr Ferrel moved : "That the said Stephens and others shall not administer the steam-bath more than three times to any patient in twenty-four hours, nor give a dose of *wake-robin* [root of *Arum triphyllum*, a powerful acrid poison] until the patient has been dead at least twelve hours, nor mountain root, only in extreme cases."

Mr M'Bee hoped that *sinators* would not stick on so many amendments likely to injure the bill.

The measure was ultimately carried, there being 22 yeas to 14 nays ! !—(Abridged from *Philadelphia Medical Examiner*, April 1850.)

New Treatment for Epidemic Jaundice.—A writer in the "New Orleans Medical and Surgical Journal" makes the following statement, in a paper which has been reprinted in the "Boston Medical and Surgical Journal :"—"The first case that came under my charge was in a young man 23 years old. *I undertook to vomit him*, for the purpose of arousing his liver. * *

"*I succeeded in vomiting another patient* freely, after giving a considerable quantity of ipecac. and tartar-emetic, and found some slight traces of bile after severe straining."—*Boston Med. and Surg. Journal*, No. 1172, p. 524.

Dr Warren's Proposition.—Dr Warren, of New York, has seriously proposed, that when a medical man believes that there is no hope or chance of recovery, his duty is to interpose the means by which the sufferer may be softly and agreeably conducted through the final moments ; and that, with the knowledge and full assent of the friends, the vapour of ether should be employed under such circumstances to occasion insensibility.—*New York Med. Gazette*, July 13, 1850. [Not to mention how very fallible the belief of a medical man may be, it is only necessary to say, that in this country such a practice would subject him very properly to the charge of manslaughter.]

Epidemiological Society.—An association has been recently formed in London for the investigation of epidemic diseases. Dr Babington is the President. —*Medical Gazette*, Aug. 16, 1850.

PUBLICATIONS RECEIVED.

Treatise on Diseases of the Larynx and Trachea. By John Hastings, M.D. London : Longman. 1850.

Cholera and its Cures. By J. Stevenson Bushnan, M.D. London : W. S. Orr & Co. 1850.

Das Krampfhafte Asthma der Erwachsenen. Von Dr J. Bergson. Nordhausen. 1850.

Principal Diseases of the Valley of North America. By Daniel Drake, M.D., Cincinnati. Winthorp B. Smith & Co. Vol. i. 1850.

Lecture introductory to the course on Surgery, delivered at the Massachusetts Medical College in Boston. By Henry J. Bigelow, M.D. Boston, 1850.

An Address, delivered before the Suffolk District Medical Society. By John Jeffries, M.D. Boston, 1850.

The Boston Medical and Surgical Journal, Nos. 1147 to 1154 inclusive ; likewise monthly part of August 1, 1850, with communication from the publisher.

Proceedings of the Medical Society of the State of Pennsylvania. April, 1850.

The Anatomy, Physiology, and Pathology of the Eye. By Henry Howard, M.R.C.S.L. Surgeon to the Montreal Eye and Ear Institution. London : Churchill ; and Montreal : Armour & Ramsay. 1850.

Journal für Kinderkrankheiten. Herausgegeben von Behrend und Hildebrand. Erlangen, 1850. Heft i., bis 6.

Adolph Henke's Zeitschrift für die Staatsarzneikunde fortgesetzt von Behrend. Erlangen, 1850. 1stes und 2tes Vierteljahrheft mit vierzigstes Ergänzungsheft.

Practical Suggestions for the Prevention of Consumption. By G. Calvert Holland, M.D. London, 1850.

A Practical Synopsis of Diseases of the Chest and Air Passages, &c. By James Bright, M.D. London, 1850.

Corpulence ; the Gulstonian Lectures on, in 1850. By Thomas King Chambers, D.M. London, 1850.

On a New and Successful Treatment for Febrile and other Diseases, through the Medium of the Cutaneous Surface. By William Taylor, M.R.C.S. England. London, 1850.

Deafness Practically Illustrated. By James Yearsley, M.R.C.S.E. Third Edition. London, 1850.

Report of the General Board of Health on the Epidemic Cholera of 1848 and 1849 ; with Appendix (A), by Dr Sutherland, and Appendix (B), by Mr Grainger. London, 1850.

Gazette Homœopathique de Paris. No. 25.

TO CORRESPONDENTS.

Communications have been received from J. B. Thomson, Esq., Tillicoultry; Adolph Büchtung, Nordhausen; M. le Docteur Debout, Redacteur du Bulletin Général de Thérapeutique, Paris; Robert Smith, Esq., Aberdeen; Dr Schœpf Merei, Pesth. Others have been privately acknowledged.

POSTAGE TO THE AMERICAN COLONIES.

We had recently occasion to negotiate an exchange of numbers with the "British American Journal," published at Montreal; and, in compliance with the wish of Dr Hall, the editor, were anxious to avail ourselves of the facilities given by the post-office for the transmission of periodical publications. On inquiry at the post-office, we were informed, that for each number of the Monthly Journal (weighing above five ounces) sent to Montreal, a postage of 14s. would be charged. As Dr Hall continued to assure us that there must be some mistake, and that he received the "Dublin Quarterly Journal" (weighing fourteen ounces) for a postage of 1s. 3d.; and as, besides, the "British American Journal" had been hitherto delivered at our publishers' for a trifling postage, we wrote to the Secretary to the General Post-office for Scotland, inclosing copies of all the journals above mentioned, and requesting precise information on the subjects of the postage chargeable on each between this country and Montreal. The answer which we received at once satisfied us that we must continue to transmit our numbers by bookseller's parcel as heretofore :—

General Post-Office, Edinburgh, 24th August 1850.

Sir,—I beg to acknowledge the receipt of your communication of the 21st inst., and to acquaint you that there has been no provision made for the transmission of periodical works, such as the accompanying, to the British colonies in North America.

The "Monthly Journal of Medical Science," according to the weight of the number enclosed, if sent by post to Montreal, would be liable to a postage of 14s., being above the weight of five ounces; but if specially addressed "*via* Halifax," the postage would be 2s. less. In both instances the postage is the same, whether prepaid or unpaid,—viz. 14s. and 12s.

The "Dublin Quarterly Journal of Medical Science" would be subject to a postage of L.1, 15s., or if specially addressed "*via* Halifax," of L.1, 10s., the number enclosed exceeding the weight of fourteen ounces. I am unable to explain how this work could be delivered by post in Montreal for 1s. 3d.; it is clear, however, that it could not be sent by post from this country at the rate stated to be paid for it in America.

Such works, according to the treaty of 1848, can be forwarded to the *United States* at the following reduced rates, namely,—

1st. For every work not exceeding 2 ounces, 1d.
2d. Do. above 2 and not exceeding 3 ounces, 6d.
3d. Do. above 3 and not exceeding 4 ounces, 8d.

4th. And for every ounce above 4 up to 16 ounces (the limit imposed in the transmission of such articles by the British office), 2d. additional, every fraction of an ounce being reckoned a full ounce; but they must be sent in bands or covers, open at the sides or end, so that they may be easily examined.

The "British American Medical and Physical Journal" is liable, according to the weight of the number enclosed (being above 1 ounce), to a postage of 4s. 8d. on delivery in this country; but these small publications are generally put up in covers, the same as newspapers, and are frequently passed as such through the post, there being little time, on the arrival of an American mail, to make a minute examination of these papers, which may probably account for your receiving this Journal at a trifling cost.—I am, &c.

Fred. Godby, *pro Secretary.*

Dr Robertson, 28, Albany Street, Edinburgh.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Fibrous Polypus.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

LORD —— —— applied to me on the 29th of June last, under the following circumstances:—About three years ago, while employed in the public service abroad, he had fallen from horseback, and sustained an injury of the nose, followed at different times by profuse bleeding,—on one occasion to the amount, it was said, of several pints,—and complete obstruction of the nasal passages, with the loss of smell and taste. Some ineffectual attempts to afford relief had been made by a military surgeon, who detected a polypous growth in the right nostril, and extracted small portions of it. The patient, in quest of more efficient assistance, had come home, and placed himself under the care of an eminent surgeon in London, who made repeated attempts at extraction, without success, and then proposed a consultation, which led merely to the prescription of alterative medicines. Two other very distinguished surgeons of the metropolis were then taken into the case, with the result of discovering a tumour in the throat. It was proposed to remove this growth by cutting through the soft palate; and deliberations were held as to the practicability and expediency of such a procedure. But, finally, at the end of two months, the original resolution against any operative interference was unanimously adopted. The patient then came to Edinburgh, bringing with him a letter to me from the gentleman first consulted, who stated in it, that he and the three other surgeons had, after careful examination, and repeated deliberations, resolved that the case did not admit of any beneficial interference.

The patient was about 34 years of age, tall, thin, and well made, without any expansion of the nasal feature, which, on the contrary, was rather narrower than usual. Upon putting my finger into the pharynx, behind the soft palate, I was surprised to recognise in the tumour, which had excited so much alarm, merely the extension of a polypous growth from the nose backwards, and offered at once to remove it without delay or ceremony. But to this proposal it was objected by the patient, and two of his brothers who accompanied him, that it would be requisite to obtain the consent of their friends in London before anything should be done in a matter engaging

the sympathies of a large family connection, and in opposition to the judgment of so many distinguished surgeons. Letters, therefore, were despatched to the principal parties concerned, and in the meantime I repeated my examination of the case.

Both nostrils were completely obstructed, in regard to inspiration as well as expiration. There was nothing to be seen or felt in the left one; but in the right a small portion of polypus, similar to the ordinary mucous growth, could be perceived. Pressure upon the pharyngeal tumour excited profuse bleeding, not so much from the throat as from the nose. At all times there was a copious distillation from the nostril of slightly coloured fluid, which, as it could not be expelled by blowing, admitted of removal only by a wringing sort of action, hardly less disagreeable to witness than to endure. At night, notwithstanding every precaution, the bed-clothes were soiled by this never-ceasing flow of watery discharge. When the certain, and not remotely fatal termination of his complaint, which had been predicted to the patient by his London attendants, is added to these constant sources of distress, it may be imagined that his state was nowise comfortable, and that he gladly availed himself of the sanction which in due season arrived for the execution of my proposal.

On the 7th of July, having requested my friend Dr Duncan to give me his counsel and assistance, I introduced the small polypus forceps, always employed by me, into the right nostril, and co-operating with the fore and middle fingers of my left hand, inserted behind the soft palate, speedily extracted the morbid growth in one mass through the nose. The bleeding, which had been very profuse, immediately ceased, and the patient breathed with perfect freedom by both nostrils. He remained quite well ever afterwards; and three months having passed without any threatening of a relapse, has gone to resume his duties in the colony to which he was attached.

The polypus, when examined, was found to consist principally of a fibrous substance, possessing a reddish colour, and a texture so very firm as to resist almost any degree of extensive force. At the part farthest from its centre, where it descended into the pharynx, the growth had a bulbous form, and more friable consistence. It thus, in all respects, completely agreed with the characters of what M. Dupuytren designated the "fibrous polypus"—namely, an extremely firm consistence at the central or original part, a tendency to soft degeneration at the circumference, a great disposition to bleed, a strong adhesion to the surface of bone upon which it grows, and the absence of any malignant action after its complete evulsion. Such cases are very rare, and I consider myself fortunate in having witnessed one of the two which Dupuytren has particularly related. In my own practice I have met with some remarkable examples of the disease, especially one in which I found it necessary to remove the superior maxillary bone in order to obtain the access requisite for applying forceps with effect; and another, where, having divided the upper lip to obtain sufficient space, I found that the polypus grew from the

inferior spongy bone, and readily detached it by means of the cutting pliers. In the latter case, the patient, though nearly exhausted by hemorrhage previously to the operation, enjoyed good health for many years afterwards, and, so far as I know, still continues to do so.

In conclusion, I may remark, that the source of Lord —— ——'s complaint probably existed long before the accident to which it was attributed, since he breathes with more freedom, and, on the whole, has a feeling of greater comfort, than he recollects to have enjoyed at any time previous to the operation.

ARTICLE II.—*On the Fœtus in Utero, as Inoculating the Maternal with the Peculiarities of the Paternal Organism; and on Mental States in either Parent, as Influencing the Nutrition and Development of the Offspring.* By ALEXANDER HARVEY, M.D., Lecturer on the Practice of Medicine in the University and King's College of Aberdeen, and Physician to the Aberdeen Royal Infirmary.—(*Continued from* p. 310.)

II. THAT mental causes, or states of mind, operating within the female during pregnancy, or within either parent at the time of coitus, may variously influence the nutrition and development of the fœtus, has long been matter of popular belief; and, setting a goodly number of recorded instances to the account of old wives' fables, this belief may be allowed to have a stable foundation in facts.[1] It is not my intention, however, to enter on the general subject farther than as it bears on Mr M'Gillivray's theory, referring merely to such facts as serve to show, that mental causes may so influence the growth of the fœtus as to produce results analogous to those ascribed to inoculation by a former fœtus, and therefore to exhibit a source of fallacy in the reference of these to such inoculation as their cause.

(1.) Of the cases of this kind now to be noticed, some involve changes both in the configuration and the colour of the progeny,—others, so far as appears, changes in the colour only or chiefly.

In Daniel's "Rural Sports" the following details are given respecting the setter-bitch and cur-dog, formerly referred to:—

"As the late Dr Hugh Smith was travelling from Midhurst into Hampshire, the dogs, as usual in country places, ran out barking as he was passing through the village, and amongst them he observed a little ugly cur, that was particularly eager to ingratiate himself with a setter-bitch that accompanied him. Whilst stopping to water his horse, the doctor remarked how amorous the cur was, and how courteous the setter seemed to her admirer. Provoked to see a creature of *Dido's* high blood so obsequious to such mean addresses, the doctor

[1] Any one curious in cases of this kind may advantageously consult Dr Allen Thomson's article on "Generation," in the Cyc. of Anat. and Physiology; and also an article in the Ed. Med. and Surg. Journal, vol. xxv., p. 134.

drew one of his pistols, and shot the cur. He then had the bitch carried on horseback for several miles. From that day the setter lost her appetite, ate little or nothing, had no inclination to go abroad with her master, or to attend his call ; but seemed to pine like a creature in love, and express sensible concern at the loss of her gallant. Partridge season came, but Dido had no nose. Sometime after, she was coupled with a setter of great excellence, which, with no small difficulty, had been procured to have a breed from, and all the caution that even the doctor himself could take was strongly exerted, that the whelps might be pure and unmixed. Yet not a puppy did Dido bring forth but was the *exact picture* and *colour* of the cur that had so many months before been destroyed. The doctor fumed, and, had he not personally paid such attention to preserve the intercourse uncontaminated, would have suspected that some negligence had occasioned his disappointment ; but his views were in many subsequent litters also defeated, for Dido never produced a whelp which was not exactly similar to the unfortunate cur, who was her first and murdered lover."[1]

In Mr Blaine's "Encyclopædia of Rural Sports" this other case is given :—

"The late Lord Rivers [says Mr Blaine] was famed for a breed of black-and-white *spaniels*, one of which, having more than the usual quantity of white, he presented to us. We had, at the same time, a *pug-bitch* of great beauty. The attachment of this bitch to the spaniel was singularly strong. When it became necessary to separate her, on account of her heat, from this dog, and to confine her with one of her own kind, she pined excessively ; and, notwithstanding her situation, it was some time before she would admit the attentions of the pug-dog placed with her. At length, however, she was warded by him, impregnation followed, and at the usual period she brought forth five pug-puppies, *one of which was perfectly white, and rather more slender* than the others, though a genuine pug. The spaniel was soon afterwards given away. At two subsequent litters (which were all she afterwards had) this bitch also brought forth a *white* pug-pup, which the fanciers know to be a very rare occurrence. It is also a curious fact, that each succeeding white puppy was *less slender* in form than the preceding, though all were equally white."[2]

The two cases now given have many points in common, and appear to be free from any material source of fallacy. In the former there was not even sexual intercourse—much less fruitful intercourse—between the bitch and the cur, to whom her progeny bore so decided a resemblance; and in the latter, if intercourse occurred, which it appears did not, there was no result from it. In both females there seems to have been a strong and an abiding attachment to the cur and spaniel respectively, and an equally keen and enduring emotion of disappointment at being separated from them. The resemblance of the progeny, or of certain of these, to the dogs, appears to have been of too special a kind to admit the supposition of its being accidental. There need be no question, therefore, but that the cases are fair examples of mental impressions of a permanent character so operating on the female parents as to influence

[1] Daniel's Rural Sports, vol. iii., pp. 333, 334.—A case very similar to the above, occurring in a bitch belonging to him, has been mentioned to me by Mr Walker, Portlethen, in Kincardineshire.

[2] Encyclopædia of Rural Sports, by Delabere P. Blaine, Esq., p. 412.

the development of their offspring while in utero, and that in such a manner as to cause them to resemble the male animals that were the objective causes of those impressions.

Nearly similar remarks apply to the following case, for the particulars of which I am indebted to Dr John R. Trail, of Monymusk, in Aberdeenshire. The chief point of interest in this case lies in the resemblance being at once general, and yet extending to an abnormal peculiarity of conformation, and in its thus forming a connecting link between the two foregoing cases and two others presently to be adduced. It does not appear, indeed, whether the mother manifested any special attachment to the male animal which her offspring resembled, but such attachment may reasonably be presumed.

A mare and a horse (a gelding), belonging to a friend of Dr Trail, had for some years worked together on the same farm, occupied adjacent stalls in the same stable, and pastured together in summer in the same fields. The gelding was of a black colour, with white legs and face, and had a singular peculiarity in the form of the hind legs, which, when the animal was standing, appeared as if quite straight, there being no appearance of the leg being bent at the hough-joint, as in ordinary cases; the pasterns likewise were very long, so as to cause the feet to look as if placed almost at right angles to the legs.

Having been some years thus associated with this gelding, the mare was covered by a stallion of the same colour with herself,—both stallion and mare being of a bay colour, with black legs, and a small spot of white only on the forehead. The foal which was the produce of this connection very exactly resembled the gelding in *colour*, and in the *shape*, too, more particularly of the hind legs, as above described. "From the description I have attempted to give you [Dr Trail writes me], you could not form any very distinct idea of the peculiar conformation of the horse; but the resemblance of the foal to him was remarkably clear."

Dr Montgomery, of Dublin, gives the following interesting case:—

"A lady, pregnant for the first time, to whom I recommended frequent exercise in the open air, declined going out as often as was thought necessary, assigning as her reason, that she was afraid of a man, whose appearance had greatly shocked and disgusted her: he used to crawl along the flag-way, on his hands and knees, with his feet turned up behind him, which latter were malformed and imperfect, appearing as if they had been cut off at the instep,—and he exhibited them thus, and uncovered, in order to excite commiseration. I afterwards attended this lady in her lying-in; and her child, which was born a month before its time, and lived but a few minutes, although in every other respect perfect, *had the feet malformed and defective, precisely in the same way as those of the cripple* who had alarmed her, and whom I had often seen."[1]

Nothing can be more appropriate than Dr Montgomery's short commentary on this case:—"Here was an obvious and recognised object making a powerful impression, of a disagreeable kind, complained of at the time, and followed by an effect in perfect accordance with the previous cause,—there being between the two a simi-

[1] Exposition of the Signs and Symptoms of Pregnancy, &c., pp. 16, 17.

larity so perfect, that, with Morgagni, I 'will not easily suppose that chance could have been so ingenious, if I may be allowed to speak thus, and so exact an imitator.' "

A case very similar to the foregoing has been communicated to me by a distinguished English physiologist,—himself personally cognisant of the facts. It is as follows :—

" A lady, when two or three months pregnant, was accosted by a *one-armed* beggar, who, on her refusing to relieve him, menaced her violently, so as to alarm her seriously, and shook his stump at her. She was extremely agitated, and during the whole remainder of her pregnancy was under the firm expectation that her child would be one-armed,—which was the case. This child is now grown up to manhood, and occupies a highly respectable position in society."

In this case (and the remark applies equally to Dr Montgomery's) the effect was *partial* only, and on this account it may be thought not altogether pertinent to the subject under consideration; but it probably embraced all that was peculiar in the object, as it certainly did all that was striking to the mind of the lady. And had the occasion of the impression been some well-marked but normal peculiarity of the features,—*e.g.*, of the nose,—instead of a deformity of the arm; and had the man been the object formerly of the lady's love and attachment, and still during her pregnancy of her cherished regard,—and had the resemblance in her child extended to the part supposed,—the effect, though equally partial, might have seemed more general. The case might thus have been set down as a fair example of the power of the imagination, in a pregnant female, to cause her offspring very exactly to resemble an individual not its father. Of this the following is, perhaps, an instance :—

A young married woman, residing in Aberdeen, between whom and a young man a strong attachment and a matrimonial engagement had long existed, but who were never married, and never had sexual intercourse together, gave birth to a child, which bore so striking a resemblance in its features to the woman's first lover, as to attract the notice of herself and many others of the acquaintance of the parties.

In this case—communicated to me by Mr Robert M. Erskine, surgeon, here, who was well acquainted with the individuals concerned, and had personally satisfied himself of the accuracy of the facts—the resemblance may have existed only in the imagination of the observers, or been magnified through the love of the marvellous,—and, giving it as given to myself, I adduce it merely as a possible example of what may be a real occurrence, and would contrast remarkably with the observation alluded to by Dr Allen Thomson,—viz., " that the human female, when twice married, bears occasionally to the second husband children resembling the first, both in bodily structure and mental powers,"—and also with the cases given as instances of this, in the Appendix to my former paper.

It appears that many breeders of stock are impressed with the belief, that certain *colours* present to the eye of the parent animals,

and particularly of the female, at the time and in the act of their being coupled together,—and to the eye of the female both before and during her pregnancy, influence the *colour* of the progeny; and that they make this belief a practical principle of action in the breeding of their stock, in order either to prevent or to secure the admixture of any particular colour in the offspring different from that of the parent animals. "We know," says an anonymous writer, "a great breeder of pure Angus stock [black polled breed], who makes it a rule to have every animal about his farm of a black colour, down to the very poultry."[1] And an eminent breeder of the same stock in this county informs me, that he extends this rule to the *steadings* in which his cattle are kept.

To illustrate generally the grounds of this belief and practice, the following cases may be cited:—

(*a*) A black polled [Angus] cow, belonging to Mr Mustard, a farmer in Forfarshire, came into season while pasturing in a field bounded by that of a neighbouring farmer. Out of this last there jumped into the other field an *ox*, of a *white colour*, with black spots, and *horned*, which went with the cow till she was brought to the bull,—an animal of the same colour and breed as herself. Mr Mustard had not a horned animal in his possession, nor any with the least white on it; and yet the produce of this (black and polled) cow and bull was a black-and-*white* calf, with *horns*.[2]

(*b*) Last year (1849), twenty cows of the black polled Angus breed,—belonging to Mr William M'Combie, in this county, and whose stock is perhaps the finest in the kingdom,—produced as many calves, all of them black and polled, except one single calf, which was *yellow*-and-*white spotted*. Mr M'Combie had, as usual with him, taken the precaution of causing the cows, both before and during their pregnancy, to mix with none save perfectly black cattle, except in respect of the mother of this calf, which cow had unwittingly been put to an out-farm, to be starved, in order to fit her for the bull. There, for a considerable period prior to her being served with the bull, she had grazed with a large *yellow*-and-*white spotted ox*, of which ox the calf she subsequently bore was the very picture,—the likeness, however, extending no farther than to the colour, and the calf still retaining the shape and configuration of its parents, which were both of the same breed and colour.

(*c*) Out of a large herd of cows, of the pure Teeswater breed, all of them of the brown or roan colour (belonging to Mr Cruickshank, Sittyton, near Aberdeen), there is every year dropt one, or at most two, *white* calves, which, in order to prevent the introduction of this colour among the cattle, are invariably sold, and sent away. Last year, however, concurrently with the *white-washing* of all the farm-steadings, the very large number of twelve white calves were produced. And the like occurrence happened last year also, in the herd of an extensive breeder of the same kind of stock, in Yorkshire, in connection with the like process of white-washing,—this process having, in both cases, been very extensively carried out before the breeding season began, with the view of preventing the breaking out of the pleuro-pneumonia, then epidemic in the neighbourhood, and very destructive.[3]

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[1] "North of Scotland Gazette" newspaper, for July 17, 1849.

[2] Library of Society of Useful Knowledge, volume on Cattle, p. 171.—This seems to be the same case as that given by Dr Allen Thomson, in Cyc. Anat. and Phys., art. "Generation," vol. ii., p. 474.

[3] Communicated by Mr Cruickshank, who informs me further, that he has had too many proofs of the agency of the cause in question to allow him entertaining any doubt on the subject.

As exhibiting the influence of mental causes operating at the time of coitus, the two cases following may be cited,—the one given by Dr Allen Thomson, the other communicated to me by Mr William M'Combie :—

(*a*) "At the time when a stallion was about to cover a mare, the stallion's pale colour was objected to, whereupon the groom, knowing the effect of colour upon horses' imaginations, presented before the stallion a mare, of a pleasing colour, which had the desired effect of determining a dark colour in the offspring. This is said to have been repeated with success in the same horse more than once." [1]

(*b*) "I was told (Mr M'Combie writes me) by an old servant of mine, Morrice Smith, that, when he was a servant in the parish of Glass (Aberdeenshire), a black bull served a black cow at the time when a white mare passed them, and that the produce was twin *white* calves. There were no white cattle upon the farm where this occurrence happened."

Such cases as several of those now cited can scarcely fail to recal to one's mind the story, given in the book of Genesis (chap. xxx.), of Jacob and his peeled rods, and the effect of these in causing the flocks, before whom they were placed at the time of conception, to bring forth ring-straked, speckled, and spotted cattle.

It does not appear from the sacred narrative whether the influence of the rods was exerted on the minds both of the male and female cattle, or confined to those of the female. But it seems probable (Gen. xxxi., 5-12) that the effect was a *supernatural* one, and designed to enrich the needy patriarch at the expense of the crafty Laban, by whom, for fourteen years, he had been sore let and hindered "in providing for his own house;" and that Jacob had beforehand, in a dream, intimation given him of the design, as well as a sensible representation both of the agency to be employed and of the result which was to follow.[2] If this were the character of the transaction, it would be unwarrantable to draw any inference from it in relation to the present inquiry, unless it could be shown, which I apprehend it cannot, that the same expedient will now, as then, produce the like results. At the same time it may, perhaps, be not

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[1] Art. "Generation," in Cyc. Anat. and Phys., vol. ii., p. 474.

[2] Dr Allen Thomson, in his comment on this transaction (Cyc. Anat. and Phys., art. "Generation"), seems to me to have very completely missed the import of the essential parts of the narrative. He gathers from it, that Jacob had parti-coloured males to breed with, and makes the result an affair of mere hereditary transmission. But Jacob had not this advantage, and it seems clearly to have been part of his own proposal to Laban that he should not. Laban, at least, on the bargain being concluded, very carefully singled out all the animals of that sort,—males as well as females,—even "to every one that had some white on it, and all the brown among the sheep;" and, giving them into the hand of his sons, set three days' journey betwixt this parti-coloured flock and that which was to be tended by Jacob. A difficulty, it may be added, attaches to the right understanding of the whole matter, from the obscurity of v. 40,—the true meaning, if not the proper rendering, of which appears to be this :—"And Jacob did separate the lambs, and set aside from the flock all the ring-straked and all the brown in the flock of Laban; and set them apart for a flock to himself, and put them not unto Laban's cattle."

unfairly referred to, in illustration of ordinary phenomena of a somewhat analogous kind. Read by the light of these, it may be held to indicate that the Almighty accomplished His purpose simply by *enhancing* a natural agency—"moulding it secretly in the hollow of His hand,"—and so be regarded as reflecting back light in its turn upon that agency. The validity, however, of this appeal must turn upon our being able to satisfy ourselves, on independent grounds, that there is in nature an agency of this kind, and its precise value upon the extent to which we can thus ascertain that it ordinarily operates,—questions these for the solution of which we have not at present the requisite data.

(2.) All that need be said in the way of direct inference from the facts brought together in this division of the subject, may be comprised within a narrow compass.

Supposing the statements respecting them to be authentic—and no question, I apprehend, as to this can well be raised—the cases are nearly unequivocal. The only fallacy that can attach to them, is that arising from the possibility, that the peculiarities in the progeny were either purely *accidental*, or owing to corresponding qualities *latent* in the parents, but breaking out in the offspring. The relation, however, in most of the cases between the peculiarities in question and their presumed causes, is too close and of too special a character to admit of either supposition. We are, therefore, well entitled, I think, to regard the greater number, if not the whole of them, as examples of mental causes so operating either on the mind of the female, and so acting on her reproductive powers, or on the mind of the male parent, and so influencing the qualities of his semen, as to modify the nutrition and development of the offspring.

How, in respect of the female, this influence is exerted,—what is its *modus operandi*,—and what the conditions of its agency, it is not easy to determine. The mental affections seem to have been in most of the cases, and were probably in all of them, of a *strong* and *enduring* kind; and we can easily conceive this to have been essential to the result. That the alteration in the growth of the fœtus was determined solely, as is vulgarly supposed, by the *images* in the mind of the mother,—*i.e.*, by the mere *sensations* and *perceptions* therein produced, independently of the *emotions* excited by them,—cannot well be supposed. It is, doubtless, to this "*compound state*" of mind—to use an expression, without adopting, however, the psychological theory, of Sir James Mackintosh—"easily called to mind," in consequence of the vividness of its first impression, "frequently recurring," and "warmly felt,"[1] that we must ascribe the effect.

It is not unlikely that this particular agency of the mind is more frequently exerted in the females of the lower animals than in those of our own species; and that cases exemplifying it are oftener met with in the brutes than in man. If this be so, a reasonable explana-

[1] Ethical Philosophy, edited by Whewell, 2d edition, pp. 397, 398.

tion of the fact may be given. We know that the minds of the lower animals are in a great measure limited to *particulars*, and these few in number, and almost exclusively external objects of sense; that the external senses are more perfect in them than in us; and that the perceptions resulting from their exercise seem, in various instances, to follow more surely and more quickly—to be more intuitive and wider in their scope, and more vivid—in them than in man; and that the simpler emotions (excited by those perceptions) of joy, fear, affection, anger, &c., of which they are manifestly susceptible, seem often to be peculiarly strong.[1] We know also that they possess the faculty of *memory;* and we may well suppose, from their limited range of association (or suggestion), that sensations that formerly made a powerful impression on their minds, will be more easily and oftener recalled in them than in us, who, though more apt to be "troubled about many things," are proportionally less apt to be affected, or at least permanently or continuously impressed, by any one thing. These circumstances and peculiarities of mental action must obviously be singularly favourable to the production of the results in question,—keeping in mind, that the mental agency under consideration is manifestly closely connected with *sensations* and with simple *perceptions* and *emotions* thence resulting, and with a certain *intensity* and *endurance* of all these.

That the parts of the female system specially affected by this "compound state" of mind are the blood and nutritive processes generally seems very probable; that the *kind* of change, however, therein produced, is rather *dynamical* than organic, has (just as in regard to the *inoculation* principle) been suggested to me by Dr Carpenter; that it may be of identically the same nature with the change produced by the inoculation principle is quite possible, the two, however different in their origin, thus running up into, and meeting in, one *common* principle,—to wit, a modification of the constitutional or dynamical powers of the female; and that it may be more or less *persistent*, and operate on a subsequent conception, independently of a renewal of the mental state itself, some of the cases seem to indicate. That the change in question is produced through the medium of the nervous system, we cannot doubt—"that being the acknowledged seat and instrument of mental acts;" and that the parts of the nervous system more immediately concerned, in addition to certain great nervous centres (the sensory and emotional ganglia[2]), are the ganglionic nerves, may reasonably be conjectured—these nerves being the only channels by which the blood and the nutritive processes can be brought into relation with the mind.[3]

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[1] See art. "Instinct," in Cyc. of Anat. and Phys.

[2] See Carpenter's Manual of Physiology, p. 522, et seq.; and Todd and Bowman's Physiological Anatomy, &c., vol. i., chap. xi.

[3] Alison—Outlines of Physiology, 3d edition, pp. 402-4; and Physiological Inferences from the Study of the Nerves of the Eyeball, in Medical Gazette, No. 705 (June 1841), p. 410, et seq.

Beyond this we cannot venture to go in the way of speculation. The singular influence thus exerted by the mind of the mother on the growth of the fœtus, is not one "for which," as has been remarked of other modes of action of the mind on the body, "it is likely that we shall ever be able to assign a reason, or which it would be any great hardship to be obliged to regard as an ultimate fact in physiology." [1]

III. Reverting now to the theory of inoculation, and to the cases cited as instances of it, it will be obvious, from the facts brought together under the preceding head, that in any ordinary case where an animal resembles a male, not its progenitor, by which its mother had on a former occasion been impregnated, the resemblance may be explained as well on the principle of mental agency as on that of inoculation; or, at least, that in ascribing it to this latter cause a manifest source of fallacy attaches to the assumption.

To obviate this, and to determine conclusively whether or not the phenomenon is independent of mental agency, all that seems necessary is, to institute experiments in breeding on a sufficiently large number of different kinds of animals, selecting animals of acknowledged purity of blood, and conducting the experiments so as to exclude (which I understand could easily be done) the agency of any such mental impressions on the minds of the animals concerned, as could reasonably be supposed to influence the results. Should these be of an affirmative kind, and be, at the same time, sufficiently numerous and decided, it would follow conclusively that the phenomenon is not due to mental causes. In connection, however, with this set of experiments, it would be desirable to institute another set in which every advantage should be given to the agency of mental causes. Should the results from these be equally affirmative—although this circumstance would not vitiate the inference fairly deducible from those of the other set, unless by exhibiting in these any source of fallacy otherwise perhaps imperceptible—they would afford points of comparison and contrast, and thereby enable us the better to appreciate the value of that inference; while, if they should be wholly or in a great measure negative, it is clear that they would add greatly to the conclusiveness of the inference in question.

The following is a detailed outline of the most important of these

EXPERIMENTS.

First Set.—To determine how far the circumstance of a previous impregnation by a male of a different species, breed, or colour, from the female—the agency of mental causes on the result being excluded—influences the conformation or colour of the offspring subsequently borne by her, to a male of the same breed and colour as her own.

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[1] Alison—On the Physiological Principle of Sympathy,—Transactions of Edinburgh Medico-Chirurgical Society, vol. ii., pp. 223, 224.

The female to be served the first season by a male of a different species, breed, or colour from her own; and the following season, by a male of the same breed and colour as herself,—taking these precautions against the agency of mental causes, *first*, and generally, that the female shall not at any time, up to the period of her second *accouchement*, have seen the former of those two males, nor any animal of that species, breed, or colour; and, *secondly*, and particularly, that the coitus between her and that male shall be so managed as to prevent her from seeing him; that, subsequently, during this her first pregnancy, the female shall be kept exclusively with animals of her own breed and colour; and that, immediately on her delivery, the (cross) animal produced be taken from her, and ever after kept out of her sight.

Second Set.—To determine how far mental causes, operating through the external senses, influence the colour and conformation of offspring.

I. Mental causes operating at the time of coitus only, or at least originating then.

The male and the female, both of the same breed and colour, in being coupled together, to have fully in their view animals of a different species, breed, or colour, or objects of any kind of a different colour, regard being had, in respect of the matter of colour, that this present a marked contrast to that of the animals concerned, and, particularly, that it be of a bright or striking hue.

To prevent, as far as possible, any influence of this kind from operating on the mind of the female during her pregnancy, the female to be made to herd exclusively, till after her delivery, with animals of the same breed and colour with herself.

II. Mental causes operating on the mind of the female during pregnancy only.

The male and the female, both of the same breed and colour as before, to be coupled together in the usual manner (or in the presence of animals of the same sort only); but the female, during her pregnancy, to herd exclusively with animals of a different species, breed, or colour.

III. Mental causes operating both at the time of coitus, and subsequently during the pregnancy of the female.

The male and the female, in being coupled together, to have fully in their eye animals of a different species, breed, or colour; and the female, during her pregnancy, to herd exclusively with animals of this sort.

The details now given embrace the principles on which, as it seems to me, decisive experiments might be made, and also the precautions necessary to be attended to, in order, as far as possible, to avoid fallacy in the results obtained from them. They might be very variously modified, and ought certainly to be tried on many different species of animals, in order to give at once a breadth and precision to the results.

Should it be clearly established by them, that an animal, the produce of a male and female of the same breed, born subsequently to the impregnation of the female at a former period by a male of a different breed or species from her own, has, from the single circumstance of such previous impregnation of its mother, its nutrition and development so far changed as to resemble that male animal, not its

progenitor, the experiments would farther supply materials towards determining the extent to which the fact holds, and the conditions essential to its occurrence. And, in particular, they would furnish more or less satisfactory answers to the following questions,—themselves suggestive, both of modifications in the experiments, and of the points to be attended to in noting the results.

1. Does the fact hold *universally*, or only *pretty often*, or only *occasionally* and *rarely*?
2. Does the animal so produced partake *decidedly* or only *partially* of the characters of the male animal which it resembles,—in *colour* merely, or in *conformation* also?
3. Does the degree of resemblance depend on the number of times the mother may have conceived by that male, before bearing the animal in question?
4. Is the resemblance lessened or in anywise affected by the female having been *first* impregnated by a male of her own breed and colour, prior to her having borne offspring by that other male?
5. Does the resemblance to that male become less and less in each animal successively produced, and does it ultimately become imperceptible?

Whether such experiments as are here suggested would, if instituted, establish as a principle in the physiology of generation, that, independently of mental agency, a male animal, that has once had fruitful intercourse with a female, may influence the products of subsequent conceptions of that female in which he himself is not concerned, it were needless at present to speculate. But the cases given in this and my former paper, as instances of it, taken along with the belief of the frequent occurrence of such cases, and the acknowledged importance of maintaining the purity of breed of most kinds of our domestic animals, may well engage the attention of the agricultural body, and lead them to investigate the subject. How much the Royal Agricultural Societies of England and of Ireland, and the Highland and Agricultural Society of Scotland, have it in their power to promote such an investigation; and how much the prosecution of it would be in keeping with the ends of their institution, it is unnecessary to say.

IV. The experiments suggested under the preceding head have regard mainly to the question, whether the phenomenon, of which an explanation is sought, may not be adequately accounted for on the principle of *mental* agency. It is conceivable that they may demonstrate that it is independent of any such agency, and referable, therefore, to a *material* agency connected in some way with the prior coitus of the female with the foreign male. And should they establish this, an important step in the inquiry as to its real cause would be gained. They cannot reasonably be expected, however, to demonstrate, at least directly or necessarily, that it is due to a *constitutional* change effected in the system of the mother by the fœtus begotten by that male. For, in fact, the cause of it may be, as is supposed by some, a strictly *local* change confined to the general mass of ova (or to certain of these), and effected simply by the *semen* of this male. Nay, as a professional friend has pointed out to me,

even supposing the change to be a constitutional one, it may not be produced by a fœtus in utero, but by the *semen* received by absorption into the female system, just as the small-pox virus, and other morbid poisons, are thus taken into the system, and produce in it a general and permanent change of some kind.

It is not my intention to enter into a discussion here on either of these two other explanations of the phenomenon. The first, indeed, was pretty fully considered in my former paper; and I have only farther to submit, in reference to it, that if it should appear that *impregnation* at the time of the former coitus is a necessary condition of the phenomenon, the theory of a local change, effected merely by the semen, will thereby be rendered an improbable one; because it is certain that, in the human species at least, the semen must often reach the ovaries without impregnation following (and this just because of no ova being ripe); and it seems difficult to comprehend how, if the change is of the kind supposed, and referable to some action of the semen on *immature* and then *unimpregnated* ova (and this theory necessarily implies as much), the phenomenon should occur only when that condition holds.

With regard to the other explanation, it may be conceded in support of it, and as showing that absorption of the liquor seminis is not an unlikely occurrence, that secondary syphilis may be imparted by a man to a woman through the medium of this fluid, because imparted independently of impregnation. But whether this virus is often conveyed in that way, or more generally only when impregnation follows intercourse, remains to be seen (see p. 303). In the meantime, accepting the suggestion, and registering it here along with the other theories as equally with them furnishing a possible explanation of the phenomenon, I would merely now observe regarding it, that, just as in regard to the theory of a local change, its antecedent value will turn mainly on the circumstance, whether the phenomenon with which it is sought to connect it is ever observed in cases where impregnation has not followed a former coitus. If such impregnation is an essential condition to its occurrence, the explanation in question may be set aside as worthless.

ABERDEEN, September 10, 1850.

ARTICLE III.—*Cases of Stricture of the Urethra treated by External Incision.* By JAMES DUNSMURE, M.D., Surgeon to the Royal Infirmary of Edinburgh.

THE management of stricture of the urethra has always been a most interesting subject to the practical surgeon, but more than usual attention has been paid to it in this country from the time Mr Syme published his new mode of treating it by external incision. Various papers and cases have appeared in the different medical journals,

condemnatory of Mr Syme's plan, which have been answered by Mr S. as far as it was necessary for him to do so.

During the last ten months several cases of stricture have come under my care in the Royal Infirmary, and having adopted the new treatment by external incision in three of them, I think it incumbent on me to make known the results.

The first case in which I had recourse to it was in the person of an Italian, Joseph Anthony, æt. 48, who was admitted into Ward No. 4, on the 23d January 1850. Upon seeing him the day subsequent to his admission,—he having come to the hospital after the hour of visit,—I found him suffering from infiltration of urine into the perineum and scrotum, both of which were much swollen, hard, and inflamed. There were several fistulous openings in the perineum, from which urine escaped in small quantity when he attempted to micturate. Incisions were made into the scrotum, and one of some extent and depth into the perineum, with the view of opening the dilated part of the urethra behind the stricture. Pus and a considerable quantity of urine escaped at the time. The urgent symptoms produced by the infiltration, and which are so well known that I need not describe them here, gradually subsided, and the parts regained what the patient considered their normal state,—such as they had been in for many years, viz., a thickened and hardened condition of the perineum, with various fistulous openings between the verge of the anus and the root of the scrotum. As the inflammatory swelling fell, I found that the hardness extended along the urethra for fully an inch anterior to the scrotum. The incision I had made upon his admission, and which was a little to the left side of the raphé, and in the direction of that for lithotomy, had greatly contracted, and assumed very much the appearance of the other fistulous apertures.

The man was well known to me, and to all those who had been in the habit of attending the hospital, as he had been a patient under Mr Liston, and also I believe under almost every surgeon who had done duty in the Infirmary since that period. He was addicted to drinking, and had been so for many years. As already mentioned, the more urgent symptoms gradually subsided; and, after various attempts, I succeeded with some difficulty in passing No. 2 catheter through the stricture, which extended from an inch anterior to the scrotum to the middle of the membranous portion of the urethra. Dilatation was continued until No. 7 catheter was introduced. After consultation with my colleagues, and thinking that if the original stricture was divided,—and which I hoped might be anterior to the bulb,—that the remaining hardness of the urethra would subside, I determined upon the external division of that part of the urethra anterior to the bulb, and accordingly, after having administered chloroform, introduced a staff with the groove on the back, and about the same size as No. 7. An incision was then made into the perineum so as to divide the urethra upon the staff from behind for-

wards, anterior to the bulb, and to the extent of about an inch and a quarter. No. 7 catheter was then passed and retained in the bladder by means of tapes. This was not withdrawn for two days. It was then removed, and No. 9 passed, which was kept in for a short period. In two days more No. 10 was introduced; and, after a similar period, No. 12. The thickening and hardness anterior to, and in the middle of the scrotum became considerably less; but there was not much difference to be observed in the perineum, nearly as much urine flowing through the opening made at the time of the operation as that passed by the urethra. No. 12 was introduced every fifth day. About six weeks after the division of the urethra, the patient insisted on being dismissed, which was accordingly done, although the perineal wound, which was not yet healed, still allowed urine to pass.

The difficulty experienced in this case was from the cartilaginous state of the perineum and urethra, and so hard did the textures feel, that it was not easy to distinguish the staff from the surrounding tissues. There was considerable bleeding, but well applied pressure by means of a T bandage was sufficient to arrest it. It did not come from any one vessel, but seemed to be a general oozing from the most superficial to the deepest part of the wound. I have no doubt there was somewhat of a hemorrhagic diathesis, as Dr Fyfe, my house surgeon, was obliged, from the continuance of the bleeding, to plug the wounds made in the scrotum on the day after the patient's admission.

In such a case as the one now narrated, it was hardly to be expected that Mr Syme's method would succeed, and although, from the reason mentioned, a partial trial was given to it in the manner above described, yet to have performed the operation that Mr Syme *recommends*, it would have been necessary to have divided the urethra for nearly the whole of its extent, which, of course, was out of the question. If the operation did no good, I consider it did no harm, for, although the perineal fistulæ still existed as on admission, yet, had the patient remained under treatment in the hospital, as he was urged to do, I have no doubt they would have contracted and healed under the continued use of the bougie. We can, therefore, in my opinion, draw no conclusion from this case, either for or against the operation by external incision.

The next case was that of William Tucker, æt. 44, who was admitted on the 12th March. He stated that he had laboured under stricture for seven years;—that he had applied for relief to various surgeons, and had been a patient for this disease in the Manchester Infirmary, and in two of the London hospitals;—that he was dismissed about nine months ago from one of the latter institutions after having been an inmate of it for nearly three months;—that the largest sized catheter that had been introduced was No. 7;—and that, since leaving the hospital, his symptoms had gradually become worse, and so distressing as to make him apply once more for relief.

Upon examining the perineum, a fistulous opening was discovered, which had existed previous to the time he entered the London hospital. Thin purulent matter flowed from this, unmixed with urine, no urinous odour being perceptible. Upon making water the stream was exceedingly small, almost, in fact, in drops. After one or two attempts I succeeded in passing No. 2 catheter. As this was evidently a resilient stricture, and accordingly a proper case for external division, I determined to have recourse to it. No. 2 was then passed a second time, and kept in the bladder for twenty-four hours. It was then taken out, and on the following day a small staff, with the groove on the back, and about the same size as No. 2, was passed into the bladder, the patient having previously been put under the influence of chloroform. An incision was then made into the perineum, and I had no difficulty in feeling the thickened part of the urethra, which seemed to be about half an inch in extent, and at the commencement of the membranous portion. Having felt for the healthy urethra behind the stricture, a small bistoury was pushed into it, and carried forwards, until the stricture was entirely cut through, which was known by the grating sensation being no longer felt as the parts were divided. The staff having been withdrawn, No. 8 catheter was passed with ease, and secured in the bladder. The patient was then sent to bed. No bleeding occurred. The catheter was allowed to remain in the bladder for three days (there being no irritation), when it was taken away, and Nos. 10 and 12 introduced; the latter was left in the urethra for about an hour, and then withdrawn. The following day urine was passed by the wound; but by the third, this had ceased, and from that time all the water flowed by the natural passage. In ten days from the date of the operation the wound in the perineum had healed. No. 12 was passed for the first fortnight every five days. This instrument seemed to be the full size of the urethra, judging from the degree of pressure which it was necessary to use to make it enter the meatus. I kept this man in the hospital for fully five weeks after the wound was healed, and for more than a fortnight before his dismissal no catheter was passed, in order to ascertain, as far as possible, in that short space of time, whether any contraction had recommenced. Accordingly, on the 3d May, the day he left the house, No. 12 was again inserted, and with the same ease as formerly.

Like a great many hospital patients, this man has been lost sight of, and of course I know not in what condition his urethra is at present. But this at all events is certain, that, from the date of the operation he might have been dismissed from the Infirmary in ten days, with the wound in the perineum healed—with the power of voiding his urine in a large stream—and with No. 12 catheter passing into his bladder with the utmost ease.

The third case is that of James Shortt, æt. 54, who was admitted on the 20th August. His stricture, he says, is of fourteen years'

duration, and he has been under treatment for it in Ireland, in Dundee, and in Edinburgh; in fact, he was a patient some twelve months ago in the hospital here under Dr Douglas Maclagan. When he was admitted under Dr Maclagan's care the stricture was very tight, and no instrument larger than No. 1 could be passed, and that was got through only after several trials. He was in the surgical wards for a considerable period, and was ultimately sent to the medical department of the house for an attack of acute rheumatism. By this time, however, the stricture was so far overcome as to admit No. 9, and he was told, upon leaving the medical wards, to go back to Dr Maclagan as an out-patient, and have instruments regularly passed. This he neglected to do, and his stricture gradually relapsed to its former bad condition. For a fortnight after his admission no progress was made in getting through the stricture, from his having requested and obtained permission to leave the hospital on several occasions, to do duty with the local pensioners—he being an old soldier—which corps happened to be on service at the time, so that he was not a regular patient until the beginning of September. On the 5th I passed No. 2, and tied it in the bladder, but in the course of an hour or so he withdrew it; on the 7th No. 3 was passed, and retained until the 8th; and on the 9th, after chloroform was given, Mr Syme's operation was begun in the same manner as in Tucker's case, but I found the stricture so far back in the membranous portion as to prevent me cutting from behind forwards. Accordingly, having pushed the knife into the staff anterior to the stricture, I inserted the forefinger of the left hand into the rectum, and steadied the staff by pressing upon it from the bowel upwards, while with a gentle sawing motion I divided the stricture from before backwards. The staff was then removed, and No. 7 catheter passed into the bladder, but not, I must confess, without some difficulty, from the point of the instrument getting out of the urethra and catching upon the posterior part of the incision. No hemorrhage followed the operation. For the first twenty-four hours the patient suffered considerably from pain in the urethra, caused by the presence of the catheter,—this, however, was so much relieved by camphor mixture and morphia, that next day he made very little complaint. No. 7 was then removed, and Nos. 9 and 10 introduced. On the 16th No. 12 was put into the bladder; and again on the 21st and 24th, by which time the wound in the perineum was healed—no urine having flowed from it since the 14th, and at this date, the 26th, he is quite well and able to leave the hospital.

The above is the result of the three cases in which I have adopted Mr Syme's operation; and although much more difficulty was experienced in Shortt's case than in either of the other two, yet so convinced am I of the advantage gained by it in diminishing the length of time necessary for the cure, that I intend to adopt it in those cases of bad stricture which may happen to come under my care.

ARTICLE IV.—*Notice of Attempts recently made to Improve the Condition of the Fatuous by Education, Physical and Moral.* By JOHN COLDSTREAM, M.D., Fellow of the Royal College of Physicians of Edinburgh.

UNTIL of late years, *all* children affected with congenital idiocy were regarded as incurable. It appears, however, from experiments recently made in France, Switzerland, America, and England, that certain cases are susceptible of great improvement under appropriate treatment. It is reported as the result of some of these experiments, that about one-third of the children subjected to treatment have been brought, in the course of a few years, to so greatly improved a condition of mind and body, as to be capable of conducting themselves with propriety, and taking part in the ordinary business of life. Such a result must be regarded as in the highest degree satisfactory; and is certainly sufficient to encourage physicians to persevere in endeavours to carry out the system thoroughly, so as to give *all* those affected with the dire and dreaded malady a chance of attaining to some degree of capacity for the enjoyment of life, and the exercise of the functions of humanity. And, even if future inquiries and experiments should show that, in general, not so many as a third are susceptible of such a measure of improvement, it evidently follows from what has already been accomplished, that those who have the charge of idiot-children must henceforth be regarded as bound to procure for them the means of mental and physical training, adapted to their respective cases.

Three years ago, these facts and views led to the establishment of an asylum for idiots at Highgate, near London, which quickly obtained great popularity. That such an institution was needed, was proved by the numbers of applicants for admission. Last year there were 170 candidates for 15 vacancies; and it was forthwith resolved to erect large additional accommodation. And even when this shall have been provided, the probability is, that more will soon be demanded. In their last report, the directors state that the actual change in the condition, physical and moral, of the objects of their care, in the estimation of those who had the means of making the comparison, is exceedingly striking. It has been so great in the course of six months, that one could hardly believe the persons to be the same. From having been most disorderly and unmanageable at their first gathering, the pupils have become quiet and docile. "There is now order, obedience to authority, classification, improvement, and cheerful occupation. Every hour has its duties; and these duties are steadily fulfilled. They are, essentially, not only an improving, but a happy family. And all this is secured without the aid of correction or coercion. The principle which rules in the house is love."

The institution at Highgate, however, was not the first of the kind established in England. That at Bath was commenced in 1846. Its last report contains some encouraging statements. There are nineteen inmates, in whose character and conduct three years' treatment and discipline have sufficed to produce much improvement; while the health of many has altered for the better. Only two of the children admitted have been dismissed as incapable of improvement. "The pupils are instructed, as their capabilities permit, in reading, writing, arithmetic, and geography; and their acquaintance with external objects is extended and improved. Religious knowledge and moral culture are, directly or indirectly, kept in view in every pursuit. Control of temper, obedience, order, and kindness to each other, are strictly inculcated; and sufficient progress has been made in these respects to justify much hope for their future conduct. The children show considerable affection for their teachers, and are, in general, happy and contented. Corporal punishment is not permitted in the establishment; but strict obedience is enforced by means of moral influence." With regard to some patients, whose cases were originally unpromising, it is confidently hoped that, by remaining a few years longer under training at the institution, they will hereafter prove helps instead of burdens to their friends. The general conclusion drawn from the experience of this establishment, as well as of that at Highgate, is, that "much may be done for the improvement of the idiot; *most* of the cases treated having been benefited."

So long ago as 1828, M. Ferrus, chief physician at the Asylum of the Bicêtre, Paris, organised a school for his idiotic patients, and caused them to be taught habits of order and industry, and to be instructed in reading, writing, arithmetic, and gymnastics. The success met with led Messrs Voison, Falret, and Leuret to extend the system of instruction; and latterly, at the Bicêtre, much attention has been devoted to the grand experiment of subjecting the fatuous to all those means and agencies that are likely to rouse and to invigorate their dormant faculties. After witnessing the improvement effected in the condition of some of the objects of treatment in these schools of the Bicêtre, in 1844, Dr Conolly remarked:—"It is difficult to avoid falling into the language of enthusiasm on beholding such an apparent miracle; but the means of its performance are simple,—suitable space and local arrangements adapted to the conservation of the health and safety of the pupils, and to the establishment of cleanly habits—to presenting them with objects for the exercise of their faculties of sense, motion, and intellect—and to the promotion of good feelings, and a cheerful active disposition." [1]

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[1] British and Foreign Medical Review, XIX., p. 295. Schools for the insane have been successfully established in the Lancaster County Asylum, also in those of Surrey and Hanwell.

Even cretinism (which must be regarded as idiocy in its most intense degree) has been successfully subjected to treatment within the last ten years. At the well-known retreat on the Abendberg, in Switzerland, the benevolent Dr Guggenbühl has laboured most energetically in the attempt to improve the health, physical and mental, of cretinous children. Here it was that the important principle of beginning the treatment at a very early age was, for the first time, fully acted upon.[1] To this, as well as to the peculiarly favourable locality of the establishment, must be ascribed much of the success which has been accorded to Dr Guggenbühl's exertions. That success, taken along with the boldness of the conception, which contemplated nothing less than the cure of a malady hitherto deemed incurable, and that in an hospital situated at an elevation of 4000 feet, on the slope of the Alps, has doubtless had a powerful effect in directing the attention of the philanthropists of Europe and America to the feasibleness of attempting to ameliorate the condition of the fatuous; and it must be acknowledged that civilised humanity owes a large debt of gratitude to the man who, almost single-handed, and in spite of difficulties of no ordinary magnitude, has done so much to *prove* that "there is no condition of life utterly hopeless; and that wherever there is mind, there is no imprisonment from which it may not be freed."[2]

Dr Guggenbühl's example has been followed in the kingdoms of Wurtemberg and Sardinia; the governments of which have had statistical inquiries made regarding the extent to which cretinism prevails, and have provided means for the treatment of the young subjects of it. The results of the researches made at the instance of the Sardinian government were published last year, in an admirable report, drawn up by Dr Trombotto of Turin, with the aid of a large commission, composed of professors of medicine, chemistry, geology, mineralogy, and zoology. The investigations upon which the report is based appear to have been extended to every part of the Italian portion of the kingdom, and to have included every point that is concerned in the production and modification of cretinism. A very complete natural history of the disease has thus been produced. The general conclusion regarding the causes is, that the most constant source is connected with excessive humidity, or vitiation of the air; but that, for the development of the disease, the concurrence of several causes is requisite. The commission states confidently that, in certain places, there appears to have been an

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[1] It ought not, however, to be forgotten, that, in his excellent thesis, "De Fatuitate Alpina," Dr Abercrombie distinctly suggested the desirableness of making a trial of remedial means, for the cure of that bodily ailment upon which idiocy depends, in infancy; thus he wrote,—"Ubi, tenerâ ætate, proclivitas in fatuitatem observatur, estne, quamobrèm expectemus, tempestivo roborantium medicamentorum et frigidarii usu posse obvenienti mentis imbecillitati occurri?"

[2] Kitto—"Lost Senses," II., p. 46.

abatement of the disease consequent upon the adopting of various sanitary measures.[1]

A most satisfactory commencement of a work of beneficence on behalf of the fatuous has also been made in the state of Massachusetts. It originated in 1845, with a few gentlemen at Boston, who, seeing and feeling for the sad condition of the neglected idiots, and having no very precise knowledge of what had been done for such persons elsewhere, petitioned the legislature on their behalf, and succeeded in getting commissioners appointed "to inquire into the condition of the idiots of the commonwealth,—to ascertain their number, and whether anything can be done for them." The report of these commissioners was published in 1847–48, and a most interesting and valuable document it is. The statistics which it contains were drawn from inquiries made in seventy-seven towns of the state, of which sixty-three were thoroughly examined. These contained an aggregate population of 185,942, among which were found 361 idiots. But, from other sources, the commissioners became acquainted with the particulars of other cases, so that their report is based upon the examination of 420 true idiots, "whose feebleness of intellect was connate." Of these, 187 were under twenty-five years of age, and 174 seemed capable of improvement; only 19 were found able to earn their board and clothing under discreet management; "141 do earn their board when closely superintended; 110 can do trifling work; 73 are as helpless as children of two years old; and 33 are utterly helpless as infants." The report supplies also some valuable statistical information regarding the apparent causes of idiocy, particularly those of intemperance and intermarriage between relations. Immediately upon the reception of this report, the legislature voted the sum of 2500 dollars annually, for the term of three years, for the purpose of training and teaching ten idiotic children. For the efficient application of this grant of the public money, arrangements were made with the trustees of the institution for the blind;[2] and

[1] Rapport de la Commission crée par S. M. le Roi de Sardaigne, pour etudier le Crétinism. Turin. 1849. 4to. In connection with the above statement of public measures, having in view the enlargement of our acquaintance with the statistics of cretinism, I would refer to a beautiful map of the Canton of Aargau, published in 1843, by M. E. H. Michaelis, civil engineer, on which are very distinctly laid down the numbers of cretins existing at the various localities in which the disease is prevalent, throughout the canton, with the elevation and other physical circumstances of these several localities. The map bears the title of "Skizze von der Verbreitung des Cretinismus im Canton Aargau."

[2] Much of the experience gained by those who are occupied in teaching the blind, is applicable to the training of the imbecile. Hence, there was great propriety in placing the children, who were to form the first subjects of this experiment, in an institution for the education of the blind; and, especially, in this particular institution, where, in the remarkable case of Laura Bridgman, so much had been done to cultivate mind, in spite of co-existing deafness and blindness. It is very satisfactory to see how the success met with by Dr Howe, in the attempt to educate that poor girl, has encouraged others elsewhere to

now, for two years, the work has been in process. The views entertained by the conductors of the experiment are thus stated:—"It is proposed, not only to train and develop, as much as possible, the feeble intellectual powers of the idiot, but also to call out and strengthen, as much as may be, the dormant or feeble capacities of every part of his nature. It is hoped to train him up to cleanliness and decency,—to prevent or root out vicious and debasing habits,—to moderate gluttonous appetite,—and to lessen the strength of the animal nature generally, by calling into some activity the higher feelings and desires, and by substituting constant occupation for idleness. It is proposed to train all the senses and perceptive faculties by constant and varied exercise,—to strengthen the power of attention,—to teach, as much as possible, the rudiments of knowledge,—to develop the muscular system,—and to give some degree of dexterity in simple handicraft. Efforts will be made to call out their social affections, and to lessen their inordinate selfishness. The still harder task will be attempted of appealing to the moral sense, and drawing out what little capacity there may exist for comprehending right, for exercising conscience, and for developing the religious sentiment. It is hoped that part of the pupils will gain some really useful knowledge, that most of them will become cleanly, decent, temperate, and industrious, and that all of them will be better and happier from the efforts made in their behalf."

At the time when the report, from which the above sentences are quoted, was published by Dr Howe, the pupils in the school had been only one year under training, but the result of the experiment was already "most encouraging." *All* the pupil-patients had improved in personal appearance and habits, in general health, in vigour and activity of body. Some of them could control their own appetites in a considerable degree; they sat at table with their teachers, and fed themselves decently. Almost all of them had improved in understanding, and in the use of speech. Some of them had made considerable progress in the knowledge of language; they could select words printed on slips of paper, and a few could read simple sentences. They had gained a knowledge of many objects, their names, colours, forms, dimensions, &c. But what was most important, they had made a start forward. They had begun to give their attention to things; to observe qualities and to exercise thought. "And

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devote attention to similar cases. I have much pleasure in referring here to two pupils, now in the Abbey Hill School for the Blind, in this city, who, in spite of blindness, and such a degree of deafness as makes a trumpet necessary in addressing them through the ear, have been successfully educated in all the ordinary branches. It is indeed most gratifying to see these boys communicating their ideas to one another by touch. Much patience and perseverance were required on the part of their able teacher, Mr Haig, before he succeeded in eliciting intellectual activity in these children; and, doubtless, the same sort of exertion and skill is demanded from those who apply themselves to the training of the fatuous.

thus," the intelligent reporter concludes, "it has been demonstrated that idiots are *capable of improvement*, and that they can be raised from a state of low degradation to a higher condition. How far they can be elevated, and to what extent they may be educated, can only be shown by the experience of the future."[1]

To do justice to the system of training which has been instrumental in accomplishing so much for the poor idiot, it is obviously of the greatest importance that efficient *teachers*, as well as able physicians, be employed. Such, doubtless, it will not be always easy to find. The successful teacher of fatuous children must be possessed of both intellectual and moral qualities but seldom met with in the same person. He must have a sincere and ardent love towards his charge, zealous desire to see them improve, and undoubting assurance of success; besides exhaustless patience, earnestness combined with gentleness in manner, and fertility in invention. In the working of the training school, all will depend upon the ability of teachers. "When the time comes," says Dr Howe, "that schools for idiots are established over the country, it will be found more difficult to get good teachers for them, than to get good professors for our colleges."

It is satisfactory to learn, that already individual enterprise has been called into action in connection with this object. At the village of Bendorf, not far from Coblentz, on the Rhine, Dr Erlenmeyer has recently opened a private school for idiot children, which promises to be a great blessing to the neighbourhood, where there is a marked prevalence of infantile fatuity. (This is particularly the case on an island in the Rhine near Bendorf.) A similar institution is mentioned in Dr Howe's Report, as having been commenced at Barre, in the state of Massachusetts.

We earnestly hope that no long time will be allowed to elapse ere examples so good be followed in this country. It is disgraceful to Scotland, that, with so much educational talent on the one hand, and so much medical zeal and skill on the other, as unquestionably exist amongst us, we should be so far behind other nations in the application of science in the forwarding of an experiment so noble as this is.

It was, indeed, satisfactory to see the subject taken up by the British Association, at its late meeting in Edinburgh, when it was agreed to memorialise her Majesty's government in favour of a national survey, to determine the extent to which congenital idiocy exists throughout the kingdom at large, and the causes of its prevalence in particular districts; but, while we fully recognise the importance of such a survey, we cannot admit that it is expedient to await the result of it, before making any effort to give some of our

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[1] Second Report to the Senate of Massachusetts on Training and Teaching Idiots. By S. G. Howe, M.D. 1850.

poor imbeciles the benefit of the knowledge we have already attained to, of the possibility of doing something for them. Let the national inquiry by all means proceed; but, at the same time, let the enlightened friends of humanity be up and doing, each in his own community. It requires no national survey to tell us that there are, at this moment, hundreds of fatuous children in all our large cities, and hundreds more throughout the country, for whom nothing is done, —whose parents are actually ignorant of the possibility of rescuing them from the state of useless inanity and brutish degradation into which they are permitting them to sink; but who are, nevertheless, susceptible, to a great extent, of physical, intellectual, and moral improvement, such as may render them decent and useful members of society. Let us then hope that, without delay, a movement may be made in favour of establishing a few model-schools for the training of fatuous children, not only in or near Edinburgh, but in the west, middle, and northern districts of Scotland.

ARTICLE V.—*Case of Popliteal Aneurism treated by Ligature of the Femoral.* By JAMES DRUMMOND, Surgeon, Dundee.

(*Communicated through Professor Syme.*)

JOHN FENWICK, æt. 33, seaman, employed in the coasting trade of this port, consulted me, on the 13th of July last, in consequence of a pulsating tumour in the right popliteal region, which had first attracted his attention when at sea, sometime in the beginning of May last. He states that his right knee-joint has been weak, and often the seat of pain, ever since he received a severe injury about twelve years ago, when he was thrown from the top of a stage-coach. His attention was first directed to the pulsating swelling, in consequence of increased pain and stiffness in this joint, although at that time the tumour in itself gave him little or no uneasiness, and was looked upon as a matter of small moment. It had of late, however, begun to increase considerably in size; and he now, for the first time, felt severe sharp shooting pains along the shaft of the tibia.

When I first saw him (July 13) the tumour occupied about two-thirds of the popliteal space, powerfully pulsating, visible to the eye, and conveying to the hand, both directly and laterally, the peculiar thrill so characteristic of aneurism. Pressure on the femoral immediately commanded the pulsation in the tumour, and by slight manipulation the sac could be entirely emptied of its contents.

The nature of the disease was explained to the patient, and an operation recommended. This he, however, declined, in the meantime at least, in consequence of being under engagement for a voyage to Liverpool, and he accordingly sailed next day.

I again saw him on his return (August 17). The tumour was still increasing, and now the seat of severe pain, and he expressed himself anxious for relief.

His general health was excellent. I had him confined to bed, and ordered low diet, with a gentle laxative, to prepare him for the operation, which was performed on the 23d of August, under the influence of chloroform, in the presence of Dr Monro, Mr M. Nimmo, and Mr Langlands. The sheath was exposed immediately above the crossing of the sartorius, and carefully opened on its fibular aspect to the smallest possible extent. The coat of the vessel being now seen, the needle was carried round it from within outwards, and the ligature, which was found entirely to command the pulsation in the tumour, was tied by the common reef knot, and one of the ends removed. Scarcely a drop of blood escaped. The wound was brought together by two stitches, and treated by the cold-water dressing. At the end of forty-eight hours, it was found to be united throughout its whole extent, except immediately around the ligature. The stitches were removed, and its edges kept together by plaster.

The temperature of the limb was for the first two days slightly lower than that of its fellow, but by the third no difference could be felt. The pulsation of the tumour never returned, and its contents are being rapidly absorbed. The ligature separated on the 23d of September, being the thirty-first day; and he is now about to resume his usual occupation.

[In a note accompanying this case, Mr Drummond states, that during the last twelve years there have occurred in Dundee four cases of popliteal aneurism,—viz., the one here related; two subjected to operation in the Dundee Infirmary, by Mr Crichton and Mr Nimmo; and one in which the artery was tied by Mr Syme;—all with success.]

ARTICLE VI.—*On Permanent Retention of the Placenta from Morbid Adhesions, with Remarks.* By ROBERT T. CORBETT, M.D.

(*Read before the Medico-Chirurgical Society of Glasgow, 13th August* 1850.)

MRS S., æt. 30, seven months in her sixth pregnancy, and of debilitated habit, became my patient on the 16th May 1850, when she complained of uneasiness in the belly, of four days' duration, attributed by her to a fright. She said the membranes had ruptured, and that a large discharge of water had taken place. On examination, the os uteri admitted only the point of the finger. I could not tell whether the waters had escaped or not. Bowels regular, from the use of medicine. An opiate was ordered.

17th.—Much the same as yesterday; os uteri as on former examination. Bowels not moved to-day. Opiate to be repeated.

18th.—Os uteri dilated to the size of a half-crown piece, and pains decidedly uterine; considerable bloody discharges. As she was a weakly woman—the child

undoubtedly small, and supposed to be dead, from motion having discontinued to be felt by the mother, and from the sound of the fœtal heart and placental soufflet having ceased to be heard, to terminate the labour, I gave four half-dram doses of the ergot of rye in infusion, every twenty minutes, without effect. Habeat enema domesticum quam primum.

19th.—Bowels opened from enema; had slept very well; os uteri a little more dilated; still considerable oozing. Pains those of labour, and stronger than yesterday. I administered the ergot in the same doses as the day before, but the drug was of a fresher quality. The pains were very much increased in power and frequency by its exhibition; some were even considerably protrusive, but nature had not the power, and the effects of the drug wore off. I then administered an opiate, to quiet the system and procure sleep.

20th.—Patient had had a few hours' refreshing sleep during the night, and this morning, at half-past nine o'clock, the fœtus was expelled, with little suffering, and slight labour pains. I was sent for. On my arrival I found the fœtus expelled, and in a putrid state. The placenta was still in the uterus, which was flaccid. The cord came away with the very slightest traction. After dashing whisky over the abdomen, grasping the uterus tightly, and trying the usual means to make it contract, and persevering for half an honr, as there appeared to be very little if any contraction, the left hand was completely introduced into its cavity, when the membranes were found to adhere all around, so that I could not positively say that I felt the border of the placenta, although I thought so, from my fingers passing over a rounded ridge. The usual plan of separating it, by insinuating the fingers between it and the uterine surface, and gently moving the hand from side to side, with a sawing motion, being out of the question, the plan of Hamilton and Burns was tried, without effect. The uterus contracted powerfully upon the hand, but became quite flaccid as soon as it was withdrawn. My manipulations were continued for about forty minutes. A bandage, with a compress, was now tightly applied, a grain of opium given, and the patient left to rest. The patient progressed as well as is usual in an ordinary convalescence after delivery, till the evening of the 25th, when, after the loss of not more than 12 or 14 oz. of blood by hemorrhage from the uterus, she died, notwithstanding the free exhibition of stimuli; in short, she never rallied from the collapsed state, which immediately followed the discharge, and died in four hours. As I became aware of the intimate adhesion between the placenta and the uterus at my first attempt at extraction, and as in such cases I do not approve of any after attempts at removal, no farther trial to separate it was made.

Inspection 60 *Hours after Death.*—Three medical gentlemen besides myself were present. Upon opening the cavity of the abdomen, the uterus was seen $6\frac{1}{2}$ inches long by $4\frac{1}{2}$ broad, having a baggy empty feel, but presenting a healthy surface. The uterus was detached from its position, and an incision made longitudinally through its anterior wall, when the placental mass and membranes were seen to fill its cavity. The inner surface of the uterus presented a dirty green or blackish hue, and after having been repeatedly and carefully washed, was found to be wholly covered by a lining presenting a granular fleshy appearance, at least $\frac{1}{8}$ of an inch in thickness, in some places even more. This membrane was, with some difficulty, separated, to a trifling extent, from what appeared to be the disorganised lining of the uterus. The placenta adhered to the superior and anterior part of the uterus, rather to the left side, which was the spot where I had felt it. The adhesion was complete, and so perfect that the placenta could not be detached without destroying its own substance, or that of the uterus. We could not discover the spot from which hemorrhage had taken place. The structure of the placenta resembled that of a hepatized lung, but was more fibrous, and was so intimately connected with the parietes of the uterus, as to render it impossible to tell where the placenta terminated, and the uterus commenced. The fœtal membranes were also very extensively and intimately attached to the walls of the uterus; indeed so intimately, that the attachments could be broken only with great force, and would of themselves have been a cause of retention of the placental mass.

Drs Davis and Ramsbotham believe that this morbid adhesion is produced by the formation of a new membrane, the consequence of inflammatory action existing in the uterus. Dr Ramsbotham gives several reasons for this belief.

1st, That this morbid adhesion is more frequently met with amongst the poor, who are more exposed than others to accidents during pregnancy, such as falls or blows.

2d, Accidental hemorrhages towards the close of pregnancies are occasionally followed by morbid adhesion.

3d, Disease of the placenta produces morbid adhesion.

Now, I have not the least doubt that these incidental circumstances frequently produce morbid adhesion in the manner specified by these celebrated obstetricians; but a remark which is made by most practical men, "that some women seem to have a constitutional predisposition to an adhesion of the placenta," I would bring more prominently before you, and rank as another cause, perhaps a more frequent one than is generally supposed. This constitutional predisposition I would define to be—that influence in the system, which predisposes to action in the uterus, increased beyond its natural condition, without that action being necessarily or always of an inflammatory character.

Now, we know it to be fact, that there exists a condition of the blood in all pregnant women which is characterised by the abundance of fibrine and albumen which it contains; as also, that the majority of people have some organs in their bodies which are weaker or more liable to be affected with disease than other organs, or, in other words, have a predisposition to particular diseases. Is it too much to deduce, then, when morbid adhesion of the placenta is frequently met with in the same patient, and when the various incidental circumstances, which I have enumerated as frequently causing inflammation of the lining membrane of the uterus, have never happened, that the uterus is the weak organ, or the one most liable to be affected with disease in that patient; or, in other words, that the patient has got a predisposition to morbid adhesion of the placenta?

It is rather matter of surprise, I would say, that this affection is so rarely to be met with; for, besides the peculiar condition of the blood, the uterus being the organ of the body in pregnant women to which the greatest supply of blood is sent, one would expect, even prima facie, to meet with diseased structure oftener than it occurs.

It becomes a point of great consequence, then, to ascertain if anything can be done during pregnancy to prevent this cause of danger. I would say, that in every case in which there has been retention of the placenta from morbid adhesion in a previous confinement, the practice usually recommended by authors should be tried, although, in the event of the woman having a constitutional predisposition to this affection, we would be very likely to meet with this dreadful disease, even after the use of means.

In my patient there existed the strongest predisposition to this disease. This, as I have mentioned, was her sixth confinement. Mr George Miller, surgeon, George Street, attended her in the three first; and, as he has kindly favoured me with notes of the cases, I shall give them in his own words :—

"*Notes of Mrs S.'s Third Confinement. October* 1845.

"Labour very tedious; no obvious cause; proposed using forceps; not agreed to by patient and friends. At length delivery was effected. Nothing unusual about child; placenta adhered; used traction by the cord without effect. Introduced hand, and with much trouble got the placenta detached by separating the adhering surfaces by the points of the fingers. Some hemorrhage followed, but was arrested with little difficulty. Patient had a good recovery.

"Nothing unusual about previous labours."

I have authority from Mr Miller to mention, that the extraction of the placenta in this case was more difficult than in any other that ever occurred in his practice. In the fourth accouchement, which took place at the seventh month, she was attended by Mr Parker, surgeon, Tradeston, who experienced great difficulty in removing the placenta, having only succeeded in bringing it away in pieces or shreds, after his manipulations had been continued for an hour. The patient made a good recovery.

In the fifth delivery, which took place at full time, she was attended by Dr Cunningham of Gorbals, who likewise succeeded in bringing the placenta away, but with the greatest difficulty.

In this case, then, I look upon the constitutional tendency as the cause of the morbid adhesion; for although my patient belonged to the lower ranks of life, still none of the other causes I have named, so far as I could learn from the most minute inquiries, ever existed. No inflammatory action was present, at least none of the symptoms of inflammation before the birth of the child, and none took place after it.

The patient complained of no local pain, and in her there existed none of the symptoms which usually follow when the placenta or a portion of it is left in the uterus; and I entertain no doubt whatever that, had the patient lived, the placenta would have continued to retain its attachment to the uterus. Whether that attachment would have continued through life, or whether the placenta would ultimately have been absorbed, can only be matter of conjecture; for as cases are recorded in which the placenta never passed from the uterus, and the patients recovered, we find medical men supporting both views.

That the uterus possesses absorbing powers, however, seems to be generally admitted. Supposing this to be the case, then, I remark, however diseased the general condition of the lining membrane of the uterus may be, as the preparation of this woman's uterus shows, there might still be a possibility of an impregnated ovum finding its way into the uterus; for the new membrane which covers the

whole interior of the uterus in this specimen, although tough and fibrous in its general character, is very thin, soft, and vascular, around the entrances of the Fallopian tubes, and would have been easily absorbed. Indeed, it was so fragile over the entrances of the Fallopian tubes, that, without any intention on my part, or on that of the other medical gentlemen who examined the preparation, its texture has been destroyed by the finger during examination, for an inch in extent, on both sides. Whether the ovum, after reaching the uterus, is to remain or be expelled, I should think would depend on the condition of its lining membrane. I think, in any case in which menstruation occurs afterwards, gestation may commence.

In conclusion, I remark,—

1st, That this case proves the truth of the generally received opinion, that all efforts to extract the placenta may be unavailing.

2d, That it strengthens the opinion "that constitutional predisposition is a more frequent cause of morbid adhesion than any other."

3d, That it proves that morbid adhesion of the placenta is produced by the formation of a new membrane, the result of increased action in the lining membrane of the uterus, and by the peculiar character of the blood in pregnancy.

4th, That it proves that agglutination of the fœtal membranes to the uterus, of itself may be a cause, if not of permanent retention of the placenta, at least of retention of it, until putrifaction shall so have destroyed their texture, as to permit of the mass falling away; although, from the nature of the disease, such a case must very seldom occur.

I may remark, that I did not learn this woman's unfortunate predisposition to retention of the placenta till after her death, when I began to investigate the history of her previous confinements. She had purposely kept all secret from me, lest the knowledge of it should influence my attendance upon her in her accouchement.

ARTICLE VII.—*Some Notes on the Analogy between Puerperal Fever and Surgical Fever.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.

Nearly 3000 mothers die in childbed every year in England and Wales.[1] Among these 3000 deaths, a comparatively small propor-

[1] According to the Reports of the Registrar-General, the mortality from childbirth in England and Wales was 2811 in 1838, 2915 in 1839, 2989 in 1840, 3007 in 1841, and 2687 in 1842. During these five years 14,409 English women died in childbirth; or about one every third hour. Perhaps the returns under this head show also less than the reality, deaths occurring during the puerperal month being too often referred to other causes than childbirth.

tion only are the direct result of convulsions, hemorrhage, rupture of the uterus, and the other more immediate or primary complications and accidents connected with parturition. The great majority of these maternal deaths is produced by puerperal fever. Dr Ferguson [1] believes that as many as seven-eighths of the total mortality in childbirth are owing to puerperal fever and its modifications. This estimate of its effects is probably too high; but the observations of Farr,[2] Kiwisch,[3] and others, have amply shown that puerperal fever, and the visceral inflammations and deposits which are included under that name, are undoubtedly by far the most common causes of death in childbirth. Among 2890 women delivered in the old Edinburgh Lying-in-Hospital, from 1823 to 1837, 47 maternal deaths occurred. Of these 47 deaths, 36 were the consequence of puerperal fever; and the remaining 11 were produced by other primary obstetric complications and causes.

Among the individuals who are obliged to become the subjects of serious surgical operations, a considerable number die, however dexterously and well the operations may be performed. When the operation is severe, the proportion that thus die in consequence of it is much larger than is generally believed. For instance, among every 100 cases of amputation of the limbs—including amputation of the thigh, leg, arm, and forearm—about 30 terminate in death, and 70 in recovery. Dr Fenwick, some time ago, published the results of 4937 cases of these amputations, collated from the practice of some of our best civil and military surgeons. Among these 4937 patients, 1565 died after the operations, or nearly 1 in every 3.[4] The numerous deaths which thus follow operations in surgical cases, are, in a small proportion of instances only, the more immediate and direct results of primary complications or accidents connected with the operation itself,—such as hemorrhage, syncope, collapse, &c. In a large proportion of instances, the death of the surgical patient is the result, on the contrary, of a combined febrile and inflammatory morbid state, which I believe to be generically, if not specifically, the same as puerperal fever in the childbed mother; and in accordance with the common principles of nosological nomenclature, this disease may as correctly be denominated "surgical fever," as the other is denominated "puerperal fever." [5] A few years ago, Dr

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[1] Essays on the Most Important Diseases of Women, p. 1.

[2] Fifth Report of the Registrar-General. P. 380.

[3] British and Foreign Review. Vol. XIII., p. 111.

[4] See his excellent papers in the Monthly Journal for 1847, p. 238, &c.; and the corresponding evidence of the fatality of amputations and other operations, adduced by Malgaigne, Phillips, Lawrie, Inman, &c.

[5] Puerperal fever has been often denominated, in accordance with the special notions which the writer happened to entertain of its pathological nature. Hence we have it spoken of in some works as puerperal peritonitis, or metritis, or phlebitis, peritoneal fever, &c. &c. Any name thus

Chevers published the apparent cause of death in 153 patients, whose bodies had been examined at Guy's Hospital,[1] London, and who had died within that institution in consequence of surgical operations or injuries. Among these 153 surgical cases, in 1 the precise cause of death could not be discovered, and 18 of them had sunk under hemorrhage, tetanus, sloughing suppuration, gangrene, erysipelas, diarrhœa, and the total deficiency of reparative action in the wound. Excepting, then, these 19 cases, Dr Chevers found that in all of the remaining 134 cases, the post-mortem examination betrayed, as the more immediate cause of death, the existence of acute inflammation of one or more internal organs or structures;[2] and, no doubt, in all of them the usual symptoms of surgical fever were more or less perfectly marked during the last days of life. In most of these 134 cases the recent internal inflammatory lesions discovered on the bodies of the patients were, as we shall see in the sequel, not confined to a single organ or structure, but several were frequently found affected at the same time in the same patient.

Medical literature does not yet possess a sufficient series of data to enable us to institute a full comparison between all the elements of puerperal and of surgical fever.[3] But the consideration of a few

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drawn from pathology must ever change with the changes and advances of pathology itself; while a nosological name—such as puerperal fever—never requires to be varied, and is always fixed and intelligible. For example, all physicians know and recognise dysentery under that, its nosological, name; but if we had had it described and denominated in different ages and works according to the ideas entertained of its nature, an inextricable amount of confusion would long ere this have resulted; and so with regard to most other diseases. Some surgical pathologists have described the consecutive fever and internal inflammations under which surgical patients sink, under the names of pyæmia, phlebitis, &c.,—each in accordance with his own pathological notions. The term "surgical" or "chirurgical fever" would enable us to avoid all the difficulties and perplexities connected with such a pathological nomenclature; it implies no pathological theory; and it is sufficiently precise and distinctive as a simple nosological designation.

[1] Guy's Hospital Reports, for 1843, p. 89.

[2] It is proper to state, that (as Dr Chevers remarks) in about 13 out of the above 134 cases, the nature of the injuries was such that the patients had evidently from that cause alone no fair chance of recovery; but in the whole of the other 121 cases it appeared that there was nothing to render the patient's restoration impossible, had not severe inflammation, or some other unfavourable change supervened.

[3] Perhaps I may be pardoned from remarking, in vindication of obstetrical literature, that it possesses many separate essays and volumes on puerperal fever; and every text-book on midwifery contains a full chapter on the subject. But the reverse holds too true with regard to the literature of surgery. "Works," says Dr Fenwick, "upon operative surgery are daily written, which detail with the greatest accuracy the formation of flaps, and lay down to a hair's-breadth the extent of incisions, without mentioning the secondary affections liable to occur after the operation. Huge dictionaries issue from the press, discussing the rival merits of ancient authorities to some trifling improvement in the method of amputating, but forgetting to lay down rules for

points may prove enough to indicate at least a strong analogy, if not an identity between these two forms of disease. With this view I shall in the following notes attempt very briefly to show in what respects puerperal and surgical fevers are assimilated to each other: 1. In the anatomical conditions and constitutional peculiarities of those who are the subjects of them; 2. In the pathological nature of the attendant fever; 3. In the morbid lesions respectively left by either disease; and 4, In the symptoms which accompany each affection.

1. *Analogy in the Anatomical, &c., Conditions of the Subjects of Puerperal and Surgical Fever.*—The anatomical conditions of the puerperal patient after delivery, and of the surgical patient after an operation, are in many respects the same. In the surgical patient, we have a wound or solution of continuity on the external part of the body, made by the knife of the surgeon; this wound has, opening upon its free surface, the mouths of numerous arteries and veins; and it comes to be repaired either by the direct adhesion of its opposed surfaces, or more slowly by exudation of lymph and pus from its surface, and the ultimate formation of a new skin or new enveloping or connecting tissue. In the puerperal patient, we have a wound or solution of continuity on the whole internal surface of the womb made by the separation of the placenta, and the exfoliation of the decidua or mucous membrane of the uterus; this wound has, opening upon its free surface, and specially at the former site of the placenta, the mouths of numerous arteries and veins; and it comes to be repaired under the usual accompanying exudation of serum, lymph, and pus (lochial discharge) from its surface, and by the ultimate formation of a new layer or coating of mucous membrane. The obstetrical patient has the wound complicated with constitutional states of the same kind as those observed in the subjects of surgical operations. Both, at the first, are liable to present the symptoms of shock or collapse, particularly if the labour or the operation has been unusually severe; both have generally a subsequent limited degree of febrile reaction—the traumatic fever of the surgeon—the so-called milk fever of the obstetrician; and both the external surgical wound and the internal obstetrical

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the prevention or detection of the many dangerous diseases which so often follow its performance. How many hospital surgeons seem to imagine that the necessity for their personal attention to a case of amputation terminates at the door of the operating theatre, and leave the after treatment to be solely directed by a house-surgeon or a dresser. How many content themselves with a hurried inspection of the pulse and tongue of the patient, after an amputation, instead of exploring the chest, or watching for the first indication of phlebitis; and how many young practitioners do we not find who speak of an amputation as though the interest connected with it ended with the operation, and as though success were certain if the patient surmounted the shock, or be unaffected with hemorrhage.—*Monthly Journal*, vol. viii., p. 242.

wound, are liable to deviate from the standard mode of reparation; for their secretions may alter morbidly; or they may become the seat of an excess of inflammation or of ulceration; or of phlebitic suppuration and its consequences. In the internal obstetrical, as in the external surgical wound, immediately after their infliction, air occasionally enters by the mouths of the veins opening upon their free surface; both are occasionally, though very rarely, followed by delirium, tetanus, and other nervous complications; and, in like manner, but much more frequently, they are apt to be followed by that form of combined febrile and inflammatory action which we term surgical fever in the surgical patient, and puerperal fever in the puerperal patient. In short, the two species of wounds are of the same pathological nature, repaired by the same pathological processes, subject to the same local pathological deviations, and liable to be attended with the same pathological constitutional effects and com plications.

2. *Analogy in the Pathological Nature of Puerperal and Surgical Fever.*—Two opinions were formerly held with respect to the pathological nature of puerperal fever. One class of pathologists (as Puzos, Levret, Hamilton, White, &c.) regarded it as an idiopathic or putrid fever, *sui generis;* another class (Hey, Armstrong, Mackintosh, Campbell, &c.) still more earnestly maintained that the disease was essentially a local inflammation,—that the fever was merely a consequence of, and attendant upon, this local inflammatory irritation,—and that the malady was to be treated and cured by venesection and other active antiphlogistics. The first of these doctrines became generally abandoned with the advances of pathological anatomy, because local inflammatory lesions in the uterus, peritoneum, chest, &c., were, after death, found far too frequently, and of far too marked and intense a character, to be explained upon the doctrine of a previously existing fever alone. But again, on the other hand, the idea that the disease was essentially a local inflammation, and that the fever was merely an effect symptomatic or sympathetic of that local inflammation, has been in turn gradually disproved also, as the pathological anatomy of the disease has been of late years more completely investigated. For it has been found that,—1st, There is no general uniformity of relation and sequence between the degree and intensity of the supposed cause (the local inflammatory lesions), and the degree and intensity of their supposed effect (the attendant fever); 2d, Sometimes the supposed cause (in the form of simple peritonitis or metritis, &c.), may exist, without these inflammations exciting the usual phenomena of their supposed effect, namely, the symptoms of puerperal fever; and, 3d, We see occasionally cases of true and fatal puerperal fever, without discovering on the dead body any traces or evidence of the local inflammation which had been considered the origin of the disease. In other words under this last class of cases we have the existence of the supposed

effect without the existence of the supposed cause. And this observation holds good with regard not only to the individual local inflammations, which have been illogically dogmatised into the alleged invariable origin of puerperal fever; but it holds good with regard to the whole class of local inflammatory causes. For instance, puerperal fever has been regarded as a form of fever produced by inflammation of the omentum, according to Hulme and Leake; by inflammation of the peritoneum, according to Gordon, Mackintosh, and many others; by inflammation of the uterus, according to Astruc and Denman; by inflammation of the uterine veins or lymphatics, according to Dance and Duplay; &c. &c. Now, in different epidemics, and sometimes in the course of the same epidemic, we see well-marked and fatal cases of puerperal fever, without being able to trace on the dead body any evidence of the one local inflammation theoretically fixed upon as the origin and essence of the whole disease. We thus see fatal cases without any proof whatever of omentitis, or any proof of peritonitis, or of metritis, or of uterine phlebitis or lymphatitis. In other words, in answer to those who maintain the identity of puerperal fever and peritonitis, for example, we can point to cases of genuine and fatal puerperal fever, in which the peritoneum was found perfectly healthy after death;[1] and we can repeat this proof, with regard to any other individual local inflammation that may be improperly adduced and considered, as the cause and origin of the whole disease.

Some authors, while they maintain the disease to be a fever entirely symptomatic of some local inflammation, at the same time hold that this local inflammation may be seated in different parts in different cases, and different epidemics; and that the disease originates, in one case, in metritis; in another, in ovaritis; in a third, in peritonitis; and so on. Without remarking on the illogical nature of imagining, that the same disease may have such varied origins, we may once more pointedly observe, that (as sometimes happens in continued fever), occasionally, though very rarely, no inflammatory lesions whatever can be traced upon the bodies of patients who have died of puerperal fever. Dr Locock has observed several cases of this kind.[2] And, in the practice of the late Dr Beilby, I saw one

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[1] For example, in 222 autopsies which he made of puerperal fever patients, Tonnelle found peritonitis in 193; and there was apparently no evidence of it in 39 of the dissections. "Inflammation of the peritoneum," he observes, "is one of the most frequent alterations which we met with as a result of puerperal fever; but (he adds) it would be a grave error to think that it existed always. Sometimes, in fact, this membrane preserved its natural aspect, and the most minute researches could not detect in it any appreciable change."—Archives Générales de Médecine, vol. xxii. p. 350.

[2] See his valuable article on Puerperal Fever, in the Library of Medicine, vol. i. In these several cases, "the most careful search (says Dr Locock) was made for morbid alterations of structure in the veins, the absorbents, the muscular structure, and the lining membrane of the uterus and of the adjacent parts, and nothing could be found to explain the cause of death."—P. 351.

very marked and rapidly fatal case of puerperal fever, in which my colleague, Professor Bennett, was unable to detect anywhere in the abdomen, or in the uterus, its appendages or vessels, any traces of inflammatory action or effusion. The great rarity of such instances is no sufficient argument against their important bearing upon the question of the nature of puerperal fever.

The evidence which I have thus briefly sketched, has induced of late, most of our best pathologists to reject the idea, either that puerperal fever is an idiopathic fever *sui generis*, or a disease originating in and identical with peritonitis, or with any other local inflammation. And, on the other hand, many investigations and experiments made during the last ten or twenty years upon the effects of an acquired, or artificially excited, state of vitiation or poisoning of the blood, have inclined them more and more to adopt the doctrine, that the real source and cause of puerperal fever is to be found in a toxæmia or morbid state of the circulating fluid. The direct injection of pus, and other morbid secretions and matters, into the blood of the lower animals by Gaspard, Cruveilhier, Castelnau, and others, have produced a series of symptoms during life, and a series of lesions on the dead body, showing a very strong analogy to those of puerperal fever. The commixture of pus with the blood in the human subject, in cases of phlebitis, &c., in which pure pus enters directly into the circulation, gives rise to a similarity both of febrile functional lesions as seen during life, and of inflammatory organic lesions as seen after death. And in the puerperal female, there exist such conditions as facilitate the infection of the general circulation, by pus and other morbid matters contained in the uterine cavity. For they may obtain easy access to the general circulation —1. Through the orifices of the utero-placental veins, that open upon the internal surface of the uterus; which are, perhaps, not always completely closed; and which have their mouths constantly in contact with the contents and secretions of the uterine cavity; 2. Through the inoculation of morbid and contagious matters upon the abraded surface of the vagina; and, 3. By any accidental inflammation commencing in the lining membrane of the maternal passages (which are distended and contused during delivery), readily passing by the law of continuity alone, through the venous orifices opening on the interior of the uterus, and thence along the lining membrane of these vessels. Under the now generally adopted view, that puerperal fever originates in a vitiated condition of the blood, we can solve more easily the problem with respect to the relation of the two elements, constituting puerperal fever—namely, first, the febrile action, and, secondly, the internal inflammations, which are present during it. For under this pathological view we see, that the fever is not itself the cause of the attendant inflammations, nor these inflammations themselves the cause of the attendant fever; but that both of them—that is, both the fever and the inflammations—are the simultaneous sequences or effects of one common cause—

namely, the original vitiated or diseased condition of the general circulating fluid. And farther, the same doctrine enables us to perceive, how in one set of cases, or one epidemic of puerperal fever, the febrile effect or element may be more marked than the inflammatory; and how, in others, and these generally the most amenable to treatment, the inflammatory element or effect may be more marked and more prominent than the febrile.

These views regarding the pathology of puerperal fever, are borne out and corroborated by what we see in surgical fever and its inflammatory results among the internal viscera of the body; for no pathologist has ventured to maintain that the pleurisy or peritonitis, for instance, which we often see upon the dead bodies of patients who have died of surgical fever, is the effect or consequence of that fever; or that the fever itself is a consequence or effect of these local inflammations. All at the present day are agreed that the phenomena of the occurrence of these local internal inflammations is not explicable, and is not in any way explained, by the old doctrine of sympathy or metastasis; and few now dissent from the idea, that we are to look for their origin, and for the origin of the attendant fatal fever, in some diseased or vitiated condition of the blood. What this vitiated condition of the blood may specifically and actually consist of, in puerperal and in surgical fevers,—whether it consists in the presence of some one or more of the elements of purulent matter alone; or whether other animal secretions than pus may be its common or occasional cause; and whether the attendant type of fever and its effects may not be fixed and regulated by differences in the diseased material which is present in the circulation, are all questions which a more advanced era of pathological research, and a more subtle chemistry and histology than has yet been brought to bear upon them, will, perhaps, yet ultimately determine. The character and phenomena of puerperal and surgical fever and their inflammatory results, agree with those of small-pox, measles, scarlatina, rheumatism, and the other so-called "disseminated inflammations" of M. Chomel[1] in this respect, that, whilst originating, as he believes, in different specific "infections of the liquids" or blood, they all agree with each other in several respects; as that they cannot be excited artificially by the common causes of inflammation, but are developed by specific causes; they are thus secondary inflammations resulting from a primary morbid diathesis or alteration of the animal fluids; antiphlogistic measures possess usually comparatively little influence upon the duration of these disseminated inflammations, and often but a doubtful influence upon their intensity; and, lastly, whilst the unity of each affection is preserved, each is represented by a multiplicity of local inflammatory lesions, developed simultaneously or successively, and frequently in organs and parts distant from each

[1] Leçons de Clinique Medicale, 1834, page 529 to 539.

other. Of this last general fact, as far as it holds good with regard to puerperal and surgical fever, the next paragraphs will afford us due evidence.

3. *Analogy in the Internal Pathological Lesions left by Puerperal and Surgical Fever.*—Both diseases, as we have already so repeatedly stated, are liable to leave upon the dead subject evidence of recent acute inflammation in one or in several of the internal surfaces or organs of the body. These internal inflammations are generally characterised by an unusual tendency to the effusion of loose coagulable lymph and pus. We possess as yet few statistical details as to the internal organs which are most frequently found affected with inflammatory lesions in surgical patients. But Dr Chevers, in the valuable communication to which I have already referred, has given us some interesting details relative to this point, in the results of the post mortem examination of the 153 fatal surgical cases that had occurred in Guy's Hospital; and which form the basis of his essay. I have already stated that, with the exception of 19 cases, recent internal inflammatory lesions were found in all of these cases; or, in other words, in 134 out of the 153. The following table shows the special recent internal inflammations that were discovered in the post mortem examination of these 134 surgical patients :—

Inflammatory Lesions in 134 *Fatal Cases of Surgical Fever.* (From Chevers.)

Peritonitis was observed in	52	Cases.
Enteritis (excluding cases of Hernia)	9	,,
Pneumonia and its results	47	,,
Pleuritis	35	,,
Bronchitis, Laryngitis, and Diphtheritis	4	,,
Pericarditis	14	,,
Arteritis and Aortitis	4	,,
Phlebitis	3	,,
Meningitis	27	,,
Cerebritis	9	,,
Cystitis	8	,,
Pus in Muscles or Joints	3	,,
Inflammation of Tunica Vaginalis	1	,,

Let us contrast with the above the records of the pathological anatomy of puerperal fever. These records are defective in this point, that most of the observers have apparently confined their examinations and descriptions almost entirely to the morbid changes seen in the uterus, uterine appendages, peritoneum, and other parts, which they believed to be the principal seats of the disease; and have not given the morbid appearances found within the chest, encephalon, &c. Dugès and Tonnellé have, however, left the results of their researches into the state of the thoracic organs, and have examined the condition of the encephalon in a few instances. Dugès reports the result of the examination of the bodies of not less

than 341 women who had died of puerperal fever. Among these 341 cases he found the following lesions, in the following proportions :—

Inflammatory Lesions in 341 *fatal cases of Puerperal Fever.* (From Dugès.)

Peritonitis was observed in	266	Cases.
Metritis, or pus in Veins, &c.	200	„
Ovaritis	48	„
Gastritis and Enteritis	4	„
Pleuritis	40	„
Pericarditis	6	„
Arachnitis	1	„
Pus in Muscles or Joints	8	„

The observations published by Tonnellé are more minute, as respects, at least, the uterine and abdominal organs. He examined the bodies of 222 puerperal patients, with the following results :—

Inflammatory Lesions in 222 *cases of Puerperal Fever.* (From Tonnellé.)

Peritonitis was observed in	193	Cases.
Metritis and Ovaritis	197	„
Pus in the Uterine Veins or Lymphatics ...	112	„
Gastritis and Enteritis	6	„
Pleuritis	43	„
Pneumonia	21	„
Pericarditis and Hydro-pericarditis	1	„
Pus in Liver, Pancreas, Muscles, &c. ...	19	„
Pus in Joints	10	„

The preceding three tables, from Chevers, Dugès, and Tonnellé, afford a variety of important evidence relative to the internal morbid actions which precede death in puerperal and surgical fever.[1] Among other matters, they specially illustrate the following points :—1. Both diseases generally leave upon the dead body ample evidence of the occurrence before death of acute and often extensive internal inflammatory action. 2. The internal inflammatory lesions are seldom limited in the same case to one organ or texture only,

[1] Let me here take an opportunity of remarking, that before we can expect tabular results from post-mortem examinations, showing, more completely than the above, the analogy between puerperal and surgical fever, the entire pathological anatomy of each disease will probably require to be studied and made out at the same periods, within the same hospital, and by the same pathologist. For (judging at least from puerperal fever) the disease seems liable to vary somewhat at different times, and in different localities, in the forms, seats, and intensity of the local inflammatory lesions which it produces. And when we have the lesions noted down by different anatomists, the reports too often differ though the lesions are the same; for the attention of one observer is apt to be called particularly to one set of appearances, and that of a second observer to another. For example, we feel quite sure in stating that in Dr Chevers' table the number of cases of phlebitis (three) is greatly understated; while in the tables of Dugès and Tonnellé a similar remark is applicable to the encephalic and perhaps other secondary lesions. A comparative inquiry, by the same anatomist, into the lesions respectively left by puerperal and surgical fever in some of the large hospitals of Paris, Vienna, or Prague, would in this respect be extremely interesting and important.

but two or more different viscera or surfaces are usually observed to have been either the simultaneous or successive seats of inflammatory action, and the different parts thus attacked are sometimes very distinct and distant from each other. 3. The internal viscera or textures, which are the first and principal seats of inflammation, are often far removed from the original wound or lesion, particularly in those cases in which the wound or lesion is in the head or extremities. From the days of Valsalva and Morgagni downwards, injuries of the head have been observed to be often followed by inflammation and the effusion of pus in the liver, lungs, pleura, &c. Dupuytren, Sir Charles Bell, Guthrie, and others, have long ago remarked that after amputation of the extremities, inflammation of the lungs or pleura (parts sufficiently distant from the seat of the wound) was a very common and a very fatal sequel.[1] Pleurisy was found so often by Velpeau after these and other operations, that he proposed to give the disease the name of the "pleurisie purulente des opérés."[2] Rokitansky, Routh, and Kiwisch describe the inflammatory lesions of the chest as very common in the puerperal fever of Vienna and Prague; and I have seen it in some cases in Edinburgh; but generally, like many of these internal inflammations in puerperal and surgical fever, the phenomena of it are not very marked during life. This, like the other inflammations in these cases, is often latent in its symptoms, though the post-mortem results show how intense the morbid action had been. But, 4. In obstetric cases the uterus, uterine appendages, and peritoneum, are the most common constant seat of the accompanying internal inflammatory action, though the organs of the thorax, &c., are not unfrequently also affected. Various causes appear to lead to this special localisation of the inflammatory action and effusions in puerperal fever. All kinds of wounds of the pelvic and generative organs are particularly liable to be followed by

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[1] From some notes which I made a few years ago, relative to the frequency of pulmonary inflammations after operations, I find that, out of 77 dissections of patients dying after amputation, and recorded by Drs Lawrie, Reid, Peacock, Erischen, Orr, and May, in 30 instances inflammation of the pulmonary organs was observed; in 38 out of the 77 phlebitis and purulent deposits in different parts were observed. In a valuable paper by Professor Erischen, in the Medico-Chirurgical Transactions, vol. 26, that gentleman has shown that out of 62 cases of death, after surgical operations and injuries (exclusive of burns), taken indiscriminately from the records of University College Hospital, in no less than 28 there were evident signs of pneumonia, as shown either by the diseased condition being confined to one lung, by its having advanced to solidification, or by the co-existence of marks of inflammatory action in the pleuræ, or bronchial mucous membrane. In 11, the lungs were in a doubtful condition, presenting the characters which are common to the first stage of pneumonia and to passive congestion, without there being any collateral circumstances, by which the diagnosis could be more clearly established. In 9, the lungs were found more or less diseased, but not inflamed or congested; and in 14 only were they quite healthy.

[2] Revue Médicale for 1826, vol. iv. p. 418.

peritonitis, when they give rise to surgical fever.[1] In midwifery cases the uterus—the seat of the original wound—is, like the external wound in surgical cases, liable to inflame; and this morbid action readily extends, by the law of continuity of tissue, to its appendages and peritoneum. The table of Dr Chevers proves how very common peritonitis is in instances of surgical fever (being met with in above 30 per cent. of his dissections); but, besides the preceding reasons, there is another important one, why it should be still more frequently met with in puerperal fever. The uterus, and its peritoneal and other coats and appendages, have been the immediate seat of injury and lesion in the act of parturition; and in all febrile diseases complicated with or ending in disseminated inflammations, we find the attendant inflammation particularly liable to localise itself upon any weak or injured part. The injury or previous diseased state of a joint is thus sometimes observed to regulate the seat of attack of gout or rheumatism. In cases of hemiplegia, herpes is said to attack the paralysed side in preference to the other. And in this way small-pox, &c., is sometimes observed to be specially prevalent upon a previously weak or injured part of the surface, though that part may not be at all a common seat of confluent variola. Thus, —"A sailor was admitted into the Dreadnought on account of a bruise inflicted on one side of his breech by a fall into the hold of a ship. In the course of some days he left the hospital, having recovered from the injury, but still showing a bruise mark on the breech. A short time after, he was again admitted with severe febrile symptoms, which terminated in the eruption of small-pox. The pustules were discrete, and very few in number, all over the body, except in the exact seat of the former bruise,—and there they were extremely numerous, and for the most part confluent."[2] Might we, in any cases, regulate in any degree the principal site of the secondary inflammatory effusions in puerperal or surgical fever, by lowering, by some means or other, the vitality of a selected portion of the surface of the body? Various continental surgeons have spoken highly of the beneficial action of the actual cautery to parts of the surface in bad cases of surgical fever. Does it act upon the principle alluded to?

4. *Analogy in the Symptoms of Puerperal and Surgical Fever.*—There is almost no disease which varies more than puerperal fever does in different cases, in the intensity of its symptoms, and in the forms which they assume. Most authors have hence described several varieties of the disease, as the inflammatory, the bilious, the

[1] "Dr Addison has long been in the habit of alluding, in his lectures, to the fact, that injuries and operations, especially when occurring about the generative organs or parts around the pelvis, are apt to be followed by peritonitis or pleurisy of the most rapidly fatal description."—Guy's Hospital Reports for 1843, p. 83.

[2] Budd, in Medico-Chirurgical Transactions, vol. xxv., p. 129.

gastro-enteric, the nervous or ataxic, &c. &c. The same remark, with respect to the variability of its type or forms, holds good with regard to surgical fever. But when either disease is fully marked, the symptoms are sufficiently striking, and sufficiently similar in each,—the more marked consisting of rigors; a pulse varying in strength, but always constant with regard to the fact of its rapidity; an altered, and frequently darker, or almost icteric hue of the surface; the skin sometimes hot and dry, sometimes bathed in perspiration, or these states alternating, without any material crisis in the febrile action; local pains, and functional derangements in the parts which are the seat of the internal inflammatory effusions; but the local symptoms of these local inflammations are often, it is to be remembered, very masked and latent; anxiety, and general prostration and adynamia; very frequently nausea and vomiting, occasionally diarrhœa; and, latterly, laboured or hurried respiration; sometimes sudden swellings and effusions in the joints and subcutaneous tissue, &c.; and often, at last, rapid sinking, with or without delirium. There is no disease to which it is so difficult to assign a set of pathognomonic phenomena as puerperal and surgical fever; but whoever has seen much of the symptoms of the disease in one class of patients, has no difficulty in identifying the disease by these same symptoms in the other class.

In the preceding remarks, I have not attempted to discuss fully and minutely all the various points of analogy between puerperal fever and surgical fever. My object has been principally to direct, however imperfectly, the attention of my professional brethren to the subject, under the strong hope, that the future comparative study of the disease, and of its characteristic symptoms, lesions, and causes, in the puerperal and in the surgical patient, may yet serve mutually to illustrate the pathology of this fatal affection in each class of cases; and perhaps this hereafter may lead to the discovery of more enlightened principles of prevention and of treatment, than the isolated and divided study of the same malady respectively by the obstetrician and by the surgeon, has in times past been fortunate enough to elicit. And I do believe that if any man should ever have the good fortune to detect, or suggest any simple and practicable measures, either to avert and prevent, or to mitigate and cure, surgical and puerperal fever, he would, in doing so, confer one of the greatest of all possible benefits upon the advancement of surgery and midwifery, and be the means of saving numerous lives in operative and obstetric practice. The discovery of any such measure or measures, would undoubtedly form a most important era in the march of professional discovery. Nor does it seem utterly hopeless to expect the possible detection of some such measures, in the way of prevention at least, if not in the way of cure. We are the more encouraged to hope for such a result, as we already know various conditions capable of increasing on the one hand, and of decreasing on the other, the chances and

the intensity of surgical and puerperal fever. The disease, for example, is confessedly more common and more severe among the population of towns, than among the population of the country;[1] it is more frequent and more fatal among hospital patients than in private practice; and much more so, in crowded wards than in those where the patients are few and provided with a full and free supply of fresh air. There are epidemic states in which puerperal and surgical fever is frightfully common; others in which it is very rare and apparently difficult to excite; some localities and towns are far more frequently the seat of it than others are; and various states of the economy seem to predispose the constitution to it, or against it. Surgical and obstetric patients suffering under internal organic diseases (particularly of the abdominal viscera), and under certain functional derangements of the kidney,[2] and perhaps of other organs seem specially liable to be attacked with this consecutive fever and inflammation. In addition to the common antiphlogistic precautions and measures, various special prophylactic measures have been proposed with the view of guarding patients against attacks of puerperal fever, when epidemic in its character, such as large and repeated doses of quinine, chlorate of potass, &c., before or immediately after delivery. This is a line of inquiry that seems particularly to demand attention, and to deserve further careful investigation and research. Some surgeons have, with a view of rendering their operations more successful, subjected their patients to previous preparations, regimen, and drugging. But the exact power and propriety of these and other measures have by no means yet been precisely fixed and ascertained. Of the propriety and efficacy of one class of prophylactic measures against puerperal fever, few practitioners in this country have any doubt. I allude to those measures which are calculated to prevent the medical practitioner or the nurse being the unhappy medium of carrying the contagion of the disease from one puerperal patient to another. This is not a fit opportunity to enter into such a long question as that relative to the occasional contagion of

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[1] Among nearly the same amount of population living in the towns, and living in the country districts of England, childbirth is far more fatal among the inhabitants of the former than of the latter. In the four years ending 1841, there died out of a town population of about 1,800,000, 3195 women in childbirth; and out of nearly the same amount of rural population there died only 1806 mothers. The excess in towns was thus more than 1000 lives; the mortality was 221 and 137.—(Mr Farr, in *Registrar's Fifth Report*, p. 408). A similar difference will, I believe, be found to hold good relatively to the success of amputations and other surgical operations in town and in country practice.

[2] Among the 153 fatal surgical cases published by Dr Chevers, in as many as 93 of the patients some lesion was found in the kidneys, liver, or spleen. In 72 of these 93 cases "the kidneys were observed to be in a state of marked disease, either presenting remarkable congestion, softening, mottling, or the granular or cystiform alterations." In 21 of the remaining cases there was "marked disease of the liver or spleen."—P. 91.

puerperal fever; nor to inquire under what different circumstances and conditions it may prove capable of being communicated, as whether it ever originates from the mere inhalation of contagious effluvia, brought near to the puerperal patient, &c. I shall content myself with observing here (what I have taught elsewhere for the last ten years), that there exists, I believe, on record, a series of facts amply sufficient to prove this at least, that patients during labour have been and may be locally inoculated with a materies morbi capable of exciting puerperal fever; that this materies morbi is liable to be inoculated into the dilated and abraded lining membrane of the maternal passages [1] during delivery, by the fingers of the attendant; that thus in transferring it from one patient to another, the fingers of the attendant act, as it were, like the ivory points formerly used by some of the early vaccinators; that the materies morbi most capable of being thus inoculated and generating the disease in a new individual seems to be the inflammatory products effected upon the serous or mucous surfaces of females who are suffering under puerperal fever, or who have died of it; and lastly, that other inflammatory effusions, when in the same way transferred and inoculated into the puerperal female, appear to have sometimes the same effect, such as the effusions into tissues, that are the seat of an asthenic, erysipelatous, or gangrenous type of inflammation.[2]

Believing as many practitioners do, in this occasional communicability of puerperal fever, it becomes, of course, their bounden duty to avoid as far as may be, the possible propagation of it in this manner, and to use every available precaution against such a sad misfortune. And the omission of these precautions has proved, I sincerely believe, the cause of many deaths in childbed, and continues still to do so, particularly on the continent of Europe. In the large hospital at Vienna, out of 21,120 women delivered from 1840 to 1846, 2260 died; or about 1 in every 10 mothers delivered perished, chiefly from puerperal fever. Latterly this mortality has diminished so far, that in 1848, not above 1 in 74 mothers died. This great change was effected, in consequence simply of means being adopted to prevent the contagious inoculation of the disease being carried and transferred by the medical attendants and students, from those already affected and dead of the malady, to women who were in

[1] We know from the phenomena of dissection-wounds, how very small an abrasion or wound is sufficient to allow of the fatal inoculation of the morbid poison. Mr Travers relates a case of dissection-wound of the finger of a medical gentleman followed by fatal fever, inflammation, and gangrenous erysipelas, and where the original lesion or point of inoculation was so minute that it was not seen by the naked eye, till it was first detected by using a lens. —See his work on Constitutional Irritation. Vol. i. p. 224.

[2] It is perhaps worthy of remark, that in cases in which puerperal fever is thus communicated and produced, two or three days after delivery usually elapse before the disease breaks out; or, in other words, there is a latent period similar to what we see in the inoculation of small-pox, measles, and other communicated diseases.

labour. Before 1847, almost every woman delivered in the wards attended by the medical students, was examined by a number of students; and these students had been often allowed immediately previously to touch and handle the bodies of women who had died of puerperal fever, and were even taught upon them the manipulations and operations of midwifery. The mortality altered and diminished immensely and immediately from the time (May 1847), that the assistant-physician, Dr Semelweiss, prevented students from touching parts at the autopsies, and directed all of them to wash their hands in a solution of chlorine before and after every vaginal examination.[1]

In this instance, and in others which might be cited, was not the want of a due knowledge of the communicability of puerperal fever, the cause immediately leading to these numerous maternal deaths? Were these mothers not sacrificed merely to medical prejudice, in the form of a total disbelief on the part of our continental brethren, in the contagious communicability of puerperal fever? And, lastly, if puerperal fever may be occasionally communicated by inoculation to puerperal patients, may not surgical fever be occasionally communicated to surgical patients in the same way? The question is perhaps a far more momentous one than the simple past neglect of it might *a priori* lead us to infer. It would be perhaps wandering too far out of my own province if I were venturing to discuss it here. But I may state that, on inquiring into the subject, I have repeatedly heard of instances of a rapid succession of surgical fever cases and disasters in the practice of the same surgeon, while the other surgeons in the same locality had their patients recovering as usual; and I have been told of one eminent surgeon having locked up all his cutting instruments, for some weeks, under the impression that they were in some way or other infected, and unaccountably dangerous to all the patients upon whom, for a short time previously, he had had occasion to employ them. A gentleman, who was formerly surgeon to a very large hospital, and also an extensive practitioner in midwifery, informs me that, during the period of his surgical superintendence of the hospital, and when consequently often touching the discharges from all kinds of wounds and breaches of surface, puerperal fever was, from time to time, common in his private obstetric practice,—and, at the same period, he saw many of his hospital surgical patients die with similar symptoms. Since giving up the surgical charge of the hospital, the occurrence of puerperal fever has ceased in his private midwifery practice. A sufficient series of such cases, would so far add another proof of identity in the two forms of fever—the puerperal and surgical—by proving an identity in their origin and mode of causation.

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[1] See Wieger in the Revue Medico-chirurgicale, vol. vi., p. 136; and Routh in Medico-chirurgical Transactions, vol. xxxii. p. 27.

Part Second.

REVIEWS.

Students' Numbers of the Weekly Medical Journals. October 1850. *Introductory Addresses at the Medical Schools of London.* Session 1850–51.

We have no intention of criticising any of the publications which may be embraced under the titles which we have placed at the head of this article. We content ourselves with expressing our general concurrence in the various advices given to students, to be regular and systematic in their studies, to avoid idleness and dissipation, and to be prudent in the choice of their companions. We use them for the purpose of introducing a subject, on which we wish to say a few words, for the purpose of directing towards it the attention, not so much of medical students, as of those in authority at the various public examining boards. We allude to the classical acquirements of candidates for degrees and diplomas, or, to use a phrase more in vogue among students, "the Latin examination."

We assume it to be a proposition not admitting of dispute, that it is not only proper, but necessary, that the various public boards should satisfy themselves that the candidates for their degrees and diplomas are possessed of a knowledge of Latin, sufficient at all events to enable the graduate or licentiate to write a prescription in this language. This is commonly done by making the candidate translate a passage from some Latin author, when he presents himself for his professional examination; and it is a notorious fact, that many a student who is well prepared on all branches of professional knowledge, goes before his examiners with great apprehension as to the result of his Latin examination, from the consciousness that he has up to that moment never thought of the language from the time that he left school. It more frequently happens, however, in the case of such individuals, that the consciousness of deficient classical attainments flashes upon them during the last term of their study, and then they feel themselves obliged, at the very time when they ought to be giving undivided attention to medical science, to be relearning, from the dull pages of a dictionary, the meaning of long-forgotten Latin verbs, substantives, and adjectives. To prevent this, students are commonly advised to keep

up, during their whole term of study, a knowledge of the classics, by reading some portions of them daily. But it is much more easy to give this advice than to ensure its being taken. Those men who are good scholars will gladly follow it,--to them an occasional dip into an ode of Horace is a literary luxury; those who are disposed to be attentive to their medical studies, but who have little acquaintance with, and still less taste for, the classics, will not be readily induced to submit to what is a drudgery, when they have fresh subjects of professional interest daily thrust upon their attention, and requiring daily study for their thorough comprehension; and those who are not disposed to be industrious at all, will, with perfect impartiality, equally neglect their classical and professional studies. We hold this advice, therefore, to the medical student, to read some portion of the classics daily, to be theoretically good, but practically useless. How, then, is the evil to be remedied? How is it to be ascertained that the candidate for a diploma is completely furnished with a knowledge of Latin,—in other words, how is he to get over his Latin examination, —without sacrificing to the irksome process of cramming with a dead language, the time which ought to be spent in acquiring professional knowledge? Our answer is, by the examining boards allowing students to pass an examination in the classics at the commencement, and not at the end, of their curriculum of study. Several advantages will be gained by adopting this course; first, the candidate, if found wanting, has plenty of time before him to make up his deficiency, and has plenty of leisure for reading Latin during his first year, when he has only one or two, instead of eight or ten branches of medical study to engage his attention; secondly, the qualifications of candidates in this respect will be much better ascertained, and a higher amount of classical knowledge will be required and obtained. We are quite sure we may, without imputing dereliction of duty to any examining board, affirm, that when a young man comes up for examination after four years' study, and is found well prepared in regard to professional matters, it requires a very grievous shortcoming in Latin to induce examiners to reject him on this ground alone, and that many young men are allowed to pass with a deficiency of Latin, which would have led to their being remitted to their studies, had the period of their probation been the beginning and not the end of their studentship. And we can easily understand why this should be the case; the examiners feel it to be a severe measure in itself, to reject a candidate upon the preliminary Latin examination, whom they might find, if they went on to his professional trials, to be well qualified as regards medical matters; and, moreover, the remitting him to his studies, for the purpose of learning Latin, is quite inoperative, unless it be for at least a year,—for how can a young man relearn the Latin language in three or six months, who must be occupied in keeping up his knowledge of anatomy, chemistry, materia medica, surgery, &c., at the same time? It is not difficult to conceive that, with such considerations before them,

examiners should sometimes allow a candidate to pass who has made but a shabby appearance in Latin, if he acquit himself creditably on his professional examination.

But we believe that, upon the plan which we have ventured to recommend, not only would the classical acquirements of the candidate be more correctly determined, but a higher standard of classical knowledge would be demanded and obtained. The existing system seems to be calculated rather to ascertain that the candidate is not utterly ignorant of Latin, than to determine that he positively has anything like a satisfactory acquaintance with it. At most of our examining boards, if it is not expressly laid down by their regulations, it is established by their practice, that the candidates are required only to be able to translate passages from one or two books, such as Celsus, Heberden, Gregory's Conspectus, or the London Pharmacopœia, and hence a practice prevails of the student getting up, by the help of translations and private tutors, a mechanical sort of acquaintance with the greater part of the book most likely to be set before him, which will often carry him, if he has moderately good luck in the passage selected for him, we shall not say with credit, but at least without rejection, through his Latin examination. This is, at the best, a very poor evidence of classical knowledge, but examiners are deterred from asking more, from the very dread of being called upon to reject a man on Latin who may have spent four years diligently enough in studying medicine, and thus prevent him, for the want of what is not, strictly speaking, an essential branch of knowledge, from entering on the practice of a profession for which he may otherwise be well qualified. If, however, they have to do with a student who has three or four years of student-life before him, they will have no compunctions on this score, and will demand of him, not that he can translate this or that book, but that he can translate any Latin book that is placed before him. Of course, in giving this latitude of selection to an examining board, we presuppose their possessing sufficient common sense to see that by any Latin author we mean any one not generally esteemed difficult. We do not insist upon each candidate for a surgeon's diploma being *au fait* at Quintilian, Plautus, and Terence; but we do not think that it is too much to expect that he should have enough of the education of gentlemen to be able to translate a passage of Livy or of Virgil, or an ode of Horace,—in short, that he should have a moderately good knowledge of Latin as a classical language, not a mere grinder's acquaintance with one ancient, or one modern, medical book in Latin.

There are few persons, we apprehend, who will be disposed to deny that there is much room for improvement in classical attainments among medical students. To take no higher test than the capability of writing a prescription,—if we may dignify this with the title of classical at all,—how few young men, in the third and

fourth years of their studies, can do so respectably! We have had occasion to observe this particularly, from having made the writing of prescriptions a subject of exercise—we think it a useful one—in the lecture-room, and from having had a considerable number of young men under us as pupils in dispensary practice; and we must say, that the result of our experience is, that twenty of their prescriptions will be good enough therapeutically, chemically, and posologically, for one which will be free from error in the Latinity. We could give some curious anecdotes illustrative of this. We shall mention one which came under our own observation. A dispensary pupil, or clerk, as they are termed,—one who was engaged in, and very fairly qualified for, treating patients in their own houses, under the superintendence of the medical officer of the district,—had occasion to direct a patient to be supplied with a clyster-pipe, and wrote on the prescription-sheet the following elegant order for the article in question, "*Habeat enema fistula;*" which, we presume, he thought to be the Latin for, "Let him have—an injection—pipe." We never heard that this individual was "plucked" for his Latin; he may have got a tolerably easy passage in Gregory to translate, and he may have squeezed through,—or he may have got upon a difficult one, and stuck fast. If he was remitted for three or six months, how could we expect such a man to make anything of a Latin scholar of himself under such circumstances? He might grind up Gregory, and get through at a second trial, and yet not have acquired sufficient general knowledge of Latin to know that *habere* governs the accusative, and that, whatever he might make of the word *enema* in his prescription, he ought at least, in common fairness, to put an *m* to the end of *fistula*. But had he, at the commencement of his first winter's study, with probably nothing but anatomy and chemistry to occupy his attention, been examined, say on a passage of Ovid, been found wanting, and recommended to study Latin for at least a year, he might have been able to refresh his school-boy recollections of the rules of syntax, when he was not struggling to master the rules for applying the short forceps, or for diagnosing tubercles in the lungs,—have acquired, not a mechanical knowledge of one or two books, but a re-acquaintance with the rudiments of Latin grammar, and thus have been able, in later years, to write prescriptions whose hideous language—we shall not call it Latinity—would not excite the laughter of any smart druggist's apprentice who had occasion to dispense them.

There are one or two objections to this plan which may be offered, and upon which we wish to say a few words. It may be said that we take the young student unawares, and subject him to the ordeal of an examination and the risk of "plucking" at the outset of his career, without giving him time to refresh himself for the trial with a short period of study. This will cease to apply so soon as the

practice becomes general among examining boards; and the first thing a youth will do, who resolves on being a doctor, will be to prepare himself for becoming a student of medicine by revising his Latin. But even if this should escape his attention in time, and he should deem himself unfit to pass an examination in Latin, he has the remedy in his own hands,—he has only to occupy a good part of his first year in studying for it, and to come up at the commencement of his second year; for we would have it distinctly understood, that we would not make the plan which we recommend compulsory. We do not wish the boards to drag the student before them at the beginning of his first session; we only wish them to make arrangements for permitting him to do so if he chooses, and thus to encourage him not to delay the acquisition, or rather the re-acquisition of Latin, to the time of his studentship, when other and very various subjects of study are pressing upon him.

It may also be urged, that by this course of proceeding we run the risk of doing exactly what we wish to avoid, viz., leading the student to neglect Latin during the course of his professional studies,—leading him to suppose that, because he has "passed his Latin," he may throw it to the winds. To this we answer, first, that if he has been *well* examined, and have justly been allowed to pass at the beginning, he must have had such a knowledge of Latin that he will not be able to lose it before the end of his studentship; but farther, there yet remains a test to determine that he actually has not lost it,—we mean his being made to write a Latin prescription without abbreviations when he passes his professional examination. We shall thus have accomplished two good things: we shall have ascertained that the student was fairly instructed in Latin—or if he was not, we shall have sent him back to study it when he had time to do so,—and then we shall have seen that he had not thereafter neglected it, but could, at all events, apply it to the purpose for which it is most required by a medical man. Our advice to students, therefore, if the public boards would enable them, would be this:—Go up for your Latin examination at the beginning of your first year; or if you do not think you are qualified to pass, occupy a good portion of your first year in preparing for it, and go up at the beginning of your second; thereafter, by all means, if you can spare time, "keep up the knowledge of the classics by reading some portions of them daily," and "translate every Greek or Latin sentence you may accidentally meet with;" but at all events, learn to apply your Latin to the purpose for which you will afterwards employ it, by practising the writing of prescriptions.

We do not wish to imply, that a knowledge of the classics has no higher value than to enable a doctor to write grammatical prescriptions. We are not even expressing any opinion that rhubarb might not be prescribed in English just as well as in Latin; but this is the custom in our profession, and, therefore, the doctor must have Latin enough for that at all events. Putting this out of view,

and supposing that we all wrote our prescriptions in the vernacular, we would still have our medical man to be a well educated gentleman, and as such we would have him, to some extent at least, a classical scholar. We would cite the well known *delectant domi, non impediunt foris*, but the quotation is hackneyed.

We have restricted our remarks to Latin, not that we mean to say that a moderate acquaintance with it is our beau ideal of the literary attainments of a medical man, but because we do not wish to enter upon the vexed question, as to what is the proper amount of preliminary education which he ought to possess, and because we merely wish to address ourselves to what we think would be an improvement of things as they now stand. We have also spoken of this plan as if it were something totally new, and were a suggestion emanating from ourselves. It is not so; it is neither new nor untried. The College of Surgeons of Edinburgh, last year, adopted it; and, so far as this amount of experience entitles us to speak, it promises to work well. It has proved satisfactory to examiners, and is, we know, popular among the students. Upon a very short notice, 142 young men presented themselves for examination last winter. Passages from Horace, Virgil, Cicero, Celsus, Heberden, and Gregory were placed before them. They were required to give a written translation of at least one of these to enable them to pass; and those who translated more were recorded as having corresponding degrees of excellence, a circumstance which will be noted in their favour when they come up for their final examination. Of course, on this first occasion many of these were fourth years' students, who have since obtained their diplomas, but a large proportion were in their first, second, and third years. Of the whole 142, 112 passed, making the rejections rather more than 1 in 5. The same plan of examination will, we believe, be adopted by the Royal College this season. The Medical Faculty of the University of Edinburgh have recently determined not only to adopt this course with respect to the Latin, but have taken a step in advance, and purpose henceforth to allow students to be examined in Botany and Natural History during their first years of study.

It may, perhaps, be objected that the adoption of this practice may lead to this inconvenience,—that a student, who has begun his studies in Edinburgh or in London, and has passed his Latin examination at one of these, may afterwards think it to be for his advantage to graduate or take his diploma in the other metropolis, but may be unwilling to lose the advantage of having already got over one little trial, and thus be induced to forego what he might afterwards have cause to regret. This is easily got over,—a certificate from one examining board, of having passed his Latin examination, specifying the books on which he was examined, and the character of his performances, might be received by another board as evidence of his classical attainments, and save him from the necessity of undergoing another examination.

Epidemics Examined and Explained: or Living Germs proved by Analogy to be a Source of Disease. By JOHN GROVE, M.R.C.S.L. London: 1850. 8vo. Pp. 192.

THE recent visitation of cholera has had the effect of directing the attention of European practitioners very seriously to the causes which influence the spread of epidemics; and, although the experience of many observers of the late pestilence has yielded results in many points contradictory, and in the present state of our knowledge rather provocative of discussion than suggestive of great truths, it has brought into the field a host of writers distinguished more for fertility in hypotheses, ingenuity in handling analogies, and anxiety for the universal application of shadowy theory, than for any great accession which they have added to human knowledge. It is sad to reflect that so much ink has been shed, so much speculation indulged in vain; for even were it granted (and we deny the postulate) that any one theory of epidemics yet propounded is satisfactory, it is plain that all the rest must be erroneous—useless excrescences, of which we should be glad to rid our literature. We do not mean to assert, that all the theoretical views recently revived or invented are unworthy of notice. In the absence of direct demonstration, it is right to attach ourselves to probabilities while prosecuting an honest inquiry; but we must be cautious in forming, from probabilities alone, convictions which may afterwards, to our confusion, be overthrown by observation of facts.

There is one theory of epidemics which has of late gained ground considerably. It is that which supposes the seeds of disease to consist of vegetable germs or molecules—minute fungi, capable, in favourable circumstances, of rapid multiplication—capable, too, of lying in a dormant condition for months or years, and of then resuming all their potency in the production of disease—too small to be detected by our most improved optical instruments, too subtile for the tests of the chemist. This is the doctrine which the work of Mr Grove endeavours to illustrate. We must do the author the justice to state, that he has handled the subject with great elegance and considerable originality. Convinced himself of the truth of his theory, he labours to convince his readers by an accumulation of evidence in support of it. To certain portions of the evidence we propose very briefly to direct attention.

He first inquires,—"Whether it is probable that epidemic, endemic, and infectious diseases depend upon vital germs for their manifestations?" This question he, of course, answers in the affirmative, basing his opinion upon the very generally received doctrine, that specific morbid poisons, distinguished by the *essentially vital* power of self-multiplication, are the active agents in the production of variola, scarlatina, fevers, &c., and illustrating the

argument by reference to the opinions of Fuller, Schlegel, Coleridge, and more modern authorities. He adds :—

"Were it only on the fact of reproduction, I would be contented to take my stand, that the force of life is the indwelling power of pestilential matter. Reproduction is a law of nature, and the law of nature is the law of God. And where do we find He prevaricates with us? The more we study His laws the more harmony and perfection we find; what is seeming confusion in the ignorance of to-day, is order in the knowledge of to-morrow. If any one, ignorant of the law which regulates the diffusion of gases, were told that a heavier gas would ascend, contrary to its specific gravity, through the septum in a vessel containing a lighter gas above the heavier, he would naturally doubt your assertion, and say, 'that is contrary to the law of gravity;' but explain to him the principle by which this comes about, and the objects of the law, the order and beauty of the design become manifest. But this is no equivocation; it is evidence there, that subordinate laws exist, and nothing more. It has never been found that men have gathered 'grapes of thorns and figs of thistles;' nor has it ever been discovered that inanimate matter multiplies itself. The seed of disease 'is within itself,' multiplying and propagating itself; whether it formed a part of creation at the beginning or not, is rather a question to be solved by divines than physicians. When we know, however, the latency of seeds, and even of entire plants, and that they may be dried, and remain so for years, yet being brought again into conditions adapted to their active existence, they, as it were, revive from their sleep, and renew again their reproductive properties, can we wonder if, in the great scheme of nature, existences new to mankind should make their appearance? When the New Zealander saw the surface of his ground producing to him unknown plants, and the skins of his children generating peculiar eruptions, and each propagating its kind, would he look, think you, to the wood or the stones, the air or the water, for the solution of the mystery? No; he would naturally say these people brought the *seeds* with them. From the property of reproduction possessed by these forms of matter, we infer the value of the proposition."—Pp. 19-21.

In the absence of demonstrable truth, we cannot but admit that these speculations wear a certain air of probability. But in extending his argument—in tracing the analogies in the reproduction of animal and vegetable organisms—in his historical notice of various epidemics, including the plagues of Egypt, leprosy, black death, sweating sickness, modern plague, typhus, small-pox, cholera, &c. —in his attempts to account for the dispersion of plants and of diseases on the same principle, and to range epidemics with endemics in a common category, Mr Grove is not uniformly successful. We think he errs, and of course weakens his argument, by pursuing his analogies too far, and by assuming, as ascertained facts, mere expressions of individual opinion, resting on observations contradicted by negative testimony entitled to at least equal weight. Thus (at page 31) we find Mr Spooner's examinations of the contents of the small-pox and cow-pox vesicles alluded to, as remarkable fulfilments of a medical prophecy made in 1849, "that minute vegetable germs, in all probability all of the same type, were the agents producing epidemic and infectious diseases." It is stated with all confidence, that Mr Spooner introduced to the notice of the profession matters of disease, consisting "of minute cells not ex-

ceeding the 10,000th part of an inch in diameter," "resembling the yeast plant in appearance," and we are led to infer that these are the veritable seeds of small-pox. Now, in the first place, we are inclined to assign a low rank among the prophets to the great unknown who, in 1849, was delivered of this astounding vaticination. Centuries have elapsed since similar doctrines were promulgated; and if it were requisite to cite modern authorities, we should name the great German pathologist, Henle, and the learned American physician, Dr Mitchell, as the most original and philosophical expositors of this ancient theory. The so-called fungoid bodies of Mr Spooner are not new to microscopical observers; they have probably been looked upon with great indifference by hundreds of eyes armed with the best of optical instruments, and accustomed to their use. They possess no physical character by which we can distinguish them from the granules and small cells to be seen in ill-formed pus, among the debris of softened tubercle, &c.; and for aught that the microscope can teach, we might as reasonably assume the fluid in which these bodies swim to be possessed of the power of inducing small-pox, as endow the corpuscles themselves with the name and attributes of vegetable germs of disease.

"We may reasonably expect that these bodies, discovered by Mr Spooner, should be the elementary matters of disease. Their existence was predicted from the probability that living matter must be the agent; moreover, that this matter when discovered would be cellular, most probably resembling the yeast plant, as described by Mr Spooner.

"It was predicted that a planet would be discovered in a certain position in the heavens, because the perturbations of a comet indicated an attracting body in the path of the eccentric wanderer; the prediction and the fulfilment were almost simultaneous."—Pp. 32, 33.

Messrs Leverrier and Adams are not flattered by this comparison. The actual discovery of a new planet, suggested by the most refined exercise of mathematical and physical skill, is surely a very different matter from conjuring up a mere Will o' the Wisp.

It is a waste of ingenuity, and savours of the *bathos*, to speculate upon the fifth plague of Egypt, and on the cholera at Wandsworth, as in the following paragraphs:—

"Bible history tells us that Moses threw ashes of the furnace, which became a dust throughout all the land of Egypt; we cannot imagine that this, simply as ashes, could have caused the blain,—we may conclude that by some special miracle, the ashes were converted into a specific form of matter capable of inducing the effects recorded, or that an independent septic matter was generated for the purpose. If the latter, the act of throwing the ashes of the furnace into the air, may have been intended to signify, that the extremely minute division of the particles when thus cast into space, typified the inscrutable and hidden nature of the matter endowed with such marvellous properties. We are to understand, also, that some peculiar operation took place of a nature difficult to comprehend, which seems also to typify reproduction, for the handfuls of ashes which Moses threw into the air *became a dust in all the land of Egypt*, thus signifying an enormous reproduction of atomic matter."—Pp. 39, 40.

"Another coincidence, in connection with subsequent pestilences, arrests the attention, on the subject of the mysterious appearance on these occasions of

matter resembling dust being prevalent about the houses, and on the clothes of the people. Clouds also, and showers of dust-like particles, were not of infrequent occurrence. Indeed, in the summer of 1849, during the progress of the cholera, several phenomena of a similar nature were observed and authenticated; I myself can bear testimony to one instance of the kind. It was observed by many persons in my neighbourhood, after the passage of an ominous and lurid cloud, that as they walked their clothes became covered with a singular dust-like matter of very peculiar appearance. That this phenomenon was not destitute of significance may be gathered from the fact, that on the night of that day several severe cases of cholera occurred, though our village had been comparatively free for ten days."—Pp. 36, 37.

The latter passage in particular displays an amount of credulity which we should imagine was rarely to be met with among members of the medical profession. Are wreaths of vapour, dust-like particles, and *ominous lurid* clouds in the atmosphere, phenomena of such rare occurrence, that the fact of an epidemic cutting off a few additional victims, in a single instance, coincidently with their appearance, should connect them in our minds with the plagues of Egypt?

We object, also, to the fanciful and unnecessary comparison attempted between the leprous marks in woollen and linen garments described in Leviticus, and the appearances said to have been observed "from 1500 to 1503 in Germany and France during the prevalence of the sweating sickness."

"Spots of different colours made their appearance, 'principally red, but also white, yellow, gray, and black, often in a very short time, on the roofs of houses, on clothes, on the veils and neckerchiefs of women, &c.' Blood rain is also mentioned as having occurred at this time, which consisted of the aggregation of minute particles of red matter."—P. 38.

No part of Mr Grove's work is more distinguished for originality of thought and ingenuity of argument than the two last sections of the second chapter. We here find a succinct account of the various points in which the dispersion of the seeds of diseases, both endemic and epidemic, is presumed to resemble the extension of vegetable species from point to point of our globe. The author, of course, professes himself a contagionist *quand même;* at least we cannot otherwise interpret his recorded opinion, "that the history of the introduction of scarlet fever, hooping cough, lues, and other diseases into the various countries of the globe, is sufficiently convincing that men carry about with them the seeds of disease" (p. 77); "that cottons, woollens, furs, skins, &c., will retain the matter of infection for an almost indefinite period" (p. 75); and other passages containing an equally explicit confession of faith. No one, in fact, who adopts the vegetable hypothesis, can resist similar inferences. In Section II., there is developed a geological theory of disease, so wild, and yet so elegantly expressed, that we need make no apology to our readers for transferring it to our pages unabridged and unaccompanied with critical remarks:—

"We learn that originally this planet had a temperature much higher than at present, and that the variation of temperature between the equator and the

poles, which we now witness, did not obtain in the earlier condition of the globe. We are given to understand, and not without considerable proof, if not demonstration, that the earth was a vast bog, in which rank vegetation grew, and in which the ichthyosauri and plesiosauri must have floundered about as unwieldy and loathsome bodies. We can readily conceive a condition of atmosphere at this time to have been loaded with pestiferous vapours of an organized nature; it is entirely in accordance with all we know, that it should have been so. Allied forms of plants to those now in existence, are found in the form of fossils, by which comparisons are made, but how the transition into the present flora state took place, or at what period, it is impossible to say. That these plants should have been entirely destroyed during the revolutions of the earth by earthquakes, and their consequences; the collection of waters into the vacuities formed, and their draining off from other places by elevations of the land, is not to be dwelt on without astonishment; then again the ultimate changes of temperature on the surface of the earth, may have been another element in the history of their extinction. But if we may be allowed to imagine that there were organic germs floating in the vapours of the atmosphere, these would hardly be subject to the same influences as those which depended solely on their fixation to the soil for subsistence. The atmosphere, their native element, being influenced by the commotions from below, would be agitated; vortiginous currents would be established, hurricanes would sweep over the stagnant pool and reeking morass, and the higher regions of the air might have thus given protection to these subtile germs, while almost a total extinction of the elegant ferns, the stately palm, and the towering cane was in course of procedure. Then, when the strife of the earth and elements had subsided, these would descend with the gentle breezes, and again find in various spots a local habitation—

> 'Where blue mists, through the unmoving atmosphere,
> Scatter the seeds of pestilence, and feed
> Unnatural vegetation.'

"In the new era, when the earth took its present physiognomy, who shall say whether much of the pestiferous matter may not have been enclosed and condensed in the bowels of the earth; and when it is remembered that earthquakes and convulsions of nature,[1] have invariably preceded the outbreak of any great pestilences, that stinking mists coming from some unknown regions, and unusual vegetations, have made their appearance in concert at these times, what, I ask, is more natural than to imagine that they have been let loose during the general convulsion? It may be asked, what is to be said about that revolution of the earth, when the great deluge spread over the whole face of the globe? It can only be replied, that this is a part of the scheme of cosmogony into which we are not called upon to enter. There are yet strenuous supporters of the partial as well as total submersion of this planet, but whether it be true that the vast torrents which appear to have swept the surface uniformly in a southern direction, were of a date coeval with the deluge and constituted an essential portion of the phenomena, of which one was, that 'the fountains of the great deep were broken up,' or whether they were anterior to this catastrophe, will not at all interfere with the conjecture of a very early

[1] "Earthquakes have in all times been considered to have some connection with pestilences. 'A most grievous pestilence broke out in Seleucia, which from thence to Parthia, Greece, and Italy, spread itself through a great part of the world, from the opening of an ancient vault in the temple of Apollo, and that it raged with so much fury as to sweep away a third part of the inhabitants of those countries it visited.'—*Dr Quincy, on the Causes of Pestilential Disease.*"

"'Upon an earthquake the earth sends forth noisome vapours which infect the air; so it was observed to be at Hull in Yorkshire, by the Rev. Mr Banks, of that place, after a small earthquake there in 1703; it was a most sickly time for a considerable while afterwards, and the greatest mortality that had been known for fifteen years.' —*Anonymous*, 1769."

formation and propagation of the germs of pestilential diseases; for the commotions of a deluge were less likely to interfere with the vapours of the atmosphere, than extensive volcanic and electric disturbances. Moreover, it is rather in favour of this theory, that the regions where the temperature and exhalations most nearly resemble those of the former condition of the earth, are those in which pestilential disorders most frequently arise, and where their virulence has always been most strongly marked."—P. 64–68.

The third and fourth chapters of the work are chiefly occupied with observations on Liebig's chemical, and Henle's animalcular, theory of epidemics (both of which Mr G. considers unsound and untenable),—on the nature of those poisons which most resemble the morbid poisons in their effects on the body; and on the results obtained from the effects of remedial agents in proof of the hypothesis. On these subjects we shall, for the present, forbear to express an opinion.

We may observe, that even were the seeds of disease demonstrable objects which we could see and measure, little would be added to our knowledge of the laws of epidemics. We should be as much at a loss as at present, to explain the definite course, from one part of the world to another, which plague after plague has been observed to pursue,—to account for the fluctuations observed in the numbers attacked from day to day, and for the final extinction of an epidemic in any given locality,—for the protection against subsequent attacks, which variola, scarlatina, &c., undoubtedly afford; and for a host of other circumstances connected with epidemics, which seem likely long to baffle human wisdom. Our knowledge of the seed would avail us little, if unaccompanied with information as to the infinite number of causes which could influence its germination, multiplication, incubation, abortion, or destruction. But have we any *positive* proof of the existence of even the seed? "In all cases," says Liebig, "where the strictest investigation has failed to demonstrate the presence of organic beings in the contagion of a miasm, or contagious disease, the hypothesis that such beings have co-operated, or do co-operate in the morbid process, must be rejected as totally void of foundation and support."[1] We fully concur; and although we think it possible that renewed investigations and improved methods of observation may, at some future period, demonstrate the connection of epidemic disorders with animal or vegetable germs, we maintain that, in the present state of science, there is a total want of positive evidence in favour of such connection. All attempts to detect their supposed vegetable germs have signally failed. They cannot be ranged in the same category with scabies, tinea favosa, pitiryasis, &c., whose true pathology has been illustrated by modern microscopical observations. To take the single instance of cholera,—will any unprejudiced observer, whose testimony is worth invoking, assert that the corpuscles

[1] Liebig, quoted by Mr Grove, p. 174.

described in 1848-9 by the Bristol microscopists, Messrs Swayne and Brittan, are the successive developments of the germs producing Asiatic cholera? It is true that when the discovery was first promulgated, men, from whom better things might have been expected, did rashly commit themselves to some such doctrine, while the weekly medical press teemed with mistaken observations in corroboration of it. Grievous to such speculators must have been the report of the sub-committee of the London College of Physicians, which showed, by evidence indisputable, that the so-called cholera-fungus was a mere vision, and that its several manifestations consisted of torulæ, penicillium glaucum, starch, bran-cells, the uredo or bunt of wheat, epithelium, fragments of vegetables derived from the food, mineral matter or portions of infusoria from chalk mixture, &c., which had been all clubbed together under a common head, to suit and illustrate a preconceived theory. One form only, of the many figured by Mr Swayne, has not yet been clearly identified as an object familiar in nature; but as this is a large body, quite unlike the others, comparatively rare, and never seen at all in the course of a very patient investigation of the subject, undertaken in Edinburgh by Dr Bennett and Dr Robertson, it has obviously no connection with cholera, and its origin may, in relation to the vegetable hypothesis, be viewed with considerable indifference. The doctrine of cholera corpuscles has, for the present, received its *quietus*, and we are not inclined to predict for it a speedy resuscitation.

In concluding this notice of Mr Grove's treatise, it is but fair to state that, although in many points we must demur to the author's conclusions, as drawn from imperfect or insufficient evidence, there is much in his treatise well worthy of perusal, evincing a philosophical turn of mind, and suggestive of practical reflections, to which we would gladly refer if our space permitted. The graceful style in which the work is written, is, in our eyes, not its least merit.

A Systematic Treatise, Historical, Etiological, and Practical, on the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux Varieties of its Population. By Daniel Drake, M.D. Cincinnati: 1850. 8vo. Pp. 878.

This is a very remarkable volume, constituting, perhaps, the most extensive and able work on medical topography that has ever issued from the press. A description of the diseases peculiar to a tract of country beginning with the tropics and terminating with the polar circle, having an area of six million of square miles, and inhabited by numerous races of men, may well be considered a gigantic undertaking. Yet long journeys of observation, during a

period of forty years, and personal examinations carried through eighteen degrees of latitude and nearly as many of longitude, have enabled the author to publish the present work.

The contents of the book are so varied and extensive as to defy analysis. They include, 1st, The topographical and hydrographical etiology of this great valley, with a description of the cities and rivers it contains, illustrated with excellent maps; 2d, The nature, dynamics, and elements of the climate,—the temperature, atmospheric pressure, winds, electrical phenomena, distribution of plants and animals, &c.: this portion of the work is rich in very carefully and laboriously compiled tables; 3d, An interesting account of the population,—modes of living and domestic habits of the natives; lastly, An elaborate description of the various fevers which ravage this portion of the American continent. A second volume, with an account of the remaining diseases, will complete the work.

To the medical practitioners of the region described, Dr Drake's labours will prove invaluable. To the profession at large, his book constitutes a valuable addition to our knowledge of diseases, more especially of the manner in which these are modified by the topographical and hydrographical peculiarities of the North American continent.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

AUSCULTATION.

The object of auscultation is to ascertain and appreciate the nature of the various sounds which occur in the interior of the body. It has been found most useful when applied to the pulmonary and circulatory organs. Auscultation of the abdomen is occasionally serviceable, especially in certain cases of pregnancy, and during labour. It has also been applied to the head, although I have never been able to make out any useful results from the practice.

General Rules to be followed in the Practice of Auscultation.—1. Auscultation may be practised directly by applying the ear to the part, or indirectly through the medium of a stethoscope. Generally speaking, direct auscultation answers every necessary purpose except when the surface is unequal, or when it is desirable to limit the sounds to a small region, as during auscultation of the heart. In either of these cases a stethoscope is necessary. The instrument is also useful to confirm or nullify the existence of certain fine sounds which may be detected by the naked ear; to remove the head of the practitioner a respectable distance from the bodies of persons not distinguished for cleanliness; and lastly, as the most delicate method of auscultating the chest anteriorly in women. You should regard the stethoscope as a mere means to an end,—that end being, 1st, the education of the ear; and, 2d, a right appreciation of the pathological changes indicated by certain sounds.

2. In the choice of a stethoscope, you should observe, 1st, That the ear-piece fits your own ear; 2d, That the trumpet-shaped extremity is not above an inch and a-half in diameter, and is rounded so as not to injure the patient's skin when pressure is made upon it; 3d, That it is light and portable. The instruments recently made of gutta percha fulfil all these conditions.

3. In applying the ear, the surface should be covered only with a smooth piece of linen or a towel. In using the stethoscope, it should be applied to the naked skin, and held steady immediately above the trumpet-shaped extremity by the thumb and index finger, pressed down with tolerable firmness, whilst the second, third, and fourth fingers enable you to ascertain that the circular edge is perfectly applied—a circumstance absolutely essential.

4. The position of the patient will vary according to the part examined. In auscultating the lungs anteriorly the erect or recumbent positions may be chosen, when the two arms should be placed in a symmetrical position by the side. If the chest be examined posteriorly, the individual should lean somewhat forward and cross the arms in front. The practitioner, also, should choose such a position as will prevent too much stooping or straining. Generally speaking, the beds in the Infirmary are too low, and render auscultation very fatiguing to the physician.

5. Whenever individuals are thrown into such a state of agitation as to interfere with the regular action of the heart or lungs, the examination should be deferred until their fear diminishes, or the greatest caution exercised in drawing conclusions. Non-attention to this rule has led to many errors.

6. Before examining patients in a hospital, it is necessary that you should have made yourselves perfectly acquainted with the sounds which are continually going on in the healthy body. Omission of this rule not only renders the examination of patients useless, but betrays great want of consideration. For, as it is only from the alterations the healthy sounds undergo, or their being replaced by others, that we draw conclusions, how can this be accomplished if we are ignorant of their character in the first instance? It is expected, therefore, of every examining pupil, that he should be familiar with the character and theory of the various sounds heard in the healthy body before coming to the bedside. This study belongs to the Institutes of Medicine, rather than to that of Clinical Instruction.[1]

Special Rules to be followed during Auscultation of the Pulmonary Organs.—1. In listening to the sounds produced by the action of the lungs, we should pay attention to three things,—1st, The natural respiration; 2d, The forced or exaggerated respiration; and, 3d, The vocal resonance. For this purpose, having listened to the sounds during ordinary breathing, we direct the patient to take a deep breath, and then, still listening, we ask him a question, and during his reply judge of the vocal resonance.

2. You should commence the examination immediately under the centre of one clavicle,—and having ascertained the nature of the sounds and vocal resonance there, you should immediately listen in exactly the corresponding spot on the opposite side. The examination should be continued alternately from one side to the other, in corresponding places, until the whole anterior surface of the chest is explored. The posterior surface is then to be examined in like manner.

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[1] Before commencing the actual examination of patients, you should make one or more serious, careful, and prolonged examinations of the chest of one of your fellow-students in private, so as to familiarise your ear with the healthy laryngeal, tracheal, pulmonary, and cardiac sounds, and with the character of the vocal resonance, and of the cough, as heard in various parts of the chest. You should then listen in the same manner to the chest of a young boy of from five to eight years of age, and observe how clear and exaggerated the pulmonary sounds are. Then read the description of the healthy sounds, and the theory of their formation, in Barth and Roger—an excellent work, which has been translated into English by Dr Newbigging of this city.

3. When, in the course of the examination, anything different from the normal condition is discovered at a particular place, that place and the parts adjacent should be made the subject of special examination, until all the facts regarding the lesion be ascertained.

4. It is occasionally useful to tell the patient to cough, in which case we are enabled to judge, 1st, Of forced inspiration, as it precedes the cough ; and, 2d, Of the resonance which the cough itself occasions.

Of the Sounds elicited by the Pulmonary Organs in health and in disease.—I am anxious to impress upon you, that the sounds which may be heard in the lungs are like nothing but themselves. Students are too apt to take up erroneous notions from reading on this subject, and, instead of listening to the sound actually produced, fatigue themselves in a vain endeavour to hear something like the crackling of salt, the rubbing of hair, foaming of beer, or other noises to which these sounds have been likened. Preconceived notions frequently oppose themselves to learning the truth, and have to be got rid of before the real state of matters can be ascertained. Hence the great importance of obtaining your first impressions of the sounds to be heard by auscultation, not from books or lectures, but from the living body itself.

If you listen through your stethoscope, placed over the larynx and trachea of a healthy man, you will hear two noises,—one accompanying the act of inspiration, and the other that of expiration. These are called the *laryngeal and tracheal sounds or murmurs.* If you next place your stethoscope a little to the right or left of the manubrium of the sternum, you will hear the same sounds diminished in intensity. These are the *bronchial sounds or murmurs.* If now you listen under and outside the nipple on the right side, or posteriorly over the inferior lobe of either lung, you will hear two very fine murmurs. That accompanying the inspiration is much more distinct than that accompanying the expiration. By some, on account of its excessive fineness, it is stated that there is no expiratory murmur in health ; but this is incorrect. These sounds, then, are the *vesicular respiratory murmurs.* All these sounds become exaggerated during forced respiration, but in a state of health they never lose their soft character. Again, if you listen in the same places, whilst the individual speaks, you will hear a peculiar resonance of the voice, which has been called, in the first situation, *pectoriloquy ;* in the second, *bronchophony;* while in the third, it is scarcely audible. A knowledge of these circumstances, and a capability of appreciating these sounds, are necessary preliminary steps to the right comprehension and detection of the murmurs which may be heard during disease.

I have to suppose, then, that you have made your ears familiar with these sounds, and that you are acquainted with the present state of theory regarding their formation. This last may be stated in very few words to be, that the respiratory murmurs are occasioned by the vibration of the tubes through which the air rushes, according to well known acoustic principles. Hence they are loudest in the trachea, finer in the large bronchi, and finest in their ultimate ramifications. The vocal resonance, on the other hand, originates in the larynx; and diminishes or increases,—1st, according to the distance of any point from the source of the sound ; and 2d, according to the power which the textures have in propagating it.

If now you examine, in succession, any six of the cases in the wards which are labouring under well-marked pulmonary diseases, you will have no difficulty in recognising that all the sounds you hear may be classified into two divisions,—1st, alterations of the natural sounds ; 2d, new or abnormal sounds, never heard during health.

1. *Alterations of the Natural Sounds.*—All the sounds of which we have spoken, and which can be heard in the lungs during health, may, in certain diseased conditions, be increased, diminished, or absent ; their character or position may be changed ; and, with regard to the respiratory murmurs, they may present alterations in rhythm, or duration with respect to each other.

Alterations in Intensity.—Some persons have naturally louder respiratory murmurs than others; if this occur uniformly in both sides, it is a healthy condition. Occasionally, however, the sounds are evidently stronger in one place, or on one side (*puerile respiration*), generally indicating increased action of the lung, supplementary to diminished action in some other part. In the same manner, there may be feeble respiration simply from diminished action, as in feeble or old persons; but it may also be occasioned by pleurodynia, obstructions in the larynx, trachea, or bronchi,—pleurisy, or pulmonary emphysema, or exudations filling up a greater or less number of the air-cells and smaller tubes, as in pneumonia, phthisis, &c. Complete absence of respiration occurs when there is extensive pleuritic effusion or hydrothorax.

Alterations in Character.—The various respiratory murmurs may, in certain conditions of the lung, assume a peculiar harshness, which, to the ear of the practised auscultator, is a valuable sign, indicative of altered texture. Thus in incipient phthisis the vesicular murmur under the clavicle is often *rude* or *harsh.* In pneumonia the bronchial respiratory murmur presents a similar character. When ulceration exists, it becomes what is called *cavernous* (hoarse or blowing); and in certain cases of pneumothorax with pulmonary fistula, it assumes an *amphoric* character.

Alterations in Position.—It frequently happens that the sounds which are natural to certain parts of the chest, are heard distinctly where in health they are never detected. Thus, in pneumonia, *bronchial* or *tubular breathing*, as it is sometimes called, may be evident, where only a vesicular murmur ought to exist. This is often well marked with regard to the vocal resonance, as certain lesions, which occasion condensation or ulceration, will enable us to hear in parts where, under ordinary circumstances, no voice can be heard, either bronchophony or pectoriloquy.

Alterations in Rhythm.—In health, the inspiration is usually three times as long as the expiration. In certain diseased conditions this relation is altered, or even inverted. In incipient phthisis we often find the expiration unnaturally prolonged. In chronic bronchitis and emphysema it is three or four times longer than the inspiration.

II. *New or Abnormal Sounds.*—These are of three kinds; 1st, rubbing or friction noises; 2d, moist rattles; 3d, vibrating murmurs.

1. *Rubbing or Friction Noises* are caused in the pulmonary apparatus by some morbid change in the pleuræ, whereby, instead of sliding noiselessly on one another, they emit a rubbing sound. This may be so fine as to resemble the rustling of the softest silk, or so coarse as to sound like the creaking of a saddle, grating, rasping, &c.; and between these two extremes you may have every intermediate shade of friction noise. This variation in sound is dependent on the nature of the alteration which the pleuræ have undergone. If covered with a softened thin exudation, the murmur will be soft; if it be tougher and thicker, the sound will be louder; if hard, dense, and rough, it will assume a creaking, harsh, or grating character, &c. &c. These noises are heard in the various forms of pleurisy.

2. *Moist Rattles* are produced by bubbles of air traversing or breaking in, a somewhat viscous fluid. This may occur in the bronchi, when they contain liquid exudation, mucus, or pus, or in ulcers of various sizes. They may be so fine as to be scarcely audible (when they have been called *crepitating*), or so coarse as to resemble gurgling or splashing, when they have received the name of *cavernous.* Here again, between these two extremes, we may have every kind of gradation, to which auscultators have attached names, such as *mucous*, *submucous*, *subcrepitating*, &c. &c. With these names you need not trouble yourselves; all that it is important for you to recognise is, that the sound be *moist*, and you will easily recognise that the rattles are coarse or large, in proportion to the size of the tubes or ulcers in which they are produced. These rattles may be heard in pneumonia, phthisis pulmonalis, bronchitis, pulmonary apoplexy, &c. &c.

3. *Dry Vibrating Murmurs* arise when the air-tubes are obstructed, constricted, or lose their elasticity and become enlarged, whereby the vibrations into which they are thrown by the column of air, produce sounds or tones of an abnormal character. Hence murmurs may be occasioned of a fine squeaking (*sibilous murmur*), or of a hoarse snoring character (*sonorous murmur*), and between the two extremes, there may be all kinds of variations, to which ingenious people have applied names. These only cause confusion ; all that is necessary, being to ascertain that the murmur is *dry*, and you will readily understand that the fineness or coarseness of the sound will depend on the calibre of the tube or cavity thrown into vibrations. They are usually heard in cases of bronchitis and emphysema. Occasionally they present a blowing character, as when ulcers are dry, which often occurs in phthisis.

The *vocal resonance*, besides undergoing the changes already noticed in intensity, character, and position, may give rise to abnormal sounds. Occasionally it presents a soft reverberating or trembling noise, like the bleating of a goat (œgophony). The value of this sign, as indicative of pleurisy, was much overrated by Laennec. At present it is little esteemed. Sometimes the resonance gives rise to a metallic noise, like dropping a shot into a large metallic basin, or the note produced by rubbing a wet finger round the edge of a tumbler or glass vessel. This is often best heard immediately after a cough, in cases of pneumothorax, or large tubercular excavations of the lung. Œgophony is supposed to be produced, when a thin layer of serous fluid between the pleuræ is thrown into vibrations. The cause of metallic tinkling has created great discussion ; but Drs Spittal and Skoda have shown that the existence of air in a cavity which is thrown into vibrations is the necessary condition.

Such, then, are the principal sounds which may be heard by auscultation of the pulmonary organs in health and during disease. Many writers have endeavoured to point out their diagnostic importance, and drawn up rules which have always appeared to me much too arbitrary. Indeed, in so far as the education of medical students is concerned, I have long been persuaded that the study of these rules has retarded their powers of diagnosis, and afterwards led to dangerous errors in practice. I know of no dogma, for instance, more mischievous than the one which asserts a crepitating (that is a fine moist) rattle to be pathognomonic of pneumonia, because it is just as common in phthisis, and is frequently heard in various other lesions of the pulmonary organs. Hence we should regard a crepitating rattle, not as indicative of this or that so-called disease, but simply of fluid in the smaller air-passages ; increased resonance of the voice, as indicating hollow spaces with vibrating râles, or increased induration of the pulmonary textures, and not as diagnostic of phthisis, pneumonia, &c., and so on. I wish, then, strongly to impress upon you,—

1st, That the different sounds are only indicative of certain physical conditions of the lung, and in themselves bear no fixed relation to the so-called diseases of systematic writers.

2d, No single acoustic sign, or combination of signs, is invariably pathognomonic of any certain pathological state,—and conversely, there is no pathological state which is invariably accompanied by any series of physical signs.

3d, Auscultation is only *one* of the means whereby we can arrive at a just diagnosis, and should never be depended on alone.

Special Rules to be followed during Auscultation of the Circulatory Organs. —1. In listening to the sounds produced by the action of the heart and arteries, we should pay attention,—1st, to the impulse ; 2d, the character and rhythm of the sounds ; 3d, the place where they are heard loudest, and the direction in which they are propagated.

2. You should commence the examination by feeling for the spot where the apex of the heart beats against the walls of the chest, which will enable you

to judge of the impulse. This ascertained, place your stethoscope immediately over it, and listen to the sounds. Then place the instrument above, and a little to the inside of, the nipple, near the margin of the sternum, and listen to the sounds there. In the one situation you will hear the first or systolic sound, in the other the second or diastolic sound loudest.

3. If anything different from the normal condition be discovered in either one or the other position, or in both, they should be again carefully examined, and by moving the stethoscope below and round the apex of the heart, or above, in the course of the aortic arch or carotids, on the right and left side, &c. &c., it should be ascertained at what point, or over what space, the abnormal sounds are heard loudest, and whether they be or be not propagated in the course of the large vessels. Occasionally listening over the back and in the course of the descending aorta may be useful.

4. When, during the above examination, we discover a new source of impulse and of sound in one of the large vessels, this must be especially examined, the limits of such impulse and sound carefully ascertained,—whether they be or be not synchronous with those originating in the heart,—their direction, &c.

5. Under ordinary circumstances, the respiratory do not interfere with the detection of the cardiac sounds; but where the former are very loud and the latter indistinct, it is useful to direct the individual to hold his breath for a few moments. Sometimes the impulse and sounds of the heart are heard better by directing the patient to lean forward; they may also, if necessary, be exaggerated and rendered more distinct by directing him to walk up and down quickly, or make some exertion for a short time.

Of the Sounds elicited by the Circulatory Organs in Health and Disease.—On placing your ear over the cardiac region in a healthy person, you will feel a beating, and hear two sounds, which have been likened to the tic-tac of a watch, but to which they bear no resemblance. They may be imitated, however, very nearly, as pointed out by Dr Williams, by pronouncing in succession the syllables *lupp*, *dupp*. The first of these sounds, which is dull, deep, and more prolonged than the second, coincides with the shock of the apex of the heart against the thorax, and immediately precedes the radial pulse; it has its maximum intensity over the apex of the heart,—below and somewhat to the outside of the nipple. The second sound, which is sharper, shorter, and more superficial, has its maximum intensity nearly on a level with the third rib, and a little above and to the right of the nipple—near the left edge of the sternum. These sounds, therefore, in addition to the terms first and second, have also been called inferior and superior, long and short, dull and sharp, systolic and diastolic,—all which expressions, so far as giving a name is concerned, are synonymous.

The two sounds are repeated in couples, which, if we commence with the first one, follow each other with their intervening pauses, thus—1st, There is the long dull sound coinciding with the shock of the heart; 2d, There is a short pause; 3d, The short sharp sound, and 4th, A longer pause,—all which correspond with one pulsation. In figures, the duration of these sounds and pauses by some have been represented thus,—the first sound occupies a third, the short pause a sixth, the second sound a sixth, and the long pause a third. Others have divided the whole period into four parts; of which the two first are occupied by the first sound, the third by the second sound, and the fourth by the pause. The duration, as well as the loudness, of the sounds, however, are very variable even in health, and are influenced by the force and rapidity of the heart's action, individual peculiarity, and form of the thorax. Their extent also differs greatly. They are generally distinctly heard at the precordial region, and diminish in proportion as we withdraw the ear from it. They are less audible anteriorly on the right side, and still less so posteriorly on the left side. On the right side posteriorly they cannot be heard. Their tone also varies in different persons; but in health they are free from a harsh or blowing character.

Great diversity of opinion has existed regarding the causes of these sounds,—all of which you will of course have heard discussed before coming here. You must never forget, however, the cardiac actions which coincide with them; for our reasoning from any changes we may detect, will entirely depend upon our knowledge of these. We may consider, then, that there coincide with the first sound,—1st, The impulse, or striking of the apex against the thoracic walls; 2d, Contraction of the ventricles; 3d, Rushing of the blood through the aortic orifices; and 4th, Flapping together of the auriculo-ventricular valves. There coincide with the second sound,—1st, Rushing of the blood through the auriculo-ventricular valves; and 2d, Flapping together of the aortic valves. Contraction of the auricles immediately precedes that of the ventricles. The result of numerous pathological observations, and of many experiments, is that, in health, the first sound is produced by the combined action of the auriculo-ventricular valves of the ventricles, and of the rushing of the blood, which sound is augmented in intensity by the impulsion of the heart's apex against the thorax; whereas the second sound is caused only by the flapping together of the sigmoid valves.

With the cardiac as with the respiratory sounds, the alterations which take place during the disease may be divided into,—1st, Modifications of the sounds heard in health; 2d, New or abnormal sounds.

I. *Modifications of the Healthy Sounds.*—These refer to the variations the healthy sounds present in their seat, intensity, extent, character, and rhythm.

Seat.—The sounds may be heard at their maximum intensity *lower* than at the points previously indicated, as in cases of dilated hypertrophy of the left ventricle, enlargement of the auricles, or of tumours at the base depressing the organ. They may be *higher*, owing to any kind of abdominal swelling pushing up the diaphragm. They may be more on *one side* or the other, in cases where the heart is pushed laterally by effusions of air or fluid in a pleural cavity. Various other circumstances may also modify their natural position, such as tumours in the anterior or posterior mediastinum, aneurisms of the large vessels, adhesions of the pericardium, deformity in the bones of the chest, &c. &c.

Intensity and Extent.—These are *diminished* in cases where the heart is atrophied or softened; when there is pericardial effusion, concentric hypertrophy of the left ventricle, or emphysema at the anterior border of the left lung. They are *increased* in cases of dilated hypertrophy, of nervous palpitations, and when neighbouring portions of the lung are indurated, especially in certain cases of pneumonia and phthisis pulmonalis.

Character.—The sounds become *clearer* or *duller* than usual, according as the walls of the heart are thinner or thicker. Occasionally they sound *muffled* in cases of hypertrophy or softening of the muscular walls. Not unfrequently there is a certain degree of *roughness*, which is difficult to determine as being healthy or morbid. Occasionally it ushers in more decided changes; at others, continues for years without alteration.

Rhythm or Time.—I need not say that the frequency of the pulsations differs greatly in numerous diseases altogether independent of any special disease in the heart. In certain cardiac affections, however, the beats are *intermittent*, in others *irregular*—that is, they succeed each other at unexpected intervals. The *number* of the sounds also varies. Sometimes only one can be distinguished, it being so prolonged as to mask the other. Occasionally three or even four sounds may be heard, depending either on reduplication in the action of the valves when diseased, or on want of synchronism between the two sides of the heart. Not unfrequently the increased and irregular movements of the organ, combined with the sounds, are of such a character as to receive the name of *tumultuous*.

II. *New or Abnormal Sounds.*—These are of two kinds: 1st, Friction murmurs; 2d, Blowing or vibrating murmurs. Dr Latham has called them *exocar-*

dial and *endocardial.* I am in the habit of denominating them *pericardial* and *valvular.*

Pericardial or Friction Murmurs.—These murmurs are the same in character, and originate from the same causes, as the friction noises connected with the pulmonary organs. It is only necessary to observe, that occasionally they are so soft as closely to resemble blowing murmurs, from which they are only to be distinguished by their superficial character and limited extent.

Valvular or Vibrating Murmurs.—These murmurs vary greatly in character,—some being so soft as to resemble the passage of the gentlest wind; others are like the blowing or puff from the nozzle of a bellows (*bellows murmurs*); whilst others are harsher, resembling the noise produced by *grating, filing, sawing,* &c. They are all occasioned, however, by diseases interfering with the functions of the valves. Sometimes these do not close, and the blood consequently regurgitates through them; at others, whilst this is the case, they are constricted, indurated, roughened, and even calcareous,—whence the harsher sounds. They may be single or double, and have their origin either in the auriculo-ventricular or arterial valves, or in both at once,—the detection of which constitutes the diagnosis of the special diseases of the organ. Occasionally these sounds resemble *musical notes,* more or less resembling the cooing of a dove, singing or twittering of certain small birds, whistling, tinkling, &c. &c. These depend either upon excessive narrowing of the orifices, or upon any causes which induce vibrations of solids in the current of blood,—as, when there are perforations in the valves, irregularities of their margins, string-like or other shaped exudations on their surface, &c. &c.

Auscultation of the large vessels.—On listening through the stethoscope placed over the arteries in the neighbourhood of the heart, we hear the same sounds as are produced at the sigmoid valves, propagated along its course, but more indistinct as we remove the instrument from the base of the heart. Those which are more distant have only one sound, which is synchronous with their impulse and their dilatation. This sound is of a dull character, but in health always soft.

In the various conditions of disease we have a single or double bellows sound, or it may be harsh, grating, rasping, &c. In the first place, you must ascertain whether any of these sounds are propagated along the artery from the heart, which you will know by listening over its course from that organ, and observing whether they increase as you proceed towards it. If the sound have an independent origin, it may originate from disease of the internal surface of the artery, when it will be harsh in proportion to the roughness; from stricture of, or pressure on the vessel, or from its dilatation. Generally speaking, the more dilated and superficially seated the vessel is, the sharper is the sound. Sometimes there is a double murmur in the course of a vessel, having an undoubted independent origin. This is most common in cases where there is an aneurismal pouch, into which the blood passes in and out through an opening narrower than the swelling itself. Occasionally one or both such murmurs may possess somewhat of a metallic ringing, or even musical character, when the margins of the opening are probably tense and thrown into peculiar vibrations.

I have already told you never to form a conclusion from auscultation alone. Even when combined with percussion, it is not safe to form a diagnosis without a knowledge of *all* the circumstances of the case. Hence, why I repudiate those rules which have been published in books, that have for their object the establishment of opinions from physical signs alone. At the same time, there can be no doubt that percussion and auscultation are absolutely essential to the proper investigation of maladies, although not more so than other modes of inquiry. I have, therefore, thought it best to give you a condensed resumé of

the sounds which may be heard by auscultation of the lungs, heart, and large vessels, pointing out a few of the diseased states in which they may be sometimes (not always) heard, and especially indicating the physical conditions on which they are supposed to depend. Their true diagnostic value can only be learned by the careful examination of individual cases.

LECTURES ON GENERAL THERAPEUTICS.

By Douglas Maclagan, M.D., F.R.S.E.

(*Continued from page* 272.)

CHEMICAL REMEDIES—CAUSTICS—CAUTERIES.

They may be used for their mere primary or chemical effect to destroy texture, and in this view they have been applied practically to the following purposes :—

To open abscesses.—There can be no doubt that in nineteen cases out of twenty the proper way to open abscesses is by the knife, as it is equally effectual, more expeditious, and less painful than caustic ; and where the matter is at all deeply lodged, in short, where there is any more over it than integument, escharotics as a means of opening it are out of the question. The principal indication for the use of the caustic (the cautery is never used for this purpose) is where a large portion of the skin has become attenuated and discoloured, all of which must be got rid of before a healing sore can be formed ; and this can be more readily accomplished by the destructive action of the caustic than by trusting the case to the slower process of ulcerative absorption. I do not know that I can state this indication more precisely ; but what I have generally been guided by is this—if the attenuated skin is of a circular form, so that when divided by the knife in any direction it must leave broad thin flaps, which will be long of ulcerating away, I then employ the caustic, though I must say that occasions for preferring it to the knife are rare. It is not requisite to rub the caustic over the whole surface of the skin which we wish to destroy ; it is sufficient to apply it round the edge where the healthy and the thin skin meet, and the whole of the latter being, as it were, included in a ring of eschar, will perish. This plan answers very well in large buboes, especially in scrofulous subjects, where the skin is apt to partake largely of the unhealthy character. When, however, the attenuated portion of skin, though large, is long and narrow in shape, the knife is to be preferred ; here you will not be troubled with broad flaps of unhealthy skin ; and, remember that, by using the knife, you always give your patients much less pain, though patients will not always be persuaded of this till they try it, and will sometimes, from a foolish horror of anything that savours of an "operation," allow you to inflict upon them a great deal of unnecessary suffering, rather than permit you to relieve them in a few seconds by the bistoury. If the abscess has opened spontaneously, and there is much unhealthy skin, and also unhealthy glandular or cellular tissue below it, the caustic had better be used at once, as all this has to be got rid of before a cure can be accomplished. Under such circumstances, thrust the piece of caustic into the opening, turn its point up so as to rub the skin on the inside, thrust it once or twice into the unhealthy tissue beneath, and you will thus effectually destroy all that requires to be removed.

To destroy morbid growths.—This is not the proper place for discussing what forms of morbid growths may be treated by extirpation, and what are the circumstances under which we ought to refrain from interfering surgically, and restrict ourselves to palliative treatment. When, however, in accordance with surgical principles, it has been determined that an attempt at removal shall be made, three methods of operating present themselves,—mechanically, by the knife or by ligature, and chemically by cauterants. I think that it may be laid

down as a general rule that, if we except such matters as warts and corns, which fall generally under the domain of domestic surgery, there are but few cases where we can recommend the cauterant as a substitute for the knife in the removal of morbid growths. It has no advantage in certainty of effect; it is not so safe as the knife is in the hands of a person of proper dexterity, and competent anatomical knowledge; the wound left by it, when its proper action is over, will not heal so readily as that made by the scalpel; and, therefore, ever since surgeons became more fortified with means of arresting bleeding from wounds, the cauterants have been very generally superseded by the knife, and in some cases the ligature, for the removal of morbid growths. There still, however, are found in surgical works, and still more in empirical surgical practice, statements to the effect, that some forms of local disease,—as, for instance, open cancer,—may be removed by caustic, where the knife cannot be employed. I think no one can hesitate to say that this is against all true surgical principle. When a disease, either from its nature or its situation, is unfit to be subjected to the knife, *à fortiori* it is unfit for extirpation by an agent which must of necessity be more or less uncertain in the extent of its destructive operation; and although, perhaps, now and then a malignant-looking sore has been healed in this way, yet in by far the greater majority of instances the disease is either directly aggravated, or only very partially benefited—and above all, it is obvious that no proceeding of this kind can give any better chance to the patient of exemption from the recurrence of the disease, or its attacking neighbouring parts, which is the great impediment to operative interference. I have, for instance, seen small cancerous ulcers of the lip cured by caustic. I saw some of these treated in this way by chloride of zinc when it was new as a caustic, and, therefore, for a while fashionable, and esteemed wonderful. I have also seen some of these sores of the lip which were treated by a druggist's shopman in the country. I had no opportunity of tracing any of his cases from beginning to end; but I mention this man's practice both on account of the destructive agent used by him—powdered arsenic—which, however, is perhaps hardly a true caustic,—and the means which he employed to prevent the poison getting into his patients' mouths. He used bird-lime for this purpose, making a little hollow disk, into which he put the arsenic, and stuck it firmly on the part, leaving it in contact for about six weeks, when a slough came away and the ulcer healed. Of course, I do not recommend this practice for your adoption; not that it was unsafe as regarded the arsenic, for I never could ascertain that any of this man's patients were ever affected with symptoms of poisoning, but that, in reality, there was nothing in its success, such as it was, to recommend it; for a V shaped incision would have removed and cured in a few days any of the cases which I saw; whereas, when he did succeed, he took at least two months to effect it. I have also seen the actual cautery applied to malignant ulcers under the auspices of a high surgical authority in a continental school, and the clinical lesson which I then got was one which will serve me for the rest of my life. I saw three cases of open cancer deeply burned with the cautery,—a proceeding horrible enough to witness, but still more so when the result was seen. All three died from this operation, and two at least of them were affected with purulent deposits in internal organs.

It may, however, be legitimate to use the caustic in malignant disease, where the tumour, from its mere bulk or situation, is such an annoyance to the patient that the surgeon removes it so far as he can as a palliative measure. He may then apply a caustic to the base of the tumour, either to arrest hemorrhage, if troublesome; or, if he is compelled to leave part of the tumour, to prevent its speedily growing again, and keep it in check. I have seen such cases occur, but they are very rare, and must be looked upon as deviations from ordinary surgical rule for a special palliative purpose.

There is one form of morbid growth to which the caustic is generally held to be particularly applicable. I allude to scrofulous tumours, especially when

they have passed on to that unhealthy suppuration, which frequently occurs in the course of them, and which leaves fistulous sores or flabby ulcers surrounded by hardened scrofulous tissue, in which it is very difficult to produce any approximation to a healing action. In such a state of matters destruction of the diseased tissues by caustic is an established mode of treatment. When it occurs in the ordinary lymphatic glands, in situations where a cicatrix will not cause much deformity, the caustic locally, with constitutional treatment, answers well; but when we wish to avoid having a large cicatrix, as on the neck, we eschew caustics, and try to obtain a less deforming, though more tedious cure, by stimulant applications, conjoined with constitutional remedies. When more important glandular organs are affected, as the testis and mamma, the caustic freely applied will eventually produce a cure; but it has appeared to me, that in many such cases, where the organ has been extensively invaded by the disease, even though we feel confident that a cure will ultimately follow, it would be far better to have recourse to the knife. I have seen, for instance, a scrofulous mamma in an elderly woman, the subject of consultation. It has been urged, and with truth, "That should heal sooner or later; apply the potash freely, and you will cure it." But, in such a case, what is the ultimate result?—a cure, no doubt, but at the expense of much suffering to the patient, and ultimately leaving a breast which, even if the woman were within the child-bearing period of life, would never be of any use to her as a nurse. It appears to me that, when we can cure such a disease with caustic only by leaving the useless remains of a mamma behind, it is a shorter process, and one entailing no greater ultimate injury, to excise the diseased organ. The same applies to the testicle when it is extensively diseased, and is more frequently acted upon with regard to it; and it also holds true of scrofulous disease in other external parts. It is true that an operation is to be regarded as the last resource of surgery, and should not be undertaken when there is a reasonable prospect of a cure without it. It is also true, that even very bad cases of external strumous disease may be cured by caustic; but it is always to be considered, in following this practice, whether, what we save by such a cure is to be of any use to our patient, and whether it will ultimately be worth all the protracted suffering which has been undergone in obtaining the result.

There are some forms of local disease which have no pretensions to be called malignant, but which are very intractable, being slow to heal and apt to spread, which it would be well if we could extirpate bodily, and substitute for them a clean wound, but which, from the extent of the surface involved, and the situation of the parts affected, admit of no such proceeding. The troublesome disease of the face called lupus, and some of the forms of chronic ulcer, belong to this category. To these cases caustic is generally applicable; the object in using it, however, being, not to extirpate the diseased textures, but rather to alter the action of the parts, and thus lead to the formation of a sound cicatrix. The absorption of solid matter effused into and below the skin is probably one of the chief beneficial effects of the application of the caustic to such cases;—the indurated edges and base of the ulcerated portion are thus removed, and cicatrisation can then go on.

There is one form of morbid growth which I think may be treated with the cautery more than it usually is,—I mean the erectile tumours of the skin in children. These, which consist essentially of a congeries of enlarged blood-vessels, are commonly treated by ligature; but this causes a good deal of suffering, and often much constitutional irritation, before the strangulated mass finally separates. The method of applying the cautery is to pass a large red-hot surgical needle, which may be readily heated by a gas flame, once or twice through the base of the tumour, and the object is to cause effusion of lymph, and thus to alter the structure of the tumour, and arrest its growth. There are other means of effecting this object; but I mention this as one which, though noticed in surgical works, has not received that attention which I think it deserves.

To complete the Destruction of Sloughing Textures.—Thus, in phagedenic ulceration, as in cancrum oris, phagedenic chancre, and the bad action which so often attacks sores in hospitals, thorough cauterisation of the parts ought to be had recourse to promptly. It not only expedites the separation of the sloughs which have already formed, but, by a kind of allopathic action, substituting a chemical for a morbid vital process, it arrests the progress of the affection, and gives a healthy character to the sore. In these cases the fluid caustics, such as strong nitric acid, are the best, as they penetrate into all the nooks where the diseased action has established itself.

To prevent the Effects of Poisoned Wounds.—Cauterants are also used on account of their chemical agency in cases of poisoned wounds, and in the early stages of chancre, which in this respect is regarded by many as a kind of poisoned wound. According to Ricord and others, if what is called a true venereal chancre is thoroughly cauterised during the first three or four days, constitutional symptoms never follow; and this is explained by saying, that the surface secreting the poison is thus destroyed, and the virus is prevented from getting into the system. Whether this be the true explanation or not, I need not stop to discuss; there can be no doubt that free cauterisation is the best treatment for primary sores, whether during the first three or four days or later, and all the more so if the edges and base be indurated; and when the slough separates we generally have a healthy sore left, which heals under simple treatment.

With regard to poisoned wounds, as those from dissecting punctures, and by the bites of snakes and rabid dogs, there appears to be good reason to think that the caustics are useful, both by their chemically destroying the organic poisonous matter, and by their cauterant action closing up the vessels of the part, and thus preventing absorption. The use of the caustic does not supersede the necessity in such cases for trying to prevent the ingress of the poison into the system by suction of the part, encouraging it to bleed, or by excision of the whole tract of the wound. The fluid caustics have been recommended as being likely to reach thoroughly to the bottom of these punctured wounds; but, if the injury be of such a form, it is better to dilate it freely, and thus get room for applying a solid caustic efficiently over every portion of the wound.

To arrest Hemorrhage.—I have already told you, that when a cautery or caustic is applied to a raw surface, such as a wound, there is coagulation of the blood or other fluids on the surface, and that the subsequent effects are the same as when the chemical agent is applied to unbroken skin,—that is, there is shrivelling up of the texture, the formation of a slough, subsequently the occurrence of inflammation in the contiguous parts, and lastly separation of the slough. In the arrestment of hemorrhage by the cautery or caustic, all these actions come more or less into play; first the blood on the surface is coagulated, and thus so far forms a mechanical impediment to its further escape from the vessels; but this would be of little avail were it not for the further action of the cauterant on the divided vessels themselves, which, becoming shrivelled and contracted, offer a still further impediment to the flow of blood. Most probably the blood in the vessels of the cauterised part is coagulated also; and thus the primary effect of the cauterant appears to be, to form a clot both without and within the vessel, at the same time that it contracts the open orifice, and thus the flow of blood becomes arrested. But it is clear that so soon as the eschar separated, the bleeding would be renewed, did not the cauterant produce a further effect, which renders the closure of the vessel permanent; this is the inflammatory action which takes place in the textures contiguous to the cauterised portion, and which, leading to the effusion of fibrinous matter, completes the cure by permanently shutting up the vessel,—and thus, when the slough separates, there is left merely an ordinary suppurating sore, and no further bleeding occurs. The whole point, then, on which the efficiency of the cautery or caustic, or indeed any of the other means of arresting bleeding,

depends, is,—whether the temporary occlusion of the divided vessels will be able to hold its ground till they have been permanently closed by vital action? Now, in regard to the larger arteries, it is found that this is not the case; the coagula of blood formed, and the corrugation of the orifice of the vessel caused, by the chemical agent, are not sufficient to resist the impulse of the blood; and, therefore, even were there not other objections to its employment, the cauterant would be deservedly rejected, on the score of inefficiency, as a general means of arresting bleeding from large vessels. I presume that few of you need to be told, that for this purpose the ligature is the truly efficient and trustworthy resource of the surgeon, and that in all cases where a vessel can be laid hold of and tied, this is the course to be pursued. When this cannot be done—as, for instance, where the bleeding vessel cannot be brought into view, or where the blood is escaping, not from one or two large, but from a multitude of small, vessels—pressure is had recourse to; and when we can employ neither pressure nor ligature, we then may have recourse to certain applications which will produce temporary arrestment of the bleeding, and keep it in abeyance till permanent closure of the vessels occurs. Such applications are called styptics. Some of these, such as cold, act vitally, by producing a constriction of the vessels; some, as alum and other astringents, act partly vitally, and partly, probably, by chemically coagulating the escaping blood,—and in slight forms of bleeding this means will often succeed; but when we are deprived of the use both of ligature and pressure, and when the hemorrhage is too active to be controlled by the ordinary styptics, we then have recourse to the more energetic action of the cautery or caustic, as described above.

It is not requisite for me to specify particular cases to which this is applicable—an example will suffice. Take the instance of leech-bites on a child's throat. Here we have no large vessel bleeding, but a number of small open orifices; the ligature is not applicable in the ordinary way,—the situation of the bleeding wounds, of course, prevents our making anything like efficient pressure; we may try cold and other astringents, and in the very vascular skin of a child will probably find them to fail; and we then have recourse to our caustic, which will in general be found to succeed. Or take the instance of hemorrhage from the jaw, after extraction of a tooth. There is probably a pretty active vessel bleeding, but, being at the bottom of a narrow cavity in a bone, it cannot be laid hold of and tied; pressure may be tried, but the patient will not suffer it to be maintained, from the pain which it occasions in the lacerated gum; we may then have recourse to our caustics, and a liquid one, such as the acid nitrate of mercury, will be advantageous, as it will more readily penetrate to the bottom of the cavity; and, should this fail, we may then employ the actual cautery, using for the present case such an instrument as a red-hot wire.

In using cauterants for the arrestment of hemorrhage, one or two particulars require to be attended to. All clots should be removed from the wound,—it should be cleared of blood by a few moment's pressure with a bit of dry lint,—and the cauterant immediately applied. By this means we secure the advantage of exposing as much as possible the very point from which the blood flows, and thus we can apply the cauterant with more precision; and, by having the wound free of blood at the moment of its introduction, we do not expend its chemical action on escaped blood, but carry it at once and directly to the bleeding vessels.

In cauterising to arrest bleeding from a cavity, as a leech-bite or the alveolus of the jaw, it is obviously essential that the caustic or cautery penetrate to the very bottom of the wound; for the chief source of the hemorrhage is most probably in this quarter; you ought, therefore, to be sure, before applying your caustic or cautery, that it can reach the very bottom of the cavity. I have, in the case of leech-bites in children, seen a piece of caustic applied to the wound, cause much pain, and yet leave the bleeding as active as ever, simply because the piece of caustic was too thick to reach the bottom. Though it is

a matter of individual detail, I may mention that, in cauterising leech-bites, I have often had recourse to a simple and ingenious way of making a fine piece of caustic, which was recommended by my late friend Dr James Hunter,—viz. to break off a piece of nitrate of silver from a common stick of caustic, crush it to powder, heat the point of a silver probe in a candle, and dip it into the powder; it thus becomes crusted with the nitrate, and forms a fine button of caustic, which will easily reach the bottom of any such wound. Lastly, when the bleeding has been stopped, beware of injudiciously meddling with the part. Remember that the occlusion of the divided vessels is as yet only temporary, and may be easily overcome, till the vital processes have been perfected which make the vessels permanently secure. Therefore, let the part be touched as little as possible, and wait patiently for the spontaneous separation of the slight slough which is formed.

Such are the uses and applications of the cautery and caustics, which are founded upon their mere chemical and destructive effect. We saw that the second stage in their action is the production of an ulcer with suppurative discharge. This, as a matter of course, follows the application of cauterants for any object; but sometimes we make use of them to obtain this very result for certain purposes. This is when we wish to employ them as counter-irritant agents, and when we desire to keep up the counter-irritation for some time by the establishment of an issue. As we shall, however, have to consider the whole subject of counter-irritants hereafter, I reserve for a future occasion what I have to say respecting the use of cauterants for this purpose.

There remains, then, only one more of the effects of cauterants which has been turned to useful purposes, viz., their power of forming a contractile cicatrix, and on this point a very few words will suffice.

This effect of a burn, which, in cases of accidental injury, often results in very serious deformity, has been applied by surgeons to procure a closure of preternatural openings, which, having become callous on the edges, have lost all tendency to contract naturally, such as vesico-vaginal fistula, preternatural anus, venereal ulcers which have perforated the soft palate, &c. The object which is here in view is to produce a slight artificial burn on the edges of the opening, to allow this to heal, and to gain the advantage, which the contracting tendency of the cicatrix affords, for diminishing the size of the aperture.

It is obvious that to do this successfully, the cauterant must not be applied to such an extent as to produce destruction of tissue, because, if a considerable portion of the edges of the opening were burned away, the sore, when it did cicatrise, would have to compensate, by its contraction, for this loss of tissue, and would not have improved the original malady, nay, might rather aggravate it. In attempting, therefore, to procure the closure of such openings by cauterisation, all that is to be done is to produce enough of inflammation of the edges to cause them to become raw and sore. This inflammation, which is accompanied by some swelling of the parts, often produces at first an apparent direct amendment—the passage of the urine into the vagina in one case, and the escape of fæces in the other, being lessened for a while. But the surgeon knows that this is only temporary; after a day, or even in a few hours, this inflammatory swelling subsides, the escape of fluid is as great as before, and the patient is disappointed. But the surgeon knows that it is not till after this that he is to look for the expected benefit. The sore made by the cauterant is allowed to heal, which it will take some time to do, and now it manifests a tendency to contract, and the orifice, though not closed, is lessened. A cure is not to be obtained, and must not be looked for, from one cauterisation; the same process must be repeated again and again; but this must not be done too soon. An interval of some weeks—sometimes even of some months—should be allowed to elapse before another cauterisation is practised; for, if it be repeated too soon, we interfere with that very part of the process on which the obtaining of a cure depends. It is quite a pathological mistake to suppose that the contraction takes place only when the sore is granulating. From the observation of acci-

dental burns it is quite established that the tendency to contraction continues for some time after the sore of a burn is quite cicatrised. The necessity, therefore, for leaving a sufficient interval between one cauterisation and another is obvious, if we wish to avoid undoing by the second what has been accomplished by the first, and the precise time which is to be allowed must depend upon the extent of the disease and the effect of the first cauterisation. By thus making a succession of slight burns of the edges of the opening, each followed by a certain amount of contraction, we may ultimately close the orifice.

Both caustics and the actual cautery have been used for this purpose, some surgeons preferring the cautery as more decided in its effects, others the nitrate of silver as more manageable. The truth I believe to be that either will answer if the amount of cauterisation be duly regulated.

It is right to say, however, that this proceeding is hardly applicable to openings of large size, and that in such cases, if a cure can be achieved at all, it is most likely to be obtained by paring the edges with the knife, and bringing them together by suture. Sometimes by such an operation a great part of the orifice may be closed, but one small portion refuses to unite; and in such circumstances the cauterant method may be applied to this part to complete the cure.

Part Fourth.

PERISCOPE.

MEDICINE.

ON SPASMS AND CONVULSIONS OF CHILDREN.—BY DR AUGUST SCHŒPF MEREI, PROFESSOR OF MEDICINE AT PESTH.—(*Continued from p.* 371.)

2.—*Spasmus Laryngeus.*—Syn. *Asthma Koppii, Asthma Millari, &c.*

To the term which I consider the most just and expressive, because it indicates the nature and seat of the disease, I have added two others, by which varieties, or unimportant degrees of the same affection have been described. There are several other nosological terms adopted in different languages, even in Latin,—*e. g., spasmus glottidis, asthma convulsivum infantum, laryngismus stridulus,* &c.; but it is only to the principal denominations that I propose to direct attention, as these various terms are mere synonymes, and in no way represent distinct species of disease.

The *description* of the neurosis under consideration is rather difficult; but it is sufficient to have witnessed a single case, in order to recognise all which subsequently present themselves. There is something characteristic in this affection,—it is the impeded inspiration, which consequently becomes laborious and sonorous.

An infant about to be attacked with laryngeal spasm at first shows a disposition to cry, but presently the constriction of the glottis prevents the emission of the sounds usual in these circumstances, so that, after one more or less prolonged *natural cry* (by which I understand the act of expiration while weeping), there follows a back-draught (*reprise*), which is noisy and obviously forced. After this, all the succeeding cries are short and stifled, while the inspiratory sounds are, on the contrary, very noisy and prolonged. We see,

then, that the spasm which comes on during the act of *crying* has changed, or rather inverted, the characters and relations of the cries and back-draughts thus :—

In a Child crying naturally,	*In a Child attacked with Laryngeal Spasm,*
Cries sonorous and prolonged.	Cries faint and short.
Back-draughts short and not loud.	Back-draughts long and noisy.

In proportion to the violence of the spasmodic attack will these characters of the inspiratory and expiratory acts be well marked ; in exquisite cases the back-draughts become extremely loud, laborious, and prolonged. During these acts of abnormal and impeded weeping, the physiognomy of the infant is expressive of extreme anguish ; the head is motionless and bent backwards, the mouth widely opened, the face swollen and red, its muscles tense and prominent. After half a minute, or at most a minute, the inspirations become less and less long and noisy, while the cries of the child become again audible, and gradually regain their normal character and duration. Thus the spasmodic attack is resolved into a fit of crying ; and it not unfrequently happens that an infant, which two minutes before seemed in imminent danger of suffocation, and in a condition frightful to behold, returns to the amusement interrupted by the paroxysm.

We have said that these fits commence almost constantly with crying ; we may add, that the crying is sometimes occasioned by the commencing spasm, but that at other times crying, excited by other causes, provokes the spasm of the larynx. This can readily be conceived, when we reflect that violent fits of crying, in very nervous individuals, have almost always more or less of a convulsive character.

The *varieties* of the affection which we have observed depend upon the varying intensity and duration of the spasm ; there are, in fact, cases in which the characters of the disease last only for a few moments, and in which the infant, seized with a slight fit in the midst of its play, presents no other symptoms than great restlessness, a slight stifled cough, and frequent acts of deglutition, involuntary and difficult. At the same time it is irritable, or repulses all advances made to it ; but before it bursts into tears all these phenomena disappear, and the child regains its usual condition. We witness symptoms almost identical in nervous females suffering from slight attacks of globus hystericus.

Between the slightest degree of spasm and that first described, there are numerous intermediate gradations ; and the slight attacks are worthy of our attention, being often the precursors of the more violent. Eclampsia is sometimes superadded to violent forms of laryngeal spasm, or these affections may alternate. The same fact is observed in nervous females. Globus hystericus passes into general convulsions ; and the form of disease termed *eclampsia* does not differ essentially from hysterical convulsions. But in children this transition always denotes a very violent and dangerous nervous affection ; and it takes place almost always in the cases of laryngeal spasm which terminate fatally.

One of the most frequent forms of the affection is that which is accompanied with cough, always of a spasmodic character. Then, instead of cries or screams, short and frequent expiratory efforts are heard, together with sounds of coughing, interrupted now and then by one of the prolonged sonorous inspirations already described.

The inspirations, then, are always of the same nature in cases of laryngeal spasm, but the expirations may be variously modified.

A very violent form or degree of this spasm has been described by Dr Millar ; and as he was the first to direct attention to the affection, succeeding physicians termed it "asthma infantum Millari." The description which he gives applies to ordinary laryngeal spasm extending to the bronchi, and usually attacking very young children. In fact, the younger the child, and the more tender its

constitution, the more violent are the spasmodic attacks which affect it. In such cases the bronchi, as well as the larynx, suffer strong constriction, alternating with convulsive shocks, as an exact observer may determine by external examination.

Dr Kopp believed that he was the first to discover a new species of spasm, which he attributed to hypertrophy of the thymus gland ; and physicians, following his example, admitted an *asthma Koppii* as well as an *asthma Millari* into their nosology. The distinctions between these affections are, however, so slight that they possess a merely historical interest. In the course of this article we shall prove, that the abnormal condition of the thymus gland is merely a frequent coincidence with the retarded evolution, or with that excessive development of the cerebral mass which pathologists are too fond of terming cerebral hypertrophy. It consequently often co-exists with laryngeal spasm, but is not its cause.

The varieties of the affection might be multiplied if regard were paid to the periodicity and frequency of the paroxysms. A certain periodicity I have observed in infants of great nervous susceptibility. One whom I treated, had for several weeks a paroxysm every evening at an almost fixed hour. In other cases, paroxysms succeeded each other every two or three hours from morning to evening, and with great regularity. I have also known fits to occur at night and during sleep ; these cases have usually been most dangerous, on account of the frequency and rapidly-increasing violence of the paroxysms ; and when the infants were very young—from one to three months old—terminated almost always fatally. I have likewise seen infants who suffered from laryngeal spasm whenever they were the subjects of gastric derangement.

Termination.—The affection often terminates favourably, even without the use of remedies. I have seen cases in which the paroxysms, at first frequent, gradually diminished, and finally ceased after a few months, when the infant had attained a certain amount of organic development and strength. Laryngeal spasms, which occur between the sixth and ninth month (at the period of commencing dentition), often cease when a few teeth have appeared. The fits too, which attack very delicate infants, in whom the ossification of the cranium is retarded,—fits for the most part slight, or of moderate intensity,—often cease spontaneously about the tenth or twelfth month.

The younger the patient, and the more violent and frequent the successive attacks, the greater is the danger to life. In such circumstances the spasms almost always pass into eclampsia or general convulsions, and these occasion death either immediately or after repeated fits. I have, however, in my practice, met with several instances in which the case terminated favourably, notwithstanding the frequent repetition of these alarming transitions. A few cases have occurred in which the spasm of the larynx, extremely violent and extending to the thoracic organs, has caused death without the intervention of eclampsia.

The time of life at which the disease occurs is almost exclusively confined to the period between the third and tenth months : cases in infants above and below this age have been, in my experience, rare.

Pathology.—Dissections, which I have often conducted, have only demonstrated what we might infer from the external examination of such children during life. The anatomical condition, with very few exceptions, has been as follows :—Cranium proportionately very large, but its ossification defective when the child's age is taken into account ; the sutures more or less open ; the mass of the brain large,—its consistence often very soft. In some rapid and violent cases I have seen meningeal congestion, but never inflammation well marked, or characterised by its special products. The thymus gland in a fœtal condition, proportionate to the early age of the infant, and to the abnormal cerebral development, with deficient cranial ossification. I must however remark, that this condition of the thymus is seldom observed, except in children whose development is evidently retarded, and who have passed the fourth month.

I have, on the other hand, examined numbers of children who have died of convulsions consecutive on laryngeal spasm, and whose bodies exhibited no trace of the fœtal condition of the thymus. I use the term "fœtal condition," because I have never seen a case to which the denomination of hypertrophy of the thymus could be fairly applied; what I have seen seemed to be the *incomplete retrograde metamorphosis*,—*i. e.*, persistence of the fœtal state of the gland. In the organs of the chest and abdomen I have seen no morbid appearances essentially connected with spasm of the larynx. The extremities, especially the lower ones, were in almost all the cases very meagre. The greater proportion of these patients were of fair complexion, and apparently of feeble make.

Causes.—The disposition to, or constitutional causes of, laryngeal spasm, can only consist in an excessive nervous susceptibility, united with a feebleness in organic development. In treating of the results of my necroscopical examinations, I have just given a description of the constitution and physio-organic condition of children who during their life were the subjects of these fits. The others, in whom the disease terminated favourably, almost all presented the same appearance. With few exceptions, all these infants had brains proportionally large, retarded cranial ossification, meagre feeble extremities, and an imperfect muscular development.

The exceptions to this general rule consisted of patients in whom the laryngeal spasm only existed in a slight degree, or in whom the paroxysms were unfrequent, and due to transient exciting causes, *e. g.*, abdominal derangement, especially obstinate constipation. Early dentition is also an occasional exciting cause. Hereditary predisposition also merits our attention. The infants of feeble nervous hysterical mothers are more subject to laryngeal spasm and eclampsia than other children.

The occasional external exciting causes are but few in number. To gastric derangement, and to the efforts of dentition, we have only to add anger, and violent fits of crying, excited by any cause, in infants of feeble nervous constitution. In some instances it has appeared to me that a violent moral perturbation on the part of the mother during the period of lactation, has induced spasm of the larynx, or general convulsions, in the child. We have already alluded to the mistaken opinion of Dr Kopp, who believed the cause of the disease to consist in tumefaction or hypertrophy of the thymus, and consequent pressure on the subjacent respiratory organs and nerves. A constant material cause like this could not induce an effect merely periodical; but what is more important, in several of our dissections we have been unable to detect the faintest trace of this abnormal condition of the thymus, peculiar to intra-uterine existence. In some cases the organ had completely disappeared.

The nature of the disease, to the best of our conception, is purely nervous, consisting in excessive innervation. The pathological appearances, the causes, constitutional and local; in short, all the observations above made, constitute the grounds of our opinion. It is remarkable that I have met with no case of encephalitis or meningitis, nor of vital exhaustion or anemia with which laryngeal spasm was associated as a symptom.

The local expression of this neurosis consists in spasmodic constriction of the fibres of the larynx, sometimes extending to the pharynx, to the trachea, and bronchi. The intimate union of the nerves supplying these parts may serve to account for the extension of the disease.

The analogy between laryngeal spasm and globus hystericus, to which I have already alluded, is inferred from close observation of the movements of the mouth, pharynx, and larynx, in children and women subject to these affections. Women who suffer more or less patiently, and without weeping, can indicate the precise seat of the spasm; but children, on the contrary, can give no such information, and commence to cry the instant that the spasm is felt. And it is precisely the respiratory and muscular actions proper to weeping which cause the laryngeal spasm of infants to assume a greater violence and aspect

different from the spasms of females. But the exact observer will not be deceived by this difference. In infants and in females he will observe the affection to commence with a like restlessness and nervous irritability; the involuntary movements of deglutition and respiration, the expression of the countenance, are the same; and what the female can tell in words, the infant indicates by its cries or involuntary movements. In both infants and females the affection is apt to pass into or alternate with general convulsions; and both the hysterical colic of adults, and *abdominal spasm* of infants (which we have considered in a former chapter), are sometimes observed to induce spasm of the larynx. There are, however, two important points of difference. Many infants die of the laryngeal disease, but women exceedingly rarely; which may be easily accounted for by the weakness and delicacy of the whole organisation of the former. Again, infants are only subject to laryngeal spasm when very young, very rarely after the completion of the twelfth month. This we can only account for by referring to that particular stage of arrested organic development very common between the third and ninth month, which is usually attended with excessive nervous susceptibility. This last condition, on the contrary, is found in females most commonly between the twenty-fifth and fortieth year,—a period fertile in uterine and nervous derangements. But these points of difference ought not to prevent us from admitting the analogy of the two affections.

Diagnosis.—The diagnosis is very easy. It is sufficient to have seen a single case of the disease to avoid all risk of confounding it with any other; but it is important to ascertain, in a case submitted for treatment, if the laryngeal spasm is constitutional and primitive, or occasional and secondary to some other affection or transitory cause. We should consider it constitutional, and of nervous origin, when the disposition or habit of the child is such as we have described (p. 460),—when the disease has crept on gradually,—and when we can discover no occasional cause. On the other hand, when the infant has symptoms of any other indisposition, we should suppose the fits to be secondary; and this we may do with all confidence when the appearance of the child indicates no constitutional predisposition. We must, in such cases, make minute inquiries to arrive at a precise diagnosis, directing our attention particularly to those occasional exciting causes which we have already considered (p. 460).

Prognosis.—This is influenced by a variety of circumstances. The younger the child, the more delicate its organisation,—the more violent and frequent the paroxysms, the greater is the danger. The passage of the disease into general convulsions is for the most part to be dreaded as a formidable symptom. When we are called to witness a first attack, our prognosis must be doubtful. If, on the other hand, we see that, after the first attack, the child is in good health, and still more if the child has had several attacks without losing its strength, or exhibiting marks of any lesion, organic or functional, there is no reason to despair of a cure. Infants above three months old recover from this affection better than those of more tender age. But at the period of dentition, when we clearly see that this proceeds with difficulty, and when, in such circumstances, a feeble child, after suffering for some days from marked uneasiness, becomes the subject of laryngeal spasm, the danger is imminent, and we have to dread sudden death from violent eclampsia.

Prophylaxis.—The prophylactic means should be those calculated to procure a regular development of the frame from the period of birth. Nourishment adapted to the particular age and constitution, and a clear, pure, and dry atmosphere in the nursery, are the principal means of rearing healthy, strong, well-developed children, and consequently of warding off spasms and convulsions. There are no special prophylactic means.

Treatment.—The medical treatment should be directed against the special causes of the spasms, against the constitutional predisposition, or directly against the paroxysms. To avoid the cause, if such be found, will be almost

equivalent to conquering the disease; and, as we have already pointed out the different causes, the means to oppose each will naturally suggest themselves to the skilful practitioner. We shall here merely make a few remarks upon the cases in which dentition is presumed to be the exciting cause. In the first place, before we can attribute the fits in question to this source, we must be assured that the dentition is proceeding irregularly; and of this we obtain local evidence, from the swollen and tender state of the gums, and from the profuse salivation; while we observe proofs of another character, in certain symptoms which commonly accompany painful dentition—bad humour and gastric derangement. We shall suppose that it is ascertained that dentition is at fault: what is to be done? If there is excessive heat in the mouth or on the forehead, and if the constitution of the infant is not too feeble, we should apply two to four leeches to the neighbourhood of the nostrils (to the interior of the mouth is not proper at this early age), cold should be applied to the head, and two or three liquid motions should be procured every day by the exhibition of a suitable purgative, or by the use of cold water with a little sugar, which, for young infants, is the best of cooling purgatives. If the child is strong, and the symptoms do not yield, the application of leeches may be repeated, and the free margins of the gums scarified. If, however, the child is very weak, or of a very delicate or nervous constitution, we should abstain from leeching and purging; and if the head is not hot, instead of the internal and external refrigerants, we should give several times a-day a spoonful of infusion of chamomile, with a drop or two of sp. melissæ. To correct the faulty constitution (of which we have treated at pp. 459, 460), or to give a healthy impulse to the vegetative forces—the most sure means of diminishing that excessive nervous irritability which is of itself sufficient to induce spasm—I call the attention of physicians to three remedies, which, in a considerable number of cases, I have used with excellent effect.

No remedy can bear comparison with *cod-liver oil.* If the retarded evolution is at the same time united with a state of atrophy more or less perceptible, the success of the medicine will be the more remarkable. To a child of two to four months old, we should give, in the course of the day, two to four teaspoonfuls; a child of six months can take two tablespoonfuls in a day. If the use of the remedy induces diarrhœa, we should give at the same time, every day, one or two small doses of Dover's powder, without discontinuing the oil; but if moderate doses interfere with the appetite and digestion, the use of the oil must be abandoned. This has rarely occurred in my experience; and in such cases I have employed the oil endermically, rubbing in from ℥ss. to ℥j. along the spine every evening.

Washing of the whole body with water, used gradually colder and colder every morning, for a time varying from one to four minutes, together with *cold affusion* on the head, constitutes another remedy which I can sincerely recommend. It is indeed a remedy of sovereign efficacy against exaggerated nervous susceptibility. If the skin of such infants does not seem sufficiently active, I direct the washing to be performed with a mixture of one part of alcohol to eight or fifteen parts of cold water. I need hardly remark, that these washings, continued for but a short time, and performed with a fluid which rapidly evaporates, can have no stimulant or too exciting effect upon the nervous centres. After the washing, the body should be well dried, and if the day is not moist the child should be taken out to the open air. These are the two chief remedies for correcting the disposition to laryngeal spasm when it is well marked.

Iron is, however, a remedy worthy of our confidence, when there is evidently a chlorotic condition,—*i. e.*, a diminution in the quantity of the blood, or impoverishment of its quality. When this condition occurs in conjunction with the above-mentioned constitution, the cod-liver oil should be administered in preference to all other medicaments. But after persisting in the use of the oil for some weeks without notable amendment of the chlorotic symptoms, re-

course must be had to iron. The Carb. Ferri. Saccharatum is a preparation well adapted for the use of infants. As an average dose, we may order for a child of four months a grain of the carbonate, to be taken twice or thrice a-day. The dose of the tincture of iron is two drops thrice daily. The effects of the medicine upon the abdominal organs, and upon the blood, must be always carefully watched.

The purely antispasmodic remedies, which practitioners and authors, both ancient and modern, recommend, have not, in my experience, seemed of much advantage. These drugs can, of course, be administered only during the *intermissions*, to prevent frequent fresh accesses of the paroxysms. I have experimented with almost all the stimulants and calmatives of the nervous system: opium is quite unsuitable in this form of spasm; and musk I have never known to produce favourable effects. Sp. Melissæ, in doses of two or three drops, given twice or thrice daily in a little warm infusion of chamomile flowers, has, I think, been useful in the cases of very delicate infants, who, after very violent and frequent paroxysms, have been much exhausted. In such children, after a few days' use of this remedy, the paroxysms have become less frequent, while there has been proportionate increase of vivacity and strength.

Quinine is a remedy worthy of all our attention. I have only commenced its employment within the last three years, and have used it in about a dozen cases. In above the half of these, the effect has been equally sudden and satisfactory. The cases in which I prescribe it are those of weak and obviously nervous infants, suffering for weeks or months from fits, but during the intervals free from all disease, eating and digesting well, and without hardness of the abdomen. In such circumstances, I have given the largest possible doses of quinine just as we should do in cases of ague or intermittent neuroses. The prescription for infants of four to six months is the following:—℞ Aq. Melissæ, ℥ss.; Mur. Quinin., gr. vi.; Acid. Mur., q. s.; Syrup. Capill. Veneris, ℥j. S. A teaspoonful should be given every second hour, till symptoms of gastric disturbance, indicative of the physiological action of quinine, be induced. Success will in suitable cases follow the use of the drug in two or three days. The periodicity of the fits being broken, we may next apply our treatment to the correction of the morbid disposition above described (p. 460). As some infants cannot tolerate the use of quinine, we must be prepared to diminish the dose, or even, in some cases, to relinquish the use of the remedy altogether.

THE STETHOMETER: AN INSTRUMENT FOR FACILITATING DIAGNOSIS BY MEASURING THE DIFFERENCE IN THE MOBILITY OF OPPOSITE SIDES OF THE CHEST. BY RICHARD QUAIN, M.D.

[We extract from the "London Journal of Medicine," October 1850, Dr Quain's description of this new instrument, by means of which he thinks that the degree of expansion attending each effort of inspiration may be measured with great accuracy.]

The little instrument consists of a flat case, not unlike a watch-case. On its upper surface is a graduated dial and an index, which stands at a maximum number, which may be considered a "zero." This case contains a very simple movement, by means of which the index can be acted on. A silk cord, which may be of a sufficient length to surround one-half, or the whole circumference of the chest, passes through an aperture in one side of the case. This cord acts on the index. When the cord is drawn out, or extended for the space of one quarter of an inch, it will be observed that the point of the index will once traverse the circumference of the graduated dial. In other words, when the index has gone once round, it shows that the cord has been extended one-fourth of an inch. The dial is graduated or divided into fifty equal parts. Each of these parts is, therefore, equal to the fiftieth part of a quarter of an inch,—that is, to the 1-200th of an inch. In making and recording observations with this instrument, I am in the habit of expressing a fact thus: right apex, 30; left, 15. It will be understood that the motion is as 15 to 30, without reference to

these figures being eighths, fiftieths, or two hundredths. The index is further capable of going round a second time, on an additional quarter inch of the cord being drawn out. Hence, two revolutions of the index are equal to half an inch of movement—an extent of motion sufficient for all practical purposes. * * * P. 924.

Mode of Application.—It is quite evident, that if the instrument be so placed that extension be made on the cord, the amount of the extension will be shown by the movement of the index on the dial. For example, if the instrument is laid flat on the spine, and held in its place by the first and second fingers of the left hand, whilst the cord is carried round the chest, and pressed on one of the ribs, or the sternum, by the fingers of the right hand, then, when the individual under examination expands the chest during inspiration, the amount of expansion will be communicated to the cord, and thus indicated on the dial. The cord may then be directed around the opposite side of the chest, and thus will be at once seen any difference which exists in the relative mobility of the two at the point under examination. It will, of course, be absolutely necessary in every examination such as this, that corresponding parts of the chest be compared. For all useful purposes, it may be said that the movements of the opposite sides of the chest, in health, are identical—the difference which exists over the region of the heart, at the left side, is too immaterial to interfere with practical conclusions. * * * P. 925.

A small pedestal with a foot attached can be fixed in the side of the case, when the observer wishes to ascertain the modifications of the movements over a limited or circumscribed spot. On this spot the foot is placed and held by the fingers of the left hand, the cord directed towards this point is held between the fingers of the right hand. These fingers, being thus made the fixed point, must be kept steady, and not allowed to touch the part under examination. The spring in the instrument is sufficiently strong to resist the gentle tension made by the fingers, and the instrument itself being pushed forward or raised by the elevation of the part during inspiration, the movement of the index, as when the case is applied on the flat surface, becomes the measure of this elevation.

Such being the mode of using the instrument, it will be necessary to say a few words on the precautions which are required in securing accuracy in the result. 1st. It is absolutely imperative that corresponding portions of the chest be examined, and that the mode of applying the instrument, and the point at which it is applied at each side, be identical. 2d. Care must be taken that the patient breathes in the same manner whilst opposite sides are being examined. 3d. It should be seen that the cord, when the observation is commenced, is held sufficiently tense to act on the index.

Uses of the Instrument.—The expansive movements of the chest during inspiration may be said to be the measure of the capacity of the lungs for air. So they are; and by an accurate measurement of the one, we might estimate tolerably correctly the other, and thus be able to compare the relative breathing powers of different individuals. It is exceedingly difficult to make this comparison by external measurement; for the movements of analogous or corresponding parts of the chest, even in those with unimpaired respiratory powers, are remarkably different in different individuals. For not only are such movements modified by age, sex, and occupation, but they are influenced by peculiarities beyond the influences apparently of any fixed or recognised rule. I therefore do not propose to use this instrument in testing whether A has a better breathing-power than B. The spirometer of Dr Hutchinson is a far more correct means of estimating this quality. I propose my instrument as a means of ascertaining any want of symmetry, independently of malformation, and therefore indicative of disease, in the movements of the corresponding parts of the same chest. It is not my intention to discuss here the various conditions under which this want of symmetry may occur. Obstruction, for example, of a bronchus, by a foreign body in, or by a tumour pressing on it, will interfere

with the movements of the side of the chest containing the lung to which such bronchus is distributed. Disease of the substance of the lung, tuberculous, malignant, or inflammatory, and emphysema affecting the air-cells, also prevent the free expansion of the lungs or parts of them affected. Diseases of the pleura,—viz., acute or chronic pleurisy, bands binding down the lung, pleuritic effusions or tumours in the pleura have a like effect. Pleurodynia, and diseases of the external walls of the chest, may, of course, interfere with their free movements. However trifling the degree of limitation of motion, which may be produced by any of these diseases, or by many others not here enumerated, I believe this instrument, if properly used, will inevitably indicate it. When such limitation is thus observed, recourse must be had to the study of the general symptoms, and to the other means of physical diagnosis, to discover on what the irregularity depends. Thus, then, attention may be, in the first instance, directed by the use of the instrument to the seat of disease; or, when disease has been suspected or discovered by other means, mensuration will be useful in confirming the diagnosis, and expressing the extent of the local lesion. In both these senses, I have found the instrument an exceedingly valuable aid in the diagnosis of the earlier stages of phthisis. Indeed, it is quite remarkable how early, and to what an extent, the respiratory movements are modified by tubercular deposit. I hope, however, on some future occasion to return at some length to this and some allied subjects connected with the disease.—Pp. 926, 927.

ECTOPIA CORDIS. BY M. MARCHAL (DE CALVI.)

One remarkable case of ectopia of the heart was published by Professor Cruveilhier, in the Number of the "Gazette Médicale," for August 7, 1841. We will compare it with one recently communicated to the Académie de Médecine, by M. Follin. In the former, ectopia of the heart was the only anomaly; the heart protruded from the chest through a circular opening in the upper part of the sternum; the infant was strong and lively. In the case of M. Follin, the fœtus had only reached the sixth month, and ectopia of the heart was complicated with protrusion of most of the thoracic and abdominal organs; the vital powers were also very feeble. M.M. Cruveilhier and Follin have principally studied in these two cases—1. The external movements of the heart,—that is, its contractions and dilatations; 2. The sounds of the heart.

With regard to the external movements (so called in contradistinction to the internal or valvular motions), M. Cruveilhier remarked as follows:—

1. The contraction of the ventricles coincided with the dilatation of the auricles, and the propulsion of blood into the arteries. The dilatation of the ventricles coincided with the contraction of the auricles, and also with the contraction of the arteries. 2. Contraction and dilatation succeeded each other without intermission, so that the period of repose recognised by authors was absent. 3. The contraction and dilatation of the ventricles and auricles succeeded each other, as if resulting from two opposed and always active forces, like the two alternate movements of a pendulum or accurately poised balance. 4. The duration of the contraction of the ventricles was double that of their dilatation; dividing the total duration of the cardiac movements into three equal periods; the contraction of the ventricles and dilatation of the auricles together occupied two parts,—the contraction of the auricles and dilatation of the ventricles one part. 5. During the ventricular systole, the twisted fibres of the apex of the heart became more prominent; the apex, in virtue of the spiral contraction of its fibres, was tilted from right to left, and from behind forwards. It was then, and in this manner, that the impulse of the apex of the heart against the walls of the chest took place. 6. The ventricular diastole was effected rapidly, and with great energy; the hand, closed upon the heart, was during this movement opened with violence. 7. The dilatation or ventricular

diastole was accompanied with so distinct an impulse downwards, that at first one was led to suppose, that the shock of the heart against the thoracic parietes was produced during the dilatation of the ventricles.

The following circumstances were observed in the case of M. Follin :—The auricles and ventricles being dilated, the auricles contracted by a rapid movement, and returned immediately to their original volume, and became motionless.

Immediately afterwards, the ventricles contracted slowly and gradually from the apex to the base. The interval comprised between the auricular and ventricular contractions, might be estimated at a quarter of a second. After their contraction, the ventricles were refilled, as it were, by the relaxation of their fibres, without any sudden movement, and before the contraction of the auricles. Then succeeded a considerable period of rest ;—after which a new "revolution of the heart," to use the expression of M. Bouillaud, recommenced with the auricular contraction.

The two observers, MM. Cruveilhier and Follin, agree, that the ventricular contraction is a slow movement, though one would naturally expect it to be sudden ; but they differ remarkably as to the manner in which the dilatation of the ventricles is effected. According to M. Cruveilhier, it takes place with quick energy, and with one impulse, solely in virtue of the contraction of the auricle. According to M. Follin, it is produced in a very different manner. The ventricle, in dilating, simply by the relaxation of its fibres, would attract a portion of the blood contracted in the auricle, and the latter by its contraction would only complete the diastole and regulate the ventricular contraction. We have observed this phenomenon to take place similarly, in several experiments upon dogs, and upon a horse. It appeared to us, that the ventricle is so promptly refilled after its contraction, that the heart is never entirely empty. And, indeed, relaxation of the ventricle is sufficient to account for its dilatation; and it cannot dilate without drawing blood from the auricle. It may be objected, however, that the ventricle is relaxed for a very short space of time, considering the length of its contraction, and that that short period is precisely occupied by the auricular contraction, so that the latter process will be inseparable from the refilling of the ventricle, which it is able to effect.

The rapidity with which the ventricular contraction succeeds the auricular contraction, is a fact which cannot be questioned, and accords with the observations of M. Follin. According to Dr Beau, an observer of great merit, to whom science is already much indebted, the two contractions succeed each other so rapidly, so convulsively as it were, that the arterial pulse is almost isochronous with the contraction of the auricle,—a statement, which would appear contradicted in some measure by the recognised long duration of the ventricular contraction.

The two observers disagree also to the repose of the heart. M. Follin has remarked a considerable period of rest. But, in his case, the heart beat only thirteen times in the minute. The question of the period of rest is an important one ; for if repose takes place after the contraction of the ventricles, before that of the auricles, this period of rest may be occupied by the passive refilling of the ventricles, in consequence of the relaxation of their fibres. Unfortunately no conclusion on this point can be drawn from the case of M. Follin, as, in a heart which only pulsates thirteen times in a minute, a period of rest may justly be regarded as abnormal.—*Gazette des Hôpitaux*, 15th October 1850.

BRONCHOCELE OF INFANTS. BY DR F. BETZ.

Dr Betz of Tubingen, in an essay published in Henle and Pfeufer's Zeitschrift, Band ix., Heft 2-3, makes the following observations :—1. The bronchocele of newly-born children is a disease hitherto little noticed. 2. Its subjects are for the most part strong and well-nourished children. The bronchocele has sometimes the form of a thyroid gland ; lies obliquely across the neck, and

is usually mistaken for a mere fold of fat. Sometimes the breadth of the neck seems to be increased by the swelling ; at other times there is no marked deformity of the throat. 3. The symptoms caused by the bronchocele are—dyspnœa from the time of birth ; the inspirations being prolonged and croaking, the expirations likewise difficult, and now and then accompanied with crying ; sometimes respiration is arrested, and suffocation impends. When the dyspnœa is very urgent the child cannot suck, or takes the breast with much difficulty. In slighter degrees of the affection, sucking and drinking are not prevented. The dyspnœa or asthma comes on in paroxysms. 4. Infantile bronchocele consists in simple hypertrophy of the thyroid gland, affecting its entire substance, one of its lateral lobes, or a portion of one of these. 5. The swollen lobes, by their pressure inwards behind the œsophagus or pharynx, squeeze it against the larynx, and thus impede the access of air to the glottis,—hence there is dysphagia and danger of choking during deglutition. 6. Infantile bronchocele seems to be hereditary. 7. The so-called *asthma laryngeum* in most cases depends upon hypertrophy of the thyroid gland.[1] 8. Many cases of *atelectasis pulmonum* may have a similar origin. 9. When the danger from the disease seems to warrant it, Dr Betz applies leeches to the hypertrophied gland, gives an emetic to clear away mucus, and orders the cavity of the mouth to be carefully cleaned out from time to time. In chronic cases he prescribes iodine both for external and internal use.—*Henle's Zeitschrift;* and *Behrend's Journal für Kinderkrankheiten*, band xv., July, August 1850, s. 149, 150.

PATHOLOGY.

REMARKS ON THE MECHANISM OF THE INFLAMMATORY PROCESS. ERNST BRUCKE.

[COMMUNICATED BY DR E. WATERS.]

On the retardation, and ultimately cessation, of the circulation in the capillaries of an inflamed part, the blood corpuscles are known to accumulate in, and at length to completely fill and obstruct them. This phenomenon constitutes one of the most interesting points in connection with inflammation, and various explanations of the *modus operandi* of its occurrence have been advanced. Drs C. J. B. Williams and Addison have attributed the inflammatory stasis to the multiplication and accumulation of the colourless corpuscles of the blood in the implicated capillaries. The merit of contesting and refuting the accuracy of this observation belongs to Dr Hughes Bennett : it is now no longer possible to deny that the greater or smaller proportion of these corpuscles to the mass of the blood is entirely independent of inflammatory action. But though the correctness of this observation has been disproved, there can be still no doubt that at least some of the changes connected with the process can, with the aid of the microscope, be perfectly well examined, and that thus facts may be established which must be taken into account in all reasoning on the subject.

Henle has given the weight of his name to a theory, which ascribes the diminished rapidity of the circulation, in the early stage of inflammation, to a primary dilatation of the capillaries and small veins. Brücke opposes this view ; he holds this dilatation to be passive, and the consequence of a primary contraction of the arterial twigs supplying the affected part. Thomson had already signalised contraction of the arteries as the earliest change in the development of inflammation, but Brücke has been the first to deduce from this contraction the subsequent phenomena of the process. He has shown that the contraction lasts for a much longer period than was previously supposed, viz., for hours ; a statement of the correctness of which we have convinced ourselves. The substance of Brücke's memoir is as follows :—

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[1] This is in direct contradiction with the observations of Dr Schœpf Merei. (See p. 460.)

"On touching, with liquid ammonia, the web of a frog's foot, expanded under the microscope, an acceleration of the circulation is the first effect witnessed. This depends, in all probability, on an increase of the heart's contractions,—as the frog, by its movements and efforts to escape, evinces other distinct signs of pain and suffering. During the movements of the animal, the circulation frequently becomes suddenly slower, and then just as suddenly resumes its rapid rate, which, as may be easily understood, is occasioned by the temporary compression of large vascular trunks. In a short time the animal becomes quiet, and the first signs of the inflammatory process appear, the circulation in the capillaries and small veins is seen to be diminished in rapidity, and these vessels contain more blood corpuscles than usual. These two phenomena continue increasing, until at length the circulation in a portion of the capillary system, and in the small veins originating in it, entirely ceases, and the vessels are dilated and turgid with highly coloured red blood corpuscles, so densely crowded that the outline of the single corpuscles can but rarely be distinguished. On examining the arteries supplying the part affected by the stasis, their ultimate twigs are frequently found to be already full of blood corpuscles, which are slowly but continually accumulating and filling the artery higher and higher.

"With each contraction of the heart, the blood advances somewhat in the vessels, but on its dilatation again recedes. Now, as the blood corpuscles are heavier than the liquor sanguinis, they are less affected by the slow retrograde movement than by the rapid impulse forwards, and fresh corpuscles are in this way, as may be easily followed by the eye, constantly added to those already arrested. On examining more closely the artery in which the oscillations are perceived, its upper portion is found to be considerably narrowed, so that a single branch filled with blood corpuscles is frequently larger than the stem from which it, together with many other branches, arises ; in fact, if the arteries are measured with the glass micrometer before the experiment, at the point where they pass from the toes into the web, it is easily proved that the inner diameter of the artery, which feeds the implicated portion of the capillary vessels, is diminished to the half, indeed to a third, or even to a fourth, of its original size during the development of the stasis. I have often observed this condition of the narrowing of the arteries, and of the oscillation in them, in a completed stasis of from four to five hours' standing.

"Dilatation of the capillaries to double their diameter, and more, as some authors maintain, never occurs during the development of the stasis ; it is only found when the inflammation has already existed for some time. The formation of the stasis cannot be accounted for by the dilatation of the capillaries ; their dilatation is so slight that it is altogether denied by some observers, and, according to my observations, does not amount, at least during the development of the stasis, to more than a fourth of their original diameter.

"As regards the primary contraction of the arteries, it cannot be the effect of the elasticity of the arterial walls, for this would only occasion it if the pressure, which the blood exerts on them from within, should cease, and among the phenomena of the stasis we find none which could have such a consequence. The diminution of the calibre of the arteries must, therefore, be due to their contractile fibres ; the anomalous condition in which, owing to the stasis, their twigs are placed, might be assigned as the cause of it ; but, if this anomalous condition can excite contraction in the arteries, there is no good reason why it should not also be primarily excited by the originally employed irritant. It now remains to be seen whether the phenomena of the stasis may not be accounted for by the contraction of the arteries.

"If fluid be driven into a ramifying tube, it is clear that, on the occurrence of contraction of the trunk, other circumstances remaining the same, the rapidity of the current in the ramifications must be diminished. On viewing the circulation under the microscope, it appears that, in the normal condition, the blood courses onwards, in the whole capillary network, at a tolerably uniform rate,—it is only in isolated and somewhat large vessels, which, as transitions

from the arteries to the veins are found interwoven in the capillary net, that the current is somewhat more rapid, the reason of which is obvious. Now, if an artery contract, this regularity in the distribution of the current, corresponding to the normal condition, and to the normal calibre of the arteries, must be disturbed in its vicinity ; and it thus becomes clear, that not only a local retardation of the circulation, but also a local stasis, and even altered direction of the current in some vessels, may be caused by the contraction of a small artery.

"On the retardation of the circulation, the phenomenon which first shows itself—in fact, simultaneously with it—is the accumulation of the blood corpuscles in the slower flowing blood. The corpuscles are specifically heavier than the *liquor sanguinis*, and are only kept suspended by the motion of the blood ; the torrent of the blood bears them along with it through the capillaries ; and it is often seen how they must turn and bend themselves, to pass through their narrow and tortuous channels. If a stream, which bears along with it a specifically heavier body, diminishes in rapidity, the celerity of the solid body not only diminishes absolutely in the direction of the stream, but also relatively to the medium rapidity of the current ; the solid body will sink, arrive in the slower moved layers, then be stilled, rolled onwards by starts in the bed of the stream, and at last remain stationary. On considering that fresh blood is continually added to the vessels concerned, and that with the diminution of the velocity of the circulation the average celerity of the blood corpuscles no longer bears the same relation to the average celerity of the *liquor sanguinis* as in a more rapid current, it becomes evident that with the increasing slowness of the circulation the blood corpuscles are multiplied in the vessels, until at length they completely fill them, form on their part an additional obstacle to the circulation, and annul the last remains of it. This phenomenon may be compared to that which is observed in rivers, which choke their original bed with the sand they carry with them, and are forced to seek new passages to the sea.

"Lastly, it remains to be shown how the dilatation of the vessels in which the blood stagnates is produced. As the pressure which the blood exerts on the walls of the vessels depends on the resistance which it has still to overcome, so, in the normal circulation, the pressure decreases rapidly from the small arteries to the capillaries, and from the latter to the veins ; in those vessels, however, in which the blood corpuscles have accumulated, the resistance is manifestly increased, and, if they are completely obstructed at any spot, then they form from this upwards a blind appendage to the afferent vessels, in which the pressure is as great as at that point of the vessel whence the last branch, in which the blood still flows, is given off.

"I do not mean to assert, that I have proved that all cases of stasis result from a contraction of the arteries ; I even believe that I am acquainted with some which originate from other causes : thus much only I believe I have shown,—viz., that the phenomena of the stasis can, at least, be as well and as completely accounted for by the contraction of the small arterial twigs as by a primary dilatation of the small veins and of the capillaries. If the phenomena connected with the occurrence of the stasis can be explained by the contraction of the arteries, it appears to me, when contraction of the arteries as well as stasis is observed on the application of any irritant, to be the most natural conclusion to consider the occurrence of the former as the immediate consequence of the irritation, and as the cause of the remaining phenomena ; because the contraction of the walls of the arteries, in consequence of irritation, belongs to the well-established facts of physiology, and dispenses with every hypothesis which would attribute the dilatation of the veins and capillaries to direct or reflex paralysis of the nerves of the vessels."

Brücke has placed beyond doubt the fact of the contraction of arteries in the frog in the first stage of inflammation, and the persistence of such contraction for a considerable period. The inferences he has drawn respecting the influence of this condition on the development of the stasis and the dilatation

of the capillaries, are in the highest degree worthy of consideration; and his memoir must be esteemed an important contribution to pathology. If a certain condition, proved to exist in the lower animals, influences the formation of the inflammatory stasis in them, we are driven to the conclusion, that the same condition would necessarily not be without effect in the production of the same phenomena in man and the higher animals, though in them not equally susceptible of ocular demonstration.

ON THE ALKALINITY OF THE SERUM OF BLOOD. BY M. CAHEN, JUN.

M. Cahen has read before the Academy of Sciences of Paris, a memoir entitled "Experimental Researches on the Alkalinity of the Blood of Man." He seeks to prove that the neutral azotised principles of the blood, albumen and fibrine, different forms of Mulder's protein, are dissolved in the serum in virtue of its alkalinity. According to him, fibrine is only albumen normally in excess beyond what the alkalinity of the fluid can keep in solution. The diminution of the dissolving power of the serum, which ought to be proportionate to a corresponding diminution of its alkalinity, should augment the quantity of spontaneously solidifiable albumen (*i. e.* fibrine); and, *vice versa*, an increase of alkalinity would, by its dissolving power, prevent the coagulation of the fibrine. The principal conclusions of the author are the following, some of them being already known, others being peculiar to himself:—*In the healthy state*—1st, The circulating serum holds in solution a substance which will ultimately divide into albumen and fibrine; 2d, Fibrine and albumen may be artificially transformed one into the other; 3d, Fibrine cannot exist with its characteristic properties in presence of a weak solution of soda at the temperature of 104° Fahr.; it is then constantly transformed into a substance which it is impossible to distinguish from albumen. *In inflammatory diseases*—4th, The fibrine is augmented, the albumen is diminished; 5th, The increase of the fibrine is equal to the diminution of the albumen; 6th, The salts of soda are in smaller quantity; 7th, The addition of a small quantity of soda re-establishes the normal proportions; 8th, The serum is less alkaline. *In the so-called diseases of putrescency*—10th, The serum is more alkaline. The diminution of the fibrine is due to the increase of the alkalinity of the serum. From these considerations M. Cahen is led to form the following therapeutic deductions, which, however, he presents with reserve from the want of practical trial—that in inflammatory affections the use of alkalies is indicated; that in putrid diseases it is proper to try the effect of acids.—*Gazette Méd. de Paris*, 6th July 1850.

[There can be no doubt that, if M. Cahen's statements on the relative alkalinity of the blood, in health and various forms of disease, were confirmed, they would have an important bearing on pathology and therapeutics; but we cannot help being uncharitable enough to think, that, in some respects, they savour of a foregone conclusion. We reserve our judgment, however, till we have seen the details of his experiments.]

ON THE STRUCTURE AND PHYSIOLOGICAL USES OF PEYER'S GLANDS. BY ERNST BRUCKE.

In the autumn of 1849, Brücke made some attempts to inject the most minute lymphatics of the bowels, by forcing oil of turpentine, coloured with alkanet root, into a portion of the intestinal tube of the cat. After the pressure on the piston of the syringe had been for a short time maintained, he observed that some of the lymphatics of the mesentery became filled, and that the coloured oil passed through the so-called *pancreas asellii;* and finally reached the *receptaculum chyli* and thoracic duct. On more minute examination, it was found that the only trunks of the lymphatic system injected were those which took their origin from the neighbourhood of the glands of Peyer. These trunks were formed by the union of minute arborescent branches, which obviously ran on the surface of the bowel, and whose finest twigs proceeded from the

glandular patches. A reddish tinge was distinguishable in the interior of some of the small glands, but was more distinct in the narrow interstices between them. Brücke supposed that by distending the cavity of the bowel, he had succeeded in rupturing the capsules of Peyer's glands, and that the injection had, through these breaches, passed into the lymphatics. This view was confirmed by other experiments, in which a column of fluid was substituted for the injecting syringe. There was no extravasation ; and the uniform way in which the injection ran, commencing in a reddish point in the gland, and then proceeding by minute channels to form an arborescent network, terminating in trunks, which proceeded through the mesentery, seemed to indicate that the fluid passed by natural canals. The success of these injections suggested the inquiry, how far the glands of Peyer resembled lymphatic glands. Brücke was aware of the views on the structure of lymphatics held by Professors Ludwig and Noll, of Zurich, which the former had shortly before communicated to him. "Each vas inferens," writes Ludwig, "before entering a gland divides into numerous small twigs, each of which, on piercing the *membrana propria* of the gland, loses its coats, and pours its contents into the cavity of the gland, which is traversed with delicate and incomplete septa of fibrous tissue, and filled with all kinds of lymph corpuscles. The vas efferens is formed in like manner by the reunion of minute twigs." On examining Peyer's glands, Brücke found great numbers of lymph corpuscles, cytoblasts, and cells, undistinguishable from the contents of ordinary lymphatic glands. It remained to trace, with the assistance of the microscope, the supposed connection of Peyer's glands with the lymphatic system. He found that many of these glandules presented, on their side next the peritoneum, a prolongation, ending in a delicate bundle of fibrous tissue, attached to the capsule of the gland. These bundles, after the addition of acetic acid, were seen to contain dark nuclei, similar to those observed in the glands themselves. No trace of a vessel was seen within the bundles, unless the fibres themselves were arranged so as to form a sort of tube. Brücke points out, that similar bundles of fibres are connected with the villi, and argues that they must be the channels by which the lymph is conveyed from the villi to the trunks of the lymphatic system. Whether each bundle forms a conduit for the chyle, or whether the fluid insinuates itself between the individual fibres, drawing along with it the nucleated and fatty elements of the chyle, may admit of question. Brücke thinks that the whole so-called "submucous tissue" consists of these bundles and connecting fibres, with nothing in the shape of a vessel, except the blood passages.

He concludes his very interesting memoir as follows :—"It is ascertained, 1. That the chyle ducts may be injected through the glands of Peyer, and that observation of the course followed by the fluid injected proves that it passes in natural and not in forced channels. 2. That the cytoblasts which fill Peyer's glands are like those in the mesenteric glands ; and during resorption form cells resembling the lymph corpuscles. 3. That Peyer's glands are connected with thread-like structures, resembling those proceeding from the villi, and constituting the passages for the chyle ; for in the submucous tissue nothing else which could serve such a purpose has been detected ; and a system of vessels, proceeding from sources so numerous as the villi, and of course containing such a mass of fluid, could not have escaped observations such as have been recently made.

"The opinion which I pronounce, that Peyer's glands are lymphatic glands, situated in the wall of the intestine, and destined to elaborate the first organised elements of the chyle, may be erroneous ; for almost all physiological conclusions, founded upon morphological investigations, attended with great difficulties, and not satisfying all conditions of inquiry, may lead to error ; but I consider that the extent and the results of my inquiries have been such, that I am entitled to submit an opinion, upon the correctness of which other observers may decide."—*Ueber den Bau und die Physiologische Bedeutung der Peyerischen Drüsen. Von Ernst Brücke. Wien* 1850.

SURGERY.

HARELIP—DIRECTION GIVEN TO THE KNIFE IN THE PARING OF THE EDGES.

A few weeks ago, Mr Fergusson had to rectify, upon a child about one year old, the deformity of harelip. The operation was performed in the usual manner, with the exception of the direction given to the knife, whilst the margins of the fissures were being pared. As the scalpel ran straight from above downwards, Mr Fergusson made it cut slightly inwards on both sides, when he reached the red portion of the lip, so as to save a little more of that portion than of the part covered by skin. This precaution is likely to remedy a defect which is generally left after the union of parts in harelip. Every surgeon has noticed the peculiar notch which the red part of the lip presents after the linear wound above it has completely united; this is more or less the case with various patients, and it must have struck many practitioners that it would be very desirable to obviate this defect, and bring about the complete union of that part of the lip which is immediately continuous with the mucous membrane of the mouth. The angle inwards, with which Mr Fergusson modified the usual rectilinear paring of the edges, is likely to effect this desideratum, and will tend to lessen very considerably the deformity left after the operation for harelip. Mr Fergusson has been in the habit of modifying the incision in this manner for the last seven years, and was sorry that this method, which he believed was of French origin, was not more followed in this country. This plan was recommended by Professor Miller of Edinburgh, and, as he (Mr Fergusson) had learned from a recent conversation with Messrs Smith and Teale, of Leeds, was followed by some of the surgeons in that town. The child has progressed very favourably, complete union has taken place, and no notch is perceptible on the lip.—*Mirror of London Surgery: Lancet*, October 12, 1850.

[In Mr Syme's "Principles of Surgery,"—the first edition of which was published in 1832, when Mr Fergusson was delivering surgical lectures in Edinburgh,—it is said, at p. 672, in regard to the operation for harelip,—"The margins of the fissure present a convex outline, which, if allowed to remain, would cause the edges, when joined, to form an unseemly angle in the lip at their point of meeting. The respective surfaces should, therefore, be made straight, or even slightly concave, so that, when brought together, they may give the lip its natural fulness." It would be interesting to know upon what grounds Mr Fergusson gives our French brethren the credit of this little manipulation.]

WOUNDS OF THE SCALP—NEW METHOD OF DRESSING. BY M. BARTH.

A lady had for some years three wens on the head. As they were productive of no uneasiness, they were not interfered with. But one day the lady, with her comb, wounded one of them, which led her to consent to the operation. M. Barth, from motives of precaution, removed only one of the tumours. He separated the hair in a straight line without shaving it, and made a simple incision, by which the cyst was turned out with the usual facility. He then separated the hair on each side of the wound into three or four bundles, crossed them from one side to the other, and thus drew the edges of the wound perfectly together. The wound was healed on the third day; a few days afterwards he removed both the other wens at the same time; he followed the same plan of bringing the edges together, and with a like good result.—*Bulletin Gen. de Thérapeutique*, 15th July 1850.

[This plan has not the recommendation of either novelty or expediency. It was proposed long ago, and tried without success. The great obstacle to primary union, after the removal of cysts from the scalp, is accumulation of blood in the cavity; and the only effectual mode of preventing this is to apply pressure firmly, by means of lint or sponge, on each side of the wound.]

DISLOCATION OF THE SCAPHOID BONE OF THE FOOT. BY DR WALKER, PETERBOROUGH.

On the 10th of June 1850, a stone-mason, aged 40, and of a spare but sinewy form, in stepping from the top of a wall eighteen feet high, to another about a foot lower, over an intervening space of about two feet, advanced his right foot, but stepped short, his toes only resting upon the wall, his whole weight being, in the act of stepping, thrown upon the right leg. The foot, as he says, "bent upwards, and something gave way." Falling down between the two walls, he found himself on the ground on his hands and knees, not hurt, as he conceived, by the fall, but quite unable to work, and his foot in great pain. He was carried home, and I saw him next day. He was in bed, and on examination, my opinion of the case entirely coincided with that of Mr Hanbury, our excellent house-surgeon, who had previously visited him. We found a bold projection on the dorsum pedis, in the situation of the os naviculare, and a corresponding depression beneath, where, in the normal condition, the tuberosity of this bone forms a distinct projection. Over the displaced bone the skin was tense and red, and over this point there was a deal of pain on pressure. He could bend and extend the ankle-joint without difficulty or much increase of pain. I at first tried to reduce the bone by firm pressure with the thumbs, but it felt firmly fixed in its abnormal position, and I could not move it.

I then requested Mr Hanbury to press the foot downwards, whilst I attempted to return the bone to its position between the astragalus and cuneiform bones, and with an audible snap it returned, and the deformity disappeared.

I then merely directed the foot to be kept quiet and fomented; but two days after, finding that the bone felt loose and easily moveable, with a tendency to slip upwards, occasioning the peculiar appearance of a slight dimple or hollow in place of its natural projection, a plaster and roller were applied.

He was confined to the house for three weeks. He then returned to his work, but continued slightly lame for about a month after. This morning (Sept. 16th) I find, on examination, no remaining appearance of injury; he has no pain, and the motions of the foot are perfect.—*Provincial Medical and Surgical Journal*, 16th October 1850.

CASE OF WOUND OF THE LEFT VENTRICLE OF THE HEART—PATIENT SURVIVING FIVE DAYS. BY JOHN W. H. TRUGIEN, M.D., OF PORTSMOUTH, VA.

On the night of March 18, 1850, at about eight and a-half o'clock, I was hastily summoned to visit a stout young negro man, æt. 21, who a few minutes previously, in a personal rencontre with another negro, had been stabbed in the chest. I found the man lying on the floor in a state of the most profound collapse. His body was as cold as a block of marble, and covered with profuse, cold, clammy sweat; nor could the nicest touch of the finger, or the ear applied over the region of the heart, detect either radial or cardiac pulsation. The only indication of remaining animation was the occasional utterance of groans, and the application of the hands to the pit of the stomach, as if to relieve pain in that situation. On stripping open the shirt, a wound of half an inch in size was discovered in the left precordial region, equi-distant from the nipple and left edge of the sternum, and just over the costo-sternal cartilage of the fourth rib. There was little or no hemorrhage from the wound. On introducing the probe, it could be carried as far as the costal cartilage, but was then turned a little to the right when allowed to pursue its own course. My first impression on seeing the man was that the heart had been wounded, and that the case would terminate fatally. The arrest of the probe, however, and its deflection to the right, caused me to come to an opposite conclusion. The extreme degree of collapse was attributed to other causes than the wound, viz., to the presence of crude indigestible food in the stomach, giving rise to intense pain there, or the impression of a blow or kick thereon, as suggested by my friend

Dr J. N. Schoolfield, who the next day visited the patient with me. The circumstance of his having vomited during the night a large quantity of crude ingesta, seemed to confirm this opinion. The result, however, will show that it was erroneous, and that the first impression was correct. Notwithstanding the efforts made to induce reaction, he continued in the state above described for five or six hours. At last, however, the forces of the system rallied, and by the following morning a moderate and equable reaction had taken place. No complaint was made of the wound, and there were no symptoms present to give rise to the supposition that the cavity of the chest or the heart had been penetrated. Respiration was normal, and the patient able to take a full inspiration without pain. There was, however, considerable tenderness and pain on pressure over the epigastrium, but which soon yielded to the application of a vesicant and the internal use of blue mass, opium, and ipecac. The strictest maintenance of the horizontal position, together with the observance of perfect quietude, were enjoined. This, together with the use of slop diet, constituted the treatment of the case. The wound was received on Monday night, and the patient continued to improve until Saturday, warranting the prognosis which was given of a favourable termination. On the last named day, however, in positive disobedience of orders, he went out, and used other improper exertions. At eight o'clock at night I was again sent for, and arrived only in time to see him die, which event took place in about five minutes after I reached him. He had been sitting up a few minutes previously, and conversing cheerfully, when he suddenly complained of feeling very badly, sunk down from his chair, and expired.

Autopsy, sixteen hours after death, by Dr J. N. Schoolfield and the writer. The wound in the external integuments had completely healed. On removing the sternum, together with the costal cartilages, a large quantity of serous fluid escaped from the chest. A wound of two-thirds of an inch in extent was found traversing in an oblique direction, and completely perforating, the fourth costo-sternal cartilage; passing thence through the pericardium, it was traced into the substance of the right ventricle, a line or two to the right of the septum-ventriculosum. Its farther course was traced from right to left through the wall of the right ventricle, without, however, penetrating its cavity; thence through the septum into the wall of the left ventricle, and through this into its cavity. Through the opening thus made, the blood had escaped into the pericardium, until it put a stop to the movements of the heart. About a pint and a-half of blood, partly fluid and partly coagulated, was taken therefrom. The wound through the pericardium had completely cicatrized, as well also as that of the heart for two-thirds of its extent. The preparation now in my possession shows this very plainly. A circle of inflammation about the size of a quarter dollar is there seen to surround the wound of the heart.—*American Journal of Medical Sciences*, July 1850.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

PROLAPSUS OF THE PREGNANT UTERUS. DR MERRIMAN.

We extract the following from a letter to the editor of the "Medical Times":—

In Professor Rizzoli's case, the procidentia had, as usual, been temporarily cured by the enlargement of the uterus, causing it to rise into the abdomen; but of course, when labour pains came on, the uterus was again drawn down, even to the extent of being propelled beyond the vulva—an occurrence which several cases on record have demonstrated to happen occasionally. It seems fair to suppose that the first pain produced this occurrence, combined as it was with the "raising a great burthen;" but we are not clearly informed whether any part of the uterus itself was propelled beyond the vulva, except by infer-

ence from the tearing of the perineum, and "a body" being felt to pass. How the perineum can have been torn at this time, unless the uterus with its contents had passed the os externum, I do not understand; yet it seems to have been found ruptured by the accoucheur on his arrival, before he had recourse to instruments. We are told that "a small foramen allowed a finger to pass up to the head itself." Of course the part most protruded would be the vagina; but we are not told that the os uteri was felt at all; are we then to understand that the os was fully dilated? That seems improbable; but if the practitioner had taken care to observe the facts, he would not have been so much at a loss, as he appears to have been, what treatment he should pursue. Again, we are told that attempts at reduction were vain, from the recurrence of pains; and that after some time mortification appeared imminent, from the amount of congestion observed in the protruded vagina. But to what is this congestion to be attributed? Was it not from the pressure of the distended uterus upon the veins of the displaced vagina, preventing the return of blood from it? If so, a constant firm support from the hand of the attending practitioner from the first would surely have prevented it; besides that, more than this, it would have enabled the contractions of the uterus to propel the head of the child against the os, and thus to dilate it. This, however, could not happen if no counter-pressure was exercised against the bearing down of the pains.

Medical practitioners of the present day seem too little disposed to look back and learn what others have done in times past; yet there are on record several cases of *procidentia vaginæ et uteri*, at the commencement of labour, which well deserve to be consulted and borne in mind. Let me refer to a few of these. In a case recorded by Mr James Shaw, in the "Memoirs of the Medical Society of London," (Vol. I., p. 213, A.D. 1786,) when mortification appeared commencing, the perineum was found, on examination, to be dilatable; it was, therefore, dilated by the finger; the protruded mass then slipped back, and the patient fell asleep for two hours; after which, pains again came on, and a fine boy (her fifth child) was born by the natural efforts. The woman bore subsequently two children, and never afterwards complained of more than a slight bearing down.

In the "Medical Museum," (Vol. I., A.D. 1763,) Mr Antrobus, of Liverpool, describes the case of a poor woman, who conceived of her fourth child while labouring under a "prolapsus uteri." She went her full time, although the cervix became "scirrhous" from exposure, and, at the time of labour, the first pain drew up the uterus, but the waters afterwards breaking, it was propelled down again before the head. Counter-pressure was therefore made, and the part lubricated; the child was born, and the "prolapsion" replaced. The woman did perfectly well.

Mauriceau gives the following case (Obs. 6, A.D. 1669):—The uterus had protruded during labour to the extent of half a foot, and was as big again as the head of an infant. The os looked like a phymosis, of which the lips were three fingers' breadth thick, and quite livid. He introduced his hand by degrees into the uterus, and thus guided the head in its exit. He subsequently applied a pessary.

Portal (Obs. 10, A.D. 1666) describes a case of protruded uterus in a first pregnancy, where the orifice was only half a line across. After consulting with M. Moreau, he introduced very carefully a silver sound; by this means he was enabled to insert his finger, and so dilate the orifice still more, it being, after three hours' dilatation, the size of an "*écu blanc*." After an hour's cessation, the dilatation was renewed, by both hands being carefully insinuated; the bag of waters assisted, and two pains brought the child into the world. The uterus was replaced, and all did well. The woman, previously to her marriage, had always had a "relaxation," but could easily replace the womb. During her pregnancy, however, it ceased to come down till the labour set in. In several respects this resembles the case recorded in the "Medical Times."

Smellie (Collection XVII., No. I., Case 2, A.D. 1746) describes a third la-

bour, in which the os uteri was pushed down by the pains beyond the os externum :—" During every pain I kept up the child's head, and the mouth of the womb, which I gradually dilated with my finger, till, being fully opened, it easily slipt up all round the head, and this afterwards opening the os externum by degrees, was safely delivered." Perfect (Case 77, A.D. 1766) relates a case in which there was "an entire hernia of the uterus," yet the os uteri was "so high up, that he could but just reach it with his finger in the vagina." "He gradually distended the os tincæ," introduced his hand, and, finding the feet presenting, seized them, and brought away the child.

The case related below has never, I believe, been given to the public. I have therefore the more pleasure in introducing it here, as it is an excellent example of this class of labours. The case occurred several years ago in the practice of Mr Gristock, surgeon, of Wardour Street, Soho. Mrs Purdie, the wife of a journeyman gunmaker, in Little Pulteney Street, a delicate woman, had already been the mother of two children. After the birth of the second, she was much troubled with a sensation of bearing down, and sometimes of slight protrusion of the uterus, which she endeavoured, very ineffectually, to remedy, by wearing some kind of bandage. The protrusion became greater when she next became pregnant, but diminished, and was at length wholly removed, as the uterus enlarged from gestation. When, however, the labour drew near, she was again sensible of much weakness and bearing down; and at length, when the pains actually came on, she was much alarmed by finding that a large fleshy mass had passed through the os externum with a good deal of fluid discharge. She sent for Mr Gristock, who found that not only a considerable portion of the vagina was protruded, but that the os uteri was to be felt considerably without the os externum. He ordered her to bed, had the parts freely fomented, and requested a friend to visit the patient with him. They found her much alarmed, but presenting no symptoms indicative of danger. It was, therefore, determined not at present to employ any active interference. The pains at first were not strong, but as they increased in strength, it was thought advisable to expand the hand over the protruded part, so as to make the ends of the fingers afford five points of support to the prolapsed vagina. By degrees it appeared to recede during the pains, and, at last, during a very strong pain, the whole mass slipped up into the pelvic cavity; the infant was speedily born, the placenta came away, and the uterus contracted firmly, remaining in its proper situation. This woman got quite well, and in less than two years became pregnant again; she then came under the care of Dr Hugh Ley. The same procidentia again took place, and was managed in the same way, with the same happy result. After some time she became pregnant again, but on this occasion there was no prolapsus; the child was found to present by the arm, and was turned by Dr Ley, but was still-born. A very interesting question may be asked here, namely, Did the transverse position of the child on this occasion prevent the ready subsidence of the uterus into the pelvis? I have been unable to obtain any further account of this person after this last confinement.

I cannot help regretting that the accoucheur in the case recorded by Professor Rizzoli did not practise the same form of treatment which has proved so effectual in the hands of the above-named experienced practitioners. He would then, possibly, have saved the life of the child, and have avoided the lacerations of the vagina and consequent hemorrhage recorded. Let me merely add, that I have presumed, in this criticism, that the rupture of the perineum took place before the medical man arrived; but if we suppose that, as in Mr Shaw's case, the uterus descended without tearing the perineum, the employment of the forceps would be almost certain to produce that accident.—*Medical Times*, April 7, 1850.

STATE OF THE GENITAL ORGANS DURING MENSTRUATION. BY M. JAUZER.

The researches of Bouchet, Bishoff, and others, have satisfactorily shown that menstruation coincides with the spontaneous maturation and discharge of

ova. An observation by M. Jauzer adds another to the many instances already recorded which countenance the ovular theory.

The young woman who forms the subject of the present case commenced to menstruate four days prior to her murder. The autopsy was made sixteen hours after. The surface of the left ovary presented a dark red spot, surrounded by minutely injected capillary vessels; this spot was formed by a little globular mass imbedded in the ovary. The mass in question was separated from the tissue of the ovary by a thin yellowish envelope, composed of fibres intermingled with fat globules. In close apposition to this membrane, a small irregular spherical body was seen, composed of cellular tissue and fat.

The right ovary contained two corpora lutea; the fallopian tubes were swollen, and contained a fluid, resembling and composed of epithelial scales. There was no trace of an ovule, or of spermatozoids. The uterine mucous membrane was injected, and had a velvety appearance; it was easily detached with the scalpel. It was evidently thickened, and was covered with a thin mucous secretion.

From this case it appears that the uterine mucous membrane, at the menstrual period, assumes an appearance analogous to that which it puts on during gestation, especially as regards the development of the mucous follicles.—*Medicinische Annalen;* and *Prov. Med. Journal*, Sept. 18, 1850.

EXTIRPATION OF A TUMOUR OF THE RECTUM, ENCLOSING THE REMAINS OF A FŒTUS. BY M. BOUCHACOURT.

M. Bouchacourt related, at a late meeting of the Academy of Sciences, the history of a case of tumour of the rectum, containing the debris of a fœtus, which was removed from a girl of six years of age. The case belongs to the class of double monsters by abdominal inclusion (*endocymiens*), of which M. Isidore Geoffroy St Hilaire has collected several examples in his "Histoire des Anomalies, &c." It throws some light upon the pathological anatomy of certain polypoid tumours of the rectum. Since the case seen by M. Velpeau, in 1840, this is the only one recorded of monstrosity by inclusion, that has given rise to a surgical operation similar to that required by tumours of various kinds. —*L'Union Médicale*, Aug. 29, 1850.

BILE IN THE MILK. BY M. GORUP-BESANEZ.

M. Gorup-Besanez has detected bile in the milk of a young woman, who became jaundiced seven months after delivery. The milk had a slight yellow tinge, and had an alkaline reaction. Treated with nitric acid, it became of a bluish-green colour, and afterwards distinctly green.—*Archiv. für Physiol. Heilkunde;* and *Prov. Med. Journal*, Sept. 18, 1850.

ALBUMINURIA AND PUERPERAL CONVULSIONS.

M. Cazeaux has found the urine albuminous in nineteen cases of puerperal eclampsia observed during the last five years. In forty-one pregnant women, affected with albuminuria, M. Blot has found convulsions to occur during labour only in seven; and in twenty similar cases, MM. Devilliers and Regnault have found convulsions to occur in eleven.—*L'Union Médicale*, Aug. 29, 1850.

MATERIA MEDICA AND THERAPEUTICS.

ELIMINATION OF IODINE FROM THE SYSTEM. BY DR J. C. DALTON, BOSTON, U.S.

That iodine appears in the urine is well known, and the fact is mentioned by Becquerel, Berzelius, Simon, and Christison. Becquerel comes to the conclusion that it appears in the urine constantly, whenever taken into the system, and that its presence is very easily detected. The method which he adopts is to mix with the urine an emulsion of starch, and add to the mixture

chlorate of potass and sulphuric acid, which, liberating the iodine from its combinations, allows it to strike the characteristic blue colour with starch. Christison recommends the addition of chlorine, by allowing it to descend in a gaseous form, from the mouth of a bottle containing nitro-muriatic acid. In ordinary cases, however, the admixture of starch with the urine, followed by a drop or two of nitric acid, to set free the iodine, will be found sufficient.

I have observed the appearance of iodine in the urine in seven cases, and in many of them it was detected at the same time in the saliva. In order to determine the time which is necessary for iodine to appear in the urine, the following experiments were instituted :—On the 21st of February, at five P.M., I took half a drachm of the syrup of the iodide of iron, and examined the urine afterwards, at intervals of ten minutes. The portions passed at ten and twenty minutes past five showed no indication of iodine ; but at half-past five a strong purple colour was produced by the addition of starch and nitric acid. This reaction continued throughout the evening, and urine passed at seven the next morning struck a deep indigo with the same reagents. At nine A.M. the colour was less marked ; and from this time it continued to grow fainter and fainter, till forty minutes past four P.M.,—nearly twenty-four hours from the time of taking the medicine,—after which no further reaction was perceptible.

The next observation was made with regard to the accumulation of iodine in the system, and the time necessary for eliminating a large quantity. Two patients, in the Massachusetts General Hospital, had been taking iodide of potassium, one for six, the other for eight, weeks. During the greater part of this time, they both took the medicine to the amount of one drachm per day. On the 24th of February, the medicine was omitted for each. The urine of both patients at this time gave a nearly black colour, with starch and nitric acid. It was then examined every twelve hours, soon after being passed. In seventy-two hours the colour produced was very faint, so that in one case it was doubtful whether it existed or not ; and in the other it was distinct on first dropping in the nitric acid, but disappeared by agitation. In twelve hours more, all trace of iodine had disappeared from both specimens.

From these, and other observations, may be deduced the following conclusions :—

I. After a single moderate dose, iodine may appear in the urine in so short a time as thirty minutes, and continue to appear for nearly twenty-four hours afterwards.

II. After a much larger dose, taken habitually for a long period, the time necessary for complete elimination is not proportionally increased.

III. It is therefore probable that iodine, or at least iodide of potassium, does not accumulate in the system in any considerable quantity; and it is questionable whether a large dose, taken habitually, produces any more constitutional effect than a moderate one, since the superfluity is constantly removed by the kidneys.

IV. The colour, produced by the reaction of starch with urine containing iodine, varies, according to circumstances, from a light purple, slaty blue, or French gray, to deep indigo, or absolute black.

V. A solution of iodide of potassium in distilled water, in the proportion of one-eighth of a grain to the ounce, produces, with starch and nitric acid, an opaque black colour, as strong as is ever observed in urine. A solution, in the proportion of one-sixteenth of a grain to the ounce, produces a strong purple, moderately translucent.

VI. It is, therefore, probable, that iodine is often excreted in the urine in about as large a quantity as one-tenth of a grain to the ounce ; since one-eighth of a grain of iodide of potassium contains nearly one-tenth of a grain of iodine.

The fact stated in the third proposition—viz., that iodine does not accumulate in the system to any great extent—is corroborated by the opinion of Becquerel, who states ("Séméiotiques-Urines," p. 128) that, "when its use is

continued, and particularly when the patient begins to be saturated with it, its passage is very abundant, and a large proportion of the iodine introduced into the system is eliminated through the urinary passages." The appearance of an eruption of acne about the face may, perhaps, be considered as an indication that this point of saturation has been reached; and it is thought that this effect will, in general, be produced by giving the medicine [iodide of potassium ?] in doses amounting to about half a drachm per day.

With regard to the rapid elimination of iodine, even after it has been given in large doses and for a long time, there is an experiment, related by Dr O'Shaughnessy, which might seem to weigh against this supposition. The instance was that of a dog, poisoned with iodine, in whom the poison was detected in the urine so late as the fifth day, though only one dose had been given. The drug was administered, however, in this instance, in the form of solid iodine, and in the quantity of one drachm; so that it is possible a part of it remained in the alimentary passages for a day or two, without being absorbed, and in this way had the effect of repeated doses.

As already mentioned, iodine is usually perceptible in the saliva at the same time that it appears in the urine; but it does not always exist in these two secretions in the same proportions. Thus, in one instance, the urine showed only a faint purplish colour by the reagents, while the saliva exhibited a strong blue. The same, or even greater, irregularity was observed by Dr O'Shaughnessy, in the case of the dog above referred to, since the poison was abundant in the saliva on the third day, but could not be detected in the urine; on the fourth and fifth days, however, it again appeared in the latter excretion. Becquerel (p. 128) speaks particularly of these occasional variations, and acknowledges that we cannot, at present, refer them to any fixed law.—*American Journal of Medical Sciences*, July 1850.

NEW ANTIPERIODICS.

M. Delioux, professor of materia medica at Rochefort, has published a note, in which he maintains that chloroform is a powerful succedaneum for cinchona. He does not profess to offer a remedy more powerfully antiperiodic than quinine or arsenic; but thinks it important to discover something less expensive than the former, and less hazardous in large doses than the latter. M. Delioux having heard that ether had been proposed as a remedy in intermittents, and having observed that the phthisical patients to whom he had given chloroform were often relieved from very troublesome febrile complications, determined to try it in the periodic fevers so common at Rochefort. The advent of the cholera diminished for some time the amount of cases of intermittent in the hospital, but a sufficient number were treated with the chloroform, and with such a regularity of success that he feels warranted in recommending it as a powerful antiperiodic. The chloroform was given in doses of from nine to thirty grains, according to the severity of the case. The patients took it several hours before the access, and continued its use for several days, as with other febrifuges. The chloroform was estimated by weight instead of dropping, as the latter does not permit the dose to be regulated with accuracy. The patients experienced no inconvenience from the remedy beyond a little heat in the throat. To make a good mixture it is to be rubbed up first with syrup, and then it mixes readily with water.—*Journal de Med. et de Chirurgie Pratiques*, July 1850.

The other new febrifuge is a well-known native of France, *Physalis Alkekengi*, or winter cherry, which M. Gendron, of Chateau de Loir, proposes as a remedy for intermittents. He says that the whole plant, twigs, leaves, capsules, and berries, possess the antiperiodic qualities of cinchona.—*Gazette Méd. de Paris*, 20th July 1850.

[Our French neighbours are at present quite on the *qui vive* as respects antiperiodics,—no doubt stimulated by the prize of 4000 francs offered by the Society of Pharmacy for the discovery of a substitute for quinine, and to which the French Minister of War has offered to add an equal sum. Great caution

is required before deciding on the antiperiodic virtues of any remedy. The mere residence in hospital will often stop the progress of intermittents, and no case should be admitted in evidence until it has been under observation for some days, and the disease is seen to be uninfluenced by the improved hygienic circumstances of the patient. There is no advantage in estimating chloroform by weight; measuring is much more calculated to ensure accuracy.]

PROCESS FOR ESTIMATING THE QUALITY OF OPIUM. BY M. DE VRY.

M. Guillermond has published a process for determining the quantity of morphia contained in opium (see vol. x., page 575 of this Journal). As the morphia obtained by that process contains some of the narcotine of the opium, I have adopted the following modification of the process. The mixture of morphia and narcotine obtained by precipitating the alcoholic solution with ammonia, after being washed, is heated with a slight excess of sulphate of copper dissolved in pure water. I have found, from experiment, that morphia decomposes sulphate of copper into sulphuric acid, with which the morphia combines, and tribasic sulphate of copper, which is an insoluble salt, while the narcotine has no action on sulphate of copper. In this way we obtain a solution of sulphate of morphia with a little sulphate of copper, while the narcotine remains undissolved with the sub-sulphate of copper. After filtering the liquor, the copper is precipitated by sulphuretted hydrogen, and the morphia by ammonia. With this slight modification, the process of M. Guillermond is not only a good one for determining the proportion of morphia contained in the opiums of commerce, but it may also be advantageously applied for preparing the alkaloid, which is thus obtained pure and almost without colour.—*Pharmaceutical Journal, August*, 1850.

ON COD LIVER AND SKATE OIL. BY M. JULES PERSONNE.

The author has communicated to the French Academy of Medicine a long memoir on this subject, for the details of which we have not room, but of which the following are the principal points.

It is well-known, from the experiments of Girardin and others, that cod-liver and skate-liver oil contain iodine, and it is thought to exist as iodide of potassium. The therapeutical effect of these oils was attributed to the presence of this substance. More lately, M. Gobley announced that the skate-oil contained phosphorus; and he ascribed its virtues to the presence of this constituent.

M. Personne examined two different kinds of cod-liver oil which are in commerce. The one most commonly used in the Parisian hospitals is dark brown, rather thick, of disagreeable taste, and containing flakes which render it turbid. The other is the pale transparent oil of English commerce, with no disagreeable taste.

He finds that the dark oil contains more iodine than either the pale English oil or the skate-liver oil; and finds that the liver of the skate, from which the oil has been extracted, retains a larger proportion of iodine than the oil itself contains.

As to the state in which the iodine exists, he thinks that it is not as iodide of potassium, but that it exists as iodine, in intimate combination with the oleaginous principle. He founds this conclusion, however, not on direct experiment, but on the analogy of what he has observed in ointment of iodide of potassium, which first becomes brown from liberated iodine, and then becomes white without any iodide escaping, from which he infers that the iodine has entered into combination with the fatty principle.

He finds that Gobley is wrong in supposing any of these oils to contain phosphorus. The presence of that element has arisen merely from the oil containing some flakes of the tissues of the livers, which abound in phosphates.

M. Personne, then, assuming that it is of importance to fix definitely the quality of the oil as regards the iodine it contains, proposes to prepare an iodu-

retted oil artificially, as a substitute for cod-liver oil. For this purpose he dissolves 1 part of iodine in 100 of olive or almond oil. The mixture becomes brown; but in thirty-six or forty hours resumes its natural colour. It again, however, becomes brown, and he eventually renders it permanently pure by agitating it with a solution of bicarbonate of soda. The oil thus prepared is acid, which is owing to the presence of hydriodic acid. A large proportion of the iodine is removed by the bicarbonate of soda, and the oil contains ultimately one half part (0·5) of iodine per cent. — *Gazette des Hôpitaux*, August 22, 1850.

[Even if it were a matter of importance, M. Personne's process appears to be both troublesome, and, as regards loss of iodine, expensive. It has therefore nothing, in an economical point of view, to recommend it.

But the whole affair proceeds upon the doctrine that it is the iodine which is the therapeutic agent, and to this, we believe, few who have had any experience of the use of cod-liver oil will assent. If this were true, why make a preparation of iodine with oil at all?—any other solvent would answer better.

M. Personne states unhesitatingly, that it has indubitably been established that the brown, nasty tasted, cod-liver oil is efficacious, but that little good has come from the use of the pale and more agreeable oil of English commerce. He here proceeds in complete ignorance of the fact, that the dark brown oil is not now used in this country, and that most satisfactory results are obtained from the clear pale oil. The latter he has shown to contain less iodine than the former, and thus English experience shows that the amount of iodine present does not influence the therapeutic value of the oil; and, therefore, his whole proposal to make an artificially ioduretted oil, is useless.]

A CHEAP AND SIMPLE ARTICLE OF NOURISHMENT FOR INFANTS—CARROT JUICE. BY DR GUMPRECHT OF HAMBURG.

Being impressed with the consideration, that the nature of the artificial nourishment of young children deprived of the breast has a most important effect on their health, the author was anxious to find some aliment more supportable by the tender organism of infants than cow's milk and amylaceous matters, which mostly tend to acidity. An observation of Schmidtmann, that the pulp of carrots is a favourite article of diet for young children in Turkey, and is much used there, led him to turn his attention to this substance, which, as is well known, from the analyses of Wackenröder and Liebig, is particularly rich in albumen and sugar. It contains, however, also a quantity of ligneous fibre (cellulose), which is indigestable, and may therefore prove injurious.

He therefore thought of employing the thickened juice of carrots in the following way:—An ounce of finely rasped carrot pulp is mixed with two cups of cold *soft* water, and left for twelve hours, during which it is occasionally stirred, then strained through a sieve, and the juice expressed from the pulp. This juice is then mixed with a sufficiency of bruised biscuit, or bruised crust of white bread, or a little arrow root, a little sugar added, and the mixture heated, but not allowed to boil completely, so that the albuminous matter may not be coagulated. More sugar may be added if required.

The author considers the addition of biscuit or bread to be necessary, in order to furnish to the child all the requisite elements of nutrition,—viz. albumen, starch, gluten, sugar, fat, and salts.

For sucklings deprived of the breast, the preparation is so far altered, that the biscuit (1 part) is rubbed up with the carrot pulp (4 parts), then macerated and strained as above (we presume it is to be heated, though the author does not expressly say so). To this juice the requisite amount of sugar, and a little salt, is added, and the child fed with it from a sucking bottle. Of course great cleanliness is requisite to prevent acidity being generated in the bottle, and for the

same reason the juice, which readily undergoes fermentation, is to be kept in a very cool place, and prepared in small quantities at a time.

So far as the author's observations have gone, this system of nourishment has answered well, the children not only bearing it well, but taking the food readily, and he has received assurances of similar success from several of his professional brethren.

It is further stated by him, that for older children the carrot pulp may be mixed with animal broth, and that large carrots answer better for the preparation of the juice than those which are young and small.

From the want of any direct experience of the above method, we cannot as yet express a judgment on the above proposition ; but we must admit that the manufacture of the food does not appear (as the author seems to think) so easy that we can always reckon on its being successfully prepared. The chances of fermentation in such a juice are so great, that it is to be feared it will hardly be avoided. We should regard any tendency to laxity of the bowels—so common an affection of children—as a complete contra-indication to its use. Further, there seems to be no reason why, even for young infants, some animal matter should not be added, in the form of broth or yolk of egg.—*Schmidt's Jahrbücher*, from *Journ. für Kinderkrankheiten*, July and August 1849.

In the "Journal für Kinderkrankheiten" for March and April 1850, Dr Gumprecht publishes letters from Dr Müller of Hamburg, Dr Mauthner, of the Children's Hospital of Vienna, and Dr Helmbrecht of Brunswick, who state that they have followed his plan with satisfactory results. The last-named physician, however, remarks, that if arrow root is used to thicken the juice, it must be boiled, else the arrow root will not dissolve, and this has the effect of coagulating the albuminous matter, which the author wishes to avoid. Dr Helmbrecht suggests salep,—a substance which is little used in this country, but which, we believe, is unjustly neglected.

ON BREAD. BY DR F. KELLER.

Most of the rye-bread, especially that baked in the country, where the leaven is often left to itself for a whole week, has an unpleasant acid taste, which, according to the process of baking, can only originate from the presence of either *acetic* or *lactic acid*. To determine which of these acids is formed, an infusion of bread, prepared by washing slices of dried bread, was subjected to distillation, and the slightly acid distilled liquor, after being neutralised with carbonate of soda, evaporated in the water-bath. The crystalline form of the salt, which was formed on a watch-glass, was that of acetate of soda. The silver salt obtained from it yielded, on an average of two experiments, 65·11 per cent. of metallic silver, the acid was consequently *acetic acid*, and, according to this, the pores of the new baked bread are lined with a thin film of spirit of wine, generated during the fermentation, but which soon changes into the above-mentioned acid.

The residue was treated with alcohol and ether, in order to remove the gum and sugar, and then examined for lactic acid, but without success.

The analysis of the rye-bread used in the experiment, was,

Water,.....		49·64
Unaltered starch,.....		36·55
Nitrogenised extract with	starch sugar,.....	4·55
	starch gum,.....	3·69
Vegetable fibre,.....		4·40
Salts,.....		1·17
		100·00

During the process of baking, part of the insoluble gluten contained in the flour passes over into the soluble modification of it ; the proportion of nitrogen in the extract of bread was two per cent.

The bread which had been perfectly exhausted by water, and which, for the

most part, consisted of unchanged starch, left behind, on being treated with diluted sulphuric acid, a nitrogenised substance, which was very analogous to the hordein of barley.

The proportions of nitrogen in rye-bread are as follows :—That of the dried bread was 1·8 per cent., that of the extract 2 per cent., and that of the exhausted bread 1·6 per cent.

The exhausted bread forms 79·7 of the anhydrous bread, and the extract 17·93 ; the proportion of nitrogen is therefore calculated to be—

For the first,.....	1·31
For the latter,.....	0·37
	1·68 per cent.

which agrees nearly with 1·8.

The proportion of nitrogen in bread calculated by 100 parts, is—

In the extract,.....	22 per cent.
In the residue,.....	78 per cent.

—*Pharmaceutical Journal*, from Buchner's *Repertorium*, Bd. iv. H. 3, P. 336.

Part Fifth.

MEDICAL NEWS.

SIR B. BRODIE'S CONDEMNATION OF THE LIGATURE.

At a dinner given to him by the President and Council of the Dublin College of Surgeons,[1] Sir B. Brodie is reported to have said, "In their steps, the English college was glad to follow * * *. The mode of curing the formidable disease of aneurism by a bloodless operation was brought into notice by Irish surgeons, and would, he was convinced, supersede every other plan. Such triumphs as these secured its pre-eminence for the school of surgery of Ireland, and he trusted it would be long enjoyed." And "Before the evening closed, Sir Philip Crampton, alluding to the instance of an Irish improvement in surgery which their eminent guest had adverted to, took the opportunity of presenting Sir Benjamin Brodie, in the name of the council, with a gift of trifling value, but which perhaps he might be gratified to receive. It was a complete set of the instruments used in the treatment of aneurism by compression, of which, together with a copy of Dr Bellingham's work on the subject, he requested Sir Benjamin's acceptance."

Our first impression on reading this statement was, that there must have been some error on the part of the reporter. But when it appeared in newspaper after newspaper, and journal after journal, without contradiction from the gentleman principally concerned, any doubt as to its authenticity could no longer be entertained. We then resolved to treat it as a mere exchange of compliments ; but, upon second thoughts, felt that we could not, consistently with our duty, pass it over so lightly.

Ever since the Dublin surgeons attempted to revive the use of pressure as a substitute for ligature of the femoral artery, it has been maintained in this Journal, that the patient's welfare and the credit of surgery would be promoted by perfecting the latter method, and seriously compromised by adopting the former. The bad consequences of hemorrhage and phlebitis were attributed to the effects of faulty procedure in exposing and tying the vessel, through

[1] Dublin Evening Post, August 31, 1850 ; and public press *passim*.

want of attention to the sources of danger, and an erroneous idea that delay in overcoming the difficulties of execution beyond the limits of a brief period, implied want of skill in the operator. With due regard to the precautions which have been again and again fully explained, it now appears, from the extensive experience of different surgeons, that the operation may be held absolutely free from danger; while, to say nothing of the failures, amputations, and fatal issues, *which have not been published*, the records of surgery already contain abundant evidence to show, that the warning we gave the sanguine advocates of pressure was not without good foundation.

Such being the state of the question, the late senior surgeon of St George's Hospital—the hospital in which John Hunter tied the femoral artery for aneurism, and thus laid the foundation of all those great operations which have shed so much honour upon British surgery—has publicly pronounced his approbation of pressure, in terms so strong and unqualified, as to imply a complete condemnation of the ligature. From the position which Sir Benjamin Brodie occupies in the respect of his profession, this judgment must tend to influence the opinion of his younger brethren; and we therefore think it requisite to express again, all the more strongly, our conviction, that pressure, however carefully and skilfully employed, must always be a tedious, uncertain, and dangerous method of treating aneurism; and that the ligature admits of being applied so as to afford a remedy, speedy, effectual, and safe.

ARMY MEDICAL DEPARTMENT.—RETIREMENT OF SIR JAMES M'GRIGOR, BART.

This distinguished Medical Officer, who has so long and so ably fulfilled the duties of Director-General of the Army Medical Department, has now, in his eightieth year, exchanged the anxieties and responsibilities of office for the repose of domestic and social life, to which he is so well entitled.

Few have presided over any Department of the public service who have done so much (and endeavoured to do more) to elevate its character and to extend its usefulness; and few have so well earned an honourable retirement, by unremitted exertion and unwearied zeal, exercised in this country's service, during a period of fifty-six years, and in every quarter of the globe.

This is not the time to enter into any lengthened detail of Sir James M'Grigor's Professional and Official career, but we cannot omit to notice his skilful and judicious conduct as Chief of the medical staff of the Anglo-Indian army, in the remarkable expedition from India to Egypt, in 1800; and his services, more recently, as Head of the medical department of the army in the Peninsula. In this latter capacity, by his admirable arrangements and provision for the sick and the wounded, aided by the ability and zeal of the army medical officers, Sir James not only obtained for himself the fullest approbation and confidence of the Duke of Wellington, but successfully vindicated the claims of the Medical Department to be noticed, and thanked for their services, in the Public Despatches. In the records of the Peninsular war, the name of Sir James M'Grigor will ever be honourably connected with that of the illustrious Commander-in-chief, and the Medical Department no less identified with the fame of the British army, in one of the most eventful and glorious periods of its history.

In his official career, we shall only at present notice the establishment of the admirable Museum and Library at Fort Pitt, Chatham, and the Collection of the valuable annual reports from medical officers in the various quarters of the globe, preserved and arranged in the Record office of the Medical Board, both of which are open to the professional public, as evincing his unceasing desire to promote the improvement of the medical officers of the army, and the extension of medical and general science.[1] With the same object, he promoted the formation of libraries

[1] These Records bear ample testimony to the zeal and industry of the Medical Department, and furnish the materials for the valuable Statistical Reports prepared by Col. Tulloch, and by that indefatigable medical officer, Dr Henry Marshall, published by the authority of Parliament.

at the army head quarters of our Colonial possessions, and gave every encouragement to the cultivation of Natural Science by the medical officers in the different regions of the world. Sir James was also the founder of the Friendly and Benevolent Societies for the benefit of Widows and Orphans of Army Medical Officers.

Sir James M'Grigor has received, as might have been expected, numerous testimonies of respect and regard from the medical officers of the army. At the conclusion of the Peninsular War, he was presented with a service of plate of the value of 1000 guineas, with an inscription expressive of their high admiration of his conduct as Chief of the Medical staff in the Peninsula, and of their respect and esteem for his public and private character. His portrait has been twice painted at their desire, once by the late J. Jackson, R.A., presented to Lady M'Grigor, and again by his friend, Sir David Wilkie, placed in the library at Chatham. We shall only add the crowning testimony of the Duke of Wellington in his memorable Despatches:—"I have every reason to be satisfied with the manner in which Dr M'Grigor conducted the department under his directions; and I consider him one of the most industrious, able, and successful public servants I have ever met with."

Nor is Sir James M'Grigor without honours, in civil life, from his fellow-countrymen. He was twice elected Lord Rector of Marischal College, Aberdeen. The University of Edinburgh conferred on him the degree of Doctor of Laws,—a degree rarely granted by them; and the Town-Council made him an honorary burgess of the Corporation. He is a Fellow of the Royal College of Physicians of Edinburgh; an Honorary Fellow of the Royal College of Surgeons of Edinburgh, of the Faculty of Physicians and Surgeons of Glasgow, and of the Royal Medical Society of Edinburgh. These are his chief Scotch titles. For a list of his other honours, and of his contributions to medical literature, we beg to refer to a recent biographical sketch in the Lancet.

It is with pleasure we have learned that Sir James M'Grigor, though suffering under some of the infirmities of age, is in possession of good general health, and of much mental vigour; and, knowing the affectionate comfort he enjoys in domestic life, we trust that, with God's blessing, he may spend the evening of his useful life in calm enjoyment and serenity.

"THE MONTHLY JOURNAL AND ITS CONTRIBUTORS."

Under the above, or like headings, the Lancet has been indulging its readers, for some weeks past, with renewed abuse of us, in reference to Mr Rankin's case of turning in a labour impeded by a tumour in the pelvis. We recur to the subject, to notice only one point, in one of the various letters which the Lancet has thought proper to admit. We distinctly stated, that, in publishing the case, we did not alter Mr Rankin's own narrative of the *facts*.[1] In a letter to the Lancet of October 5th, Mr Rankin roundly avers, in answer to us, that a "*part of the narrative—an important part too—is entirely left out.*" He supplies this part to the Lancet; and we quote it:—"Next day I examined the vagina and detected an opening, small compared with the size of the dis-

[1] Before sending the narrative of the case to the press, the only even verbal alterations which we made in the narrative were the following seven,—viz., supplying the words "her," "the," "of the child," "with the crotchet," and "amnii," which were wanting in Mr R.'s manuscript, but necessary for the grammar and sense; and changing "vagina" to "maternal canals," and "was" to "seemed." Mr Rankin has printed and circulated privately a "corrected" copy of his case, in which he adopts all these alterations of ours except the last. Nay more. In this new edition, he has himself cut off as "irrelevant" all those portions of the concluding remarks which we had cut off—so far, also, acknowledging the justice of our editorial judgment in originally omitting them; and he has even curtailed the case of those few introductory observations of his own, which we had retained as a preface to the narrative. In this last point he has, we willingly confess, shown more judgment than we did, when we printed and retained them.

eased parts, which must have passed through it ; yet after contraction *it freely admitted two fingers.* It was irregular in form ; the parts around were indurated; and with the aid of the speculum they were found to be inflamed. The bulk from the tumour above the pelvis had entirely disappeared ; but on pressure upon its former seat, and counter-pressure from within the vagina, there was still considerable fulness, which might arise from induration of the parts concerned in the recent separation and rejection of the morbid structure."

We do not stop to discuss whether it was at all probable that Mr Rankin could, as he thus fancies he did, pass two fingers into his patient's peritoneal cavity without cause and with impunity. We notice the latter to make a moral and not a medical observation,—namely, that the paragraph in question was *not,* as Mr Rankin avers, left out by us, for no such paragraph was ever sent to us, as we can prove to Mr Rankin by the manuscript of this portion of his own paper, which fortunately happens to be preserved. We willingly abstain from the very painful duty of offering any remarks upon the "morality" (to use Mr Rankin's own favourite phrase) of thus deliberately manufacturing a supposed paragraph, in order to have some semblance of ground for his own angry allegations against us.

NOMINA EORUM QUI GRADUM MEDICINÆ DOCTORIS IN ACADEMIA JACOBI SEXTI REGIS, QUÆ EDINBURGI EST, ANNO MDCCCL. ADEPTI SUNT.

**** Those who have obtained Prizes for their Dissertations.*
*** Those selected for Competition for the Dissertation Prizes.*
** Those Commended for their Dissertations.*

Angus, Joannes, Scotus. On Disordered Menstruation.
Armstrong, Fergus, Anglus. On Croup.
Arnold, Fenwick Gulielmus, Novo-Brunsvicensis. On the Surgical Treatment of Obstructions of the Mucous Passages.

**Balfour, Hugo Melville, Scotus. On Pulmonary Consumption.
Bell, Joannes, Scotus. On Amenorrhœa.
Bethune, Normanus, Canadensis. On Spina Bifida.
Booth, Carolus, Anglus. On the Therapeutic Action of Hydrocyanic Acid.
Boyd, Joannes, Scotus. On Acute Hydrocephalus.
Burn, Georgius, Scotus. On Alcohol.

Cameron, Angus, Scotus. On the Influence of Climate on the Human Race.
Capie, Jacobus, Scotus. On Disturbed Balance of the Circulation.
Carstairs, Jacobus Grafton, ex India Orientali. On Vegetable Embryology, or the Act of Impregnation in Plants.
Challacombe, Joannes Pince, Anglus. On Epidemic Cholera.
Checkley, Robertus, Hibernus. On Sclerema.
Christie, Jacobus, Hibernus. On Erysipelas.
***Christison, Alexander, Scotus. On Cannabis Indica.
Crichton, Robertus Orr, Scotus. On the Degeneration of the Solids and Fluids of the Body.
Crocker, Henricus, Anglus. On Pericarditis.
Dalzel, Gulielmus Fredericus Blyth, Scotus. On the Physiology of the Venous Circulation.
**Duke, Abraham, Anglus. On Tubercle.
Falshaw, Gulielmus, Anglus. On Diseases of the Heart.
*Ferguson, Franciscus Jacobus, Anglus. On the Morbid Appearances in the Bodies of the Insane.
Foulerton, Joannes, Scotus. On Phthisis Pulmonalis.
*Freer, Thomas, ex Insula Mona. On Variola.
*Gilchrist, Jacobus, Scotus. On the Influence of Clothing upon Health.

*Harley, Georgius, Scotus. On Oleum Jecoris Aselli.
**Hartley, Jacobus, Anglus. On Gout.
Harwood, Carolus, Anglus. On Continued Fever.
**Holland, Thomas Seward, Hibernus. On the Pathology and Diagnosis of Aneurism of the Arch of the Aorta and Arteria Innominata.
30 Holmes, Gulielmus Marshall, Anglus. On Intermittent Fever.
Hutchinson, Jacobus, Scotus. On the Evidences of Pregnancy obtained by the Microscope and Stethoscope.
James, Robertus Wallace, ex India Orientali. On Uterine Hemorrhage.
Johnson, Joannes, Anglus. On Aneurism.
Johnston, Ludovicus, e Nova Scotia. On Exanthemata.
35*Lake, Georgius Anthony Kersopp, Anglus. On Arsenic, considered as a Poison.
Long, Jacobus, Hibernus. On Ulcers.
Lord, Joannes, Anglus. On the Fasciæ of the Female Pelvis.
Macfarlane, Jacobus, ex Insula Sancta Lucia. On Fever.
Macfarlane, Gulielmus, ex Insula Sancta Lucia. On the Plague.
40**M'Gregor, Angus, Scotus. On the Operation of Moral and Physical Influences through Parents on their Offspring.
*Mackenzie, Laurentius, Scotus. On Bright's Disease of the Kidney.
M'Watt, Robertus Carolus, Scotus. On the Urine.
***Marcet, Gulielmus, Genevensis. On Fermentation.
Milner, Ebenezer, Scotus. On Catalepsy.
45 Munro, Donald, Scotus. On Rheumatism.
***Nelson, Henricus, ex India Orientali. On the Development of Entozoa.
Peters, Edmundus Henricus, Anglus. On Phthisis Pulmonalis.
Pinkerton, Archibaldus Gulielmus Pulteney, Scotus. On the Various Modes of Fatal Termination of Diseases.
Robertson, Alexander, Scotus. Observations on the Purulent Ophthalmia of Infants, and the Scrophulous Ophthalmia.
50 Rogerson, David, Scotus. On Tubercle.
*Rolston, Joannes, Anglus. On Malignant Cholera.
*Rowe, Thomas Smith, Anglus. On the Intermingled Relations of Mind and Body.
*Scholefield, Gulielmus Smith, Anglus. On Combination as an Instrument of Progress and Verification in Medical Science.
Schultze, Alexander, Scotus. On the Connection between Apoplexy and Disease of the Heart.
55 Snowdon, Samuel, Hibernus. On Phlebitis.
Spittall, Andreas, ex Insula Mona. On Delirium Tremens.
*Struthers, Alexander, Scotus. On Premature Labour and Accidental Hemorrhage.
Tait, Dalhousie, Scotus. On the Pathology and Surgical Treatment of Aneurism.
Thomson, Carolus, Scotus. On Bright's Disease of the Kidneys.
60 Thomson, Georgius Spears, Scotus. On Rheumatism.
*Traill, Gulielmus, Scotus. On Dyspepsia.
Wason, Edmundus Sidney, Scotus. On Stricture of the Urethra.
Webb, Franciscus Cornelius, Anglus. On the General Anatomy, Physiology, and Development of the Arterial System.
White, Gulielmus Alexander, Scotus. On Acute Laryngitis.
65 Wilkin, Jacobus, Scotus. On Stricture of the Urethra.
Wood, Nathaniel Stenson, Anglus. On Spontaneous Evolution of the Fœtus in Utero.
67 Young, Jacobus, Hibernus. On the Predisposing Causes of Disease.

VARIETIES.

Royal College of Surgeons of Edinburgh.—At the annual elective meeting of the College, held in October, Mr Syme was re-elected to fill the presidential chair for another year.

Royal Infirmary.—Vacancies in the surgical staff of the hospital, caused by the retirement of Professor Miller, whose period of service has expired, have been filled up by the promotion of Dr Dunsmure to the post of surgeon, and the election of James Spence, Esq., F.R.C.S., as an assistant-surgeon to the institution.

Monument to Jenner.—Mr Marshall, the eminent sculptor, is going on with a cast of the intended monument to Jenner, which is to be a bronze statue, placed in the open air, in some situation in London, yet to be selected or obtained. He has been in communication with Dr Baron, from whom he has received the original bust of Jenner. Mr Marshall expects to be able to finish the cast so as to place it in the great exhibition of 1851; and he hopes to complete the work in bronze in 1852. No efforts have yet been made to raise subscriptions, but the committee trust that the feeling of the profession and the public will be warmly evinced when appealed to.—*Provincial Medical Journal*, August 21, 1850.

Death of M. Tanchou.—This eminent Parisian practitioner is said to have recently fallen a victim to one of the quackeries of the day. He believed that the topical application of cold water was a specific against acute rheumatism; and seems to have induced a fatal chest affection, by endeavouring to illustrate his theory on his own person, while suffering from arthritic symptoms.—*L'Union Médicale.*

Revaccination in the Prussian Army.—Every recruit is vaccinated on admission into the Prussian army. In the year 1848, 28,859 soldiers were vaccinated. In 16,882 of these the vaccine disease was regular; in 4404, irregularly developed; and in 7573 the process of vaccination was ineffectual.—*Medical Gazette*, August 16, 1850.

A Hidden Compliment.—The "London Medical Gazette" has applied to us the title of "ill-favoured critics." We suppose this is from finding that, when unjustly attacked, we are apt to prove what in vulgar language would be called "ugly customers."

PUBLICATIONS RECEIVED.

Hydrocephalus Reconsidered, and its Relations to Inflammation and Irritation of the Brain Defined, &c. By Thomas Weeden Cooke, M.R.C.S., Eng. London, 1850.

The Assurance Magazine. No. I. September 1850. London, 1850.

On Inflammation of the Veins. (Jacksonian Prize Essay, 1850.) By Henry Lee, F.R.C.S., &c. London, 1850.

Contributions to the History, Diagnosis, and Treatment of Croup. By John Ware, M.D. (From Boston Medical and Surgical Journal.) Boston, 1850.

Appendices to the Reports regarding the Royal Infirmary of Edinburgh, for 1847-8-9.

On Catalepsy or Trance. By Ebenezer Milner, M.D. (From Edinburgh Medical and Surgical Journal, October 1850.)

A Manual of Qualitative Analysis. By R. Galloway, F.C.S. London. Churchill. 1850.

Address to a Medical Student. Second Edition. Oxford. Parker. 1850.

Remarks on Insanity: its Nature and Treatment. By Henry Monro, M.B., Oxen. Part I. London. Churchill. 1850.

An Address Delivered at St Bartholomew's Hospital, on the Opening of the Medical Session of 1850-51. By Charles West, M.D. London. Longman & Co. 1850.

On the Identity or Non-Identity of Typhoid and Typhus Fevers. By William Jenner, M.D., London. Churchill. 1850.

Epidemics Examined and Explained. By John Grove, M.R.C.S.L. London, 1850. Pp. 192.

An Introductory Address Delivered at the London Hospital Medical School at the Opening of the Session 1850-51. By Nathaniel Ward, F.R.C.S.E. London, 1850.

On the Structure of the Spleen. By William R. Sanders, M.D., &c. Edin. Sutherland and Knox. 1850.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr C. F. Sloan, Ayr; Dr John Gray, Perth; and Robert Smith, Esq., Aberdeen. Others have been privately acknowledged.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Comparative expediency of Lithotomy and Lithotrity, with reference to the use of Chloroform.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

IN the comparisons hitherto instituted between lithotomy and lithotrity, there have been urged against the former operation the pain attending, and the danger directly following its performance; while to the latter have been objected its uncertainty of effect, and the consequently frequent occasion for its repetition, with the risk thus incurred of irritating the bladder, so as to excite a train of symptoms more serious than those of the stone itself. The introduction of chloroform has afforded the means of obviating the first objection to lithotomy; and if this can be done without any counterbalancing evil, the grounds of question will be so much altered as to require serious reconsideration.

Loss of appetite and acceleration of the pulse are most unpleasant indications of the patient's state after lithotomy; and as these are very usual effects of chloroform, there was naturally a reluctance to give an operation so important even the appearance of going wrong. For my own part, though early induced to afford children the benefit of immunity from pain thus obtained, as they seldom suffer any serious effects from the operation, I have only lately ventured to treat adults in the same way; but am now so satisfied with the safety and advantage of doing so, that I shall certainly have no scruple for the future.

The first case of lithotomy in which I used chloroform was that of a boy, whom I cut in the North London Hospital during my incumbency at University College. For several days after the operation he suffered great derangement of the digestive organs, but made a good recovery. These disagreeable symptoms may have been owing to some fault of the chloroform, which had been recently introduced, and was then manufactured in London of very indifferent quality. But the following case, which occurred soon after my return to Edinburgh, is calculated to excite a doubt as to this explanation. A young gentleman, who came a distance of no less than 12,000 miles, suffering from symptoms of stone, had a pretty large one removed, with perfect facility, in a state of profound

sleep, and went on so well for a fortnight, that I had almost ceased to regard him as a patient, when one evening I was suddenly informed that he appeared to be dying, and accordingly found him with cold extremities, a hardly perceptible pulse, and other signs of the most alarming character. Under the use of stimulants, reaction soon took place, and then, although considerable derangement of the digestive organs continued for several days, the healing process went on favourably to the most satisfactory completion. Now, if this attack had occurred within a day or two after the operation, it would of course have been ascribed to the chloroform, with which it certainly could have had no connection. On the 27th of March last I operated upon a boy of thirteen in the hospital, and extracted a mulberry calculus thickly incrusted with the triple phosphate. Everything proceeded favourably until the third week, when the wound was nearly healed. He then all at once lost appetite and strength, became greatly emaciated, and seemed as if about to sink; but after a time gradually recovered, and was dismissed quite well on the 13th of July. This illness also, if it had commenced soon after the operation, would doubtless have been ascribed to the chloroform, instead of being, as I believe, merely an attack of influenza, from which many of the patients were at the same time suffering.

The first adult on whom I operated, with the assistance of chloroform, was a man of twenty-nine, whose case presented some remarkable features. He had been discharged from military service as labouring under an incurable stricture of the urethra, which subsequently resisted the efforts of various surgeons, in civil practice, to afford relief, and ultimately not only rendered him unfit for any employment, but threatened to prove fatal through the incessant and exhausting distress it occasioned. For the last twelve months of this miserable existence, he had, according to his own account, introduced a bougie into the bladder once a-week without affording any improvement. Upon examination, I found that a catheter, though of small size, met with resistance at the neck of the bladder, and that when pushed beyond this, as it appeared, into the cavity, it did not draw off any water. Although there was no discharge of blood, or other sign that the instrument had taken a wrong direction, I concluded, after several trials of this kind, that there was a long-established false passage, and that the symptoms depended upon some cause different from stricture, which never occurs at or beyond the prostatic region of the canal. I therefore searched for the true inlet, and before long found it going off from the side of the other, with the further result of detecting a large stone in the bladder. On the 3d of May last I extracted the concretion, which weighed nearly four ounces, and consisted of three distinct portions—there being a nucleus of mulberry calculus, then an incrustation of lithates, and externally a thick coat of phosphatic salts. It was obviously of very old standing, and unquestionably the source of all his distressing symptoms, except in so far as they proceeded from the injury done to the urethra

in attempting to remedy a stricture which had probably never existed. He recovered completely, and remains well.

Soon after the case just related, I operated in private upon a respectable citizen of Edinburgh, seventy-four years of age, who, although not of tall stature, was so developed in his muscular and adipose textures as to weigh within a trifle of eighteen stones. To give the reader an idea of the process as conducted under the new system, I may describe what took place upon this occasion. On the morning of the day fixed for the operation, I found the patient lying in bed with a smiling countenance, indicative of tranquillity and confidence, without any apprehension of danger or anticipation of suffering. I poured some chloroform upon a handkerchief, and allowed him to inhale the vapour gradually, so as to avoid any unpleasant sensation. In little more than a minute he was sound asleep. My assistants then came into the room and made the necessary preparations. The patient was taken out of bed, placed upon a table, and tied up as usual. I then extracted the stone, which weighed nearly two ounces, without any difficulty or delay, notwithstanding an uncommon thickness of perineum, secured a tube in the wound, and conveyed the old gentleman to his bed, where he, after a while, awoke with the same smiling aspect as before the operation, of which he was, of course, entirely unconscious. He recovered completely, and is now in good health.

On the 21st of August I cut, in the hospital, W. R., aged 76, from Aberdeen; and on the 25th of September, G. L., aged 66, with the same satisfactory result; so that I think chloroform may be used without any apprehension of bad consequences, even at the most advanced period of life. In so far, therefore, as the objection of pain is concerned, lithotomy comes to be on an equality with lithotrity, or rather preferable to it, since the crushing of a stone, however carefully and skilfully conducted, can hardly be accomplished without some uneasy sensations, which could not prudently be prevented through the use of chloroform, as the patient's feelings constitute a most important guide for the surgeon's proceedings. It is true that we read of lithotriptic operations performed under the influence of chloroform, but in such cases we also read of the appearances found on examining the patient's body after death, which tend to confirm, with the authority of experience, the objections to this procedure. But to simplify the question, we may put the pain attending lithotrity out of view, and proceed to consider the relative degrees of danger which attend that mode of treatment and lithotomy.

In estimating the danger of lithotomy, it has been customary to take the average number of deaths compared with that of operations. But this method of inquiry seems little calculated to afford any secure ground of judgment. Some surgeons operate with great success, others with frequent failure; and after a long train of recoveries, it is not unusual to meet with many reverses. Mr Liston cut for

stone with remarkable dexterity and success, but I have seen him operate with a fatal result twice in the same day. This uncertainty of issue cannot be owing to chance, and must admit of reference to circumstances which, if known, might be established as the objects of pursuit or avoidance. When the safety of lithotomy in general is considered, it plainly appears that the fatal issue must be owing to fault in the principle of operating, error in the mode of execution, or an improper state of the patient. I believe that lithotomy, like ligature of the femoral artery, admits of being so perfected and reduced to principles of execution so precise as to render its performance, in proper cases, nearly, if not entirely, free from danger; and if this be the case, it is surely unwise to rest satisfied with attaining average success, or to use the amount of this as an argument for adopting less efficient means of relief.

If the stone is of extreme bulk, the prostate greatly enlarged, or the coats of the bladder seriously diseased, no degree of care or skill on the part of the surgeon may be sufficient for effecting extraction with safety; but in all such cases, lithotrity would be, if possible, still less practicable with advantage, so that they may be excluded from consideration in comparing the expediency of the crushing and cutting processes. If these hopeless cases, in which interference of any kind seems forbidden by prudence, were struck off the lists from which an average rate of mortality from lithotomy has been calculated, the risk of this operation would appear greatly diminished.

The crushing of stones in the bladder has been greatly improved, since its introduction into practice, by the construction of instruments better suited for accomplishing the object of their employment with safety and efficiency, than those originally contrived for the purpose; and also by the knowledge acquired through experience of various circumstances requiring attention in conducting the manipulations. But still the process cannot be regarded as either safe or certain, and its nature precludes the possibility of any approach in this respect to the cutting operation. The various size, number, and consistence of stones, so much modify the operation of instruments and the escape of fragments,—a prolonged treatment necessarily exposes the patient to so many sources of local irritation and constitutional disturbance,—and the surgeon, in frequently employing his apparatus, is so apt to cause abrasion of the mucous surface, productive of the most serious consequences, that, unless where the concretion is very small, there must always be a considerable risk of untoward occurrences. Thus a man, aged 50, on whom I operated in the hospital last spring, after a stone, the size of a pigeon's egg, had been comminuted and brought nearly all away, by repeated use of the crushing apparatus, without any bad effect, went home to the distance of twelve miles from town, and then finding some uneasiness came in again, had a fragment extracted, and returned the following day without the slightest inconvenience; but some time afterwards, having made another journey to Edinburgh with a simi-

lar view, went home the same day, was exposed to cold in crossing the Forth at Queensferry, and then walked two miles in the rain. He had now at length got completely free of the concretion, but in consequence of this imprudence, was confined for several weeks by a smart inflammation of the testicle.

In the course of several years past, I have been occasionally consulted with regard to the case of a gentleman who had a stone crushed in London with great success, as was generally understood and believed,—the fact being, that in consequence of the last introduction of an instrument, the patient suffered an attack of local and general disturbance, so severe, that he received no fewer than a hundred visits from his surgeon, and has ever since endured the most distressing annoyance from urinary irritation. I was not long ago consulted by another gentleman, who had undergone the same process, and ever afterwards been searching in vain for relief. The bladder was found by a surgeon in London—not the one who had operated—to be in the condition of a neglected chamber utensil, being lined with deposits from the urine, so as to require washing out with acid injections. Now, in both of these cases, I know, from the examination which I made previously to any operation being performed, that the patients were in good health, that their prostates were perfectly sound, and that the stones were of a size which would have rendered their extraction equally easy and safe.

On the whole, then, I think it may be fairly concluded, that, unless where the concretion is so small that it may be readily crushed and speedily expelled, the preferable method of affording relief from stone in the bladder will be lithotomy performed under the influence of chloroform. The principles of its safe execution may be shortly stated, as a free external excision, careful division of the muscles, avoidance of the anterior and lateral portions of the membranous part of the urethra, where the artery of the bulb is exposed to danger, cutting through the left lobe of the prostate, and running the knife fairly into the bladder, so as to divide the ring at its neck, which yields little to stretching, and *cannot be torn without certain death.* In order to ensure the completion of this last-mentioned step, which in adults used to be beyond the guidance of sight or touch, I sometime ago recommended that it should be effected in them by means of a straight staff, and knife so fashioned as to attain the object with certainty. But I find that when the patient is rendered unconscious of the operation, the parts concerned being no longer drawn up by spasmodic action of the *levator ani*, the prostate is so much more within reach, that the finger may always serve as a guide for the knife in that most critical of all incisions, which is required at the neck of the bladder.

ARTICLE II.—*Case of Obstruction of the Bowels, arising from a Peculiar Conformation of the Sigmoid Flexure of the Colon, and terminating Fatally; with Remarks.* By J. A. EASTON, M.D., one of the Physicians to the Glasgow Royal Infirmary, Lecturer on Materia Medica in Anderson's Institution, &c.

MARTHA HOWIE, æt. 27, married, admitted into the Glasgow Royal Infirmary 7th January 1843. About a fortnight ago, without any assignable cause, the patient began to experience severe pain in the lower part of the back and whole of abdomen, the pains being continuous in both situations. States that she has been habitually costive, and that she was similarly affected a year ago, the abdomen having become enlarged and very tense, and that, on that occasion, she recovered under the use of injections. Has had no evacuation from her bowels for a fortnight, during which time the abdomen has become greatly distended and extremely painful. Patient is about six months advanced in her first pregnancy. The abdomen is now greatly enlarged, tense, and very tympanitic; it is the seat of intolerable pain, which is not increased upon pressure. Respiration hurried; urine scanty; pulse 90, of fair strength; tongue moist, though slightly coated with a white fur. *To have the domestic enema, with the addition of three ounces of oil of turpentine.* 9th. The injection, though repeated several times since last report, has not brought away any of the contents of the bowels. The abdomen is still more distended, and the pain in it unremitting and almost intolerable. An œsophagus tube was introduced into the rectum this morning to the extent of nearly a foot, but without causing the escape of either air or fæces. 10th. *Twelve drops of croton oil to be made into a mass with crumb of bread, and divided into six pills. Take one of these every two hours, until the bowels be moved. Take of tobacco twenty grains, boiling water ten ounces, infuse during ten minutes, and use the strained infusion as an enema.* 12th. The medicines prescribed above had no effect upon the bowels, and the patient died in great agony at 7 P.M.

Dissection, thirty-six hours after death.—On opening the abdomen, three objects prominently presented themselves, and were indeed almost the only objects visible, the other contents of the abdominal cavity being in a great measure concealed behind them. The first, and most prominent, was a large body, like an inflated ox bladder, which occupied the whole left side, lying in a perpendicular direction. This turned out, on subsequent examination, to be the distal or descending portion of the sigmoid flexure of the colon. The next object was a portion of inflated intestine, very large, but much less than the former, which lay parallel to it, exactly in the middle line of the body, and which proved to be the proximal or ascending portion of the sigmoid flexure. Lastly, there was the gravid uterus, which, rising out of the pelvis, occupied the right iliac region, being pressed out of its natural situation by the viscera just described.

On removing the uterus, and turning down the enlarged sigmoid flexure of the colon, the small intestines came into view, very much distended with flatulence, but quite natural as to position, and free from all appearance of inflammation. The stomach lay concealed behind them, collapsed, and of small size. The colon lay also concealed throughout its whole course, excepting the sigmoid flexure already mentioned. It was everywhere very much distended, but the distension was greatest at its two extremities, and there also it presented certain other anatomical peculiarities. The caput cæcum coli, instead of lying in the right iliac region, lay much higher up, about the middle of the right lumbar region of the abdomen, and, next to the sigmoid flexure, was the largest portion in size. The remaining parts of the ascending colon, as also the transverse and the descending portions of it, were quite normal as to position and connections.

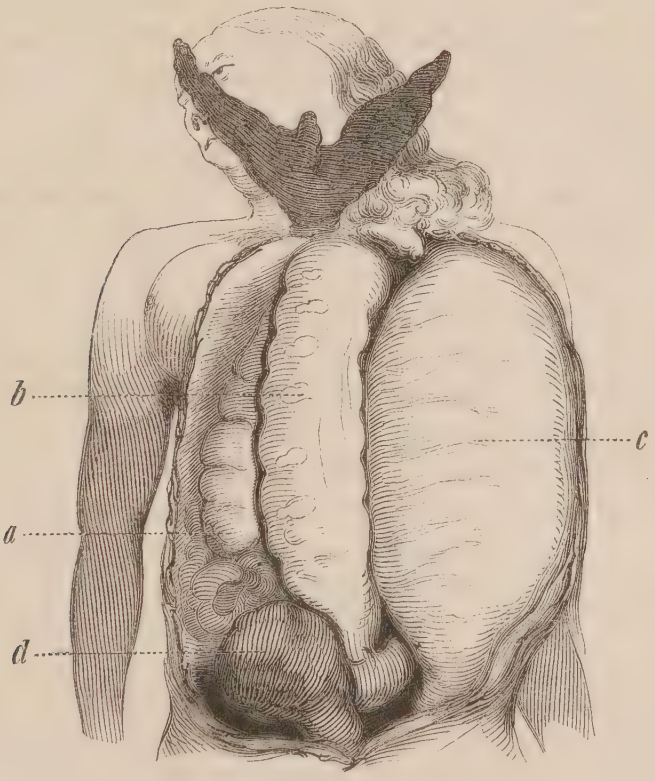
The great peculiarity of the case consisted, therefore, in the enormous size of the sigmoid flexure of the colon. It was nearly as long as all the other parts of the colon taken together. From the left iliac region, where it originated in the usual form, it ascended in the middle line, but at length inclined to the left side, till it reached the region of the spleen, and lay concealed behind the false ribs of the left side; here it turned at a very acute angle, and passed down on the left side of the abdomen, gradually attaining, as it descended, the enormous size already described. The effect of this disposition was, that the beginning of the sigmoid flexure lay in front of the end of it, and thus the latter or lower end of the colon, just where it was about to terminate in the rectum, was compressed between the portion of intestine distended with feculent matter lying before it, and the promontory of the sacrum lying behind it. This was, in all probability, the original cause of the obstruction, which must have been going on for years, gradually increasing, till brought to its height by the occurrence of pregnancy, the pressure of the gravid uterus becoming an additional cause of obstruction.

The circumference of the lower part of the descending sigmoid flexure was 25 inches,—a little higher up, 20 inches; that of the ascending sigmoid flexure was 13 inches, which continued with slight variations to be the size of the intestine as far up as the caput cæcum, which measured 17 inches.

The uterus contained a fœtus of between six and seven months.

The cavity of the chest was very much diminished in size, from the great increase of the convexity of the diaphragm. The heart was of a flattened shape, and the lungs compressed, but otherwise of a healthy appearance.

The accompanying woodcut, from a drawing executed from nature by my clerk, Dr James Bain, conveys an accurate idea of the appearances which were presented on the laying open of the abdominal parietes, only the uterus has been



a, Caput coli.
b, Ascending portion of sigmoid flexure.
c, Descending portion of sigmoid flexure.
d, Gravid uterus, turned a little down.

a little turned down, and the commencement of the colon brought into view; and I may add further, that this monster colon, stuffed with hair, so as very nearly to retain its original dimensions, has been deposited in the Hunterian Museum of the University of Glasgow, for the inspection of the curious.

Remarks.—Before making any remarks on this case, it may be as well to state, in the outset, that the disease under which this patient laboured was supposed to be ileus; and that the existence of the singular malformation of the colon, discovered after death, was not even suspected during life. This statement will explain why clysters, tobacco, croton oil, &c., were resorted to in the treatment,—a procedure proper enough, possibly, under the mistaken notion which was entertained regarding the nature of the disease, but one which certainly would not have been adopted, at least not persisted in exclusively, had the real nature of the case been apprehended. Yet some extenuation of this error will possibly present itself to the ingenuous mind, when it is recollected that malformations of the colon, as the cause of a peculiar and distinctive disease, are so comparatively infrequent as scarcely to have been noticed in that relation in our systematic works on practical medicine. Thus, the reader will look in vain for any account of the disease under consideration in "Craigie's Elements of the Practice of Physic," in the "Cyclopædia of Practical Medicine," or even in that most approved and popular text-book "Watson's Lectures," while, still more conclusive as to the rarity of the disease, it is but barely alluded to in that prodigy of labour, "Copland's Dictionary." Variations in the course of the colon,—unproductive, however, of total obstruction of the bowels,—are no doubt occasionally met with, and several of these have been particularly recorded by Morgagni, Glisson, and others; but the particular kind of variation detailed above, or others closely resembling it, and which alone I believe to be capable of producing the disease of which this woman died, are comparatively rare, and of these variations, as well as of the disease resulting from them, the most accurate—indeed the only—description has been given by my friend Dr Andrew Buchanan, Professor of the Institutes of Medicine in the University of Glasgow, to whose excellent paper on the subject in the twenty-fourth volume of the "London Medical Gazette" I shall frequently refer in the course of these observations. In this paper, it is stated that seven instances of this peculiarity in the conformation of the colon had been met with in Scotland, some of which had been recorded before, and some for the first time in Dr Buchanan's communication. Of these, the first had been observed by Dr Abercrombie in 1815, and published by him in the sixteenth volume of the "Edinburgh Medical and Surgical Journal;" the second occurred in the Edinburgh Royal Infirmary in 1819, under the care of Dr Andrew Duncan, and by his permission an account of the case was published in the same volume of the journal first mentioned; the third and fourth had been met with in the Glasgow Royal Infirmary in the years 1819 and 1821 by Drs

Richard Miller and J. A. Lawrie respectively, but were not made public till detailed by Dr Buchanan in his monograph; the fifth was seen and attended to by this last-named gentleman in Glasgow in 1825; the sixth was accidentally discovered by my colleague, Dr Hunter, in the dissecting-room of Anderson's University, Glasgow, in 1831, and the anatomical appearances were described by Dr Hunter in the fourth volume of the "Glasgow Medical Journal;" and the seventh was observed by Dr Buchanan in 1832, and also in Glasgow. Other cases have very probably been met with; but, so far as I am aware, no account of them has been published.[1]

From this history, then, it appears, that the disease in question is comparatively rare—for the obvious reason, that the cause which produces it is rare; and to this cause, as originating a disease characteristic in its symptoms, I shall now very briefly recall the attention of the reader. The disease is complete obstruction of the bowels, arising from a peculiar, and, in most instances I believe, a congenital, conformation of the colon, whereby inordinate pressure is exercised on a part of the larger bowel by a superposed portion of distended intestine, the result of which is great accumulation of feculent matter and enormous distension of the belly. It has been shown that, in the case of my patient, the sigmoid flexure of the colon, in place of passing downwards into the pelvis from the left iliac region, ascended in the mesial line towards the left side, till it reached the region of the spleen, and that thence, forming a very acute angle, it descended vertically, on the left side, into the pelvis—having its lower portion, where it runs into the rectum, compressed between the proximal or ascending portion of the flexure, distended with gas and fæces, lying before it, and the promontory of the sacrum lying behind it. Now this malformation was very likely congenital, causing the habitual constipation under which the patient laboured; and any person having such a conformation would always be liable to the baneful consequences arising from a mass of impacted feculent matter pressing heavily on a portion of intestine, and thus occasioning complete obstruction of the bowels. But in this case, the disease caused by such pressure was the more readily induced, as unquestionably it became the more appalling and irremediable, by the superadded weight of the gravid uterus; and the reader cannot have failed to notice, that the compression did not begin to be exercised with fatal continuousness till that period of utero-gestation when the impregnated uterus, for months comparatively unaltered in size, rapidly acquires an increase of bulk so great as to act as a disturbing force

[1] In the foregoing chronological history I have confined myself to the cases which have been published in Scotland; but I think it right to add, that the account of a case, similar in many respects to the one which I have detailed, and furnished by Tidicæus, will be found in the "Sepulchretum Anatomicum" of Bonetus. Pregnancy existed in this case also; the bowels had been long constipated, their obstruction at last became complete, the abdominal distension enormous, and the result fatal.

in the relative position of the abdominal contents. I have been thus particular in specifying what appears to me to be the true cause of the obstruction of the bowels in this and similar cases, because it is usual in these circumstances to ascribe such an occurrence to a twist of the colon at the lower end of its sigmoid flexure. Whether such a twist of intestine as will lead to complete obstruction can not only take place, but be maintained for days,—nay, even weeks and months, by any force acting within the body, is a point on which there may be a difference of opinion, though I cannot help thinking that any one who reads the clear and satisfactory statements of Dr Buchanan on this subject, in the paper referred to, will agree with him in believing, "that, waiving the question of the existence of an adequate force, such an effect is physically impossible without rupture of the meso-colon." But leaving this debateable ground, I shall only say, in concluding this part of the subject, that, in the case under consideration, the notion that the obstruction was caused by a twist must be abandoned, simply because no twist existed—the work of impediment having been more effectually performed by a column of nearly solid intestine pressing heavily on another portion, in contact with the promontory of the sacrum, and the gravid uterus forming the copestone of the whole.

The disease thus produced under circumstances so unusual is attended with some symptoms which are peculiar, and with others which are general. In most of the cases recorded, and in the case of my patient also, constipation had long been present, as also occasional dragging pains in the back. These are the symptoms of a common or general character. But when total obstruction supervenes, the rectum becomes a capacious void, the large bowel above the compressed point becomes more and more impacted with daily accumulating gas and fæces, the intestines now stretch to the utmost limit short of bursting of their walls, monstrous distension of the belly is the consequence, and excruciating agony, which

——"Not poppy, nor mandragora,
Nor all the drowsy syrups of the world,"

can alleviate. It is this intolerable and persistent pain which, by gradually exhausting the vital energies, induces, in the generality of instances, the fatal result. In my case it was without doubt the sole cause, for inflammation was not present, and, though the suppression of the alvine discharge for nearly three weeks must have greatly interfered with the whole economy of the system, yet, as has been well remarked by Dr Buchanan, "the mere obstruction of the bowels cannot be considered as an adequate cause of death, since persons have been known to live more than a quarter of a year without any alvine evacuation." Now, these latter symptoms, the intractable and continuous constipation, the enormously distended belly, the empty rectum, and the excruciating pain, unaccompanied by fever or other sign of inflammation, are sufficiently pathognomic, and ought to have enabled me to have distinguished this disease from all other kinds of tympanitic swelling and constipation.

It was formerly stated that this patient was supposed to labour under ileus, and it may not be an unprofitable exercise now to consider, very briefly, the diagnostic characters of that disease as contrasted with those of the one under consideration. In ileus, the abdomen seldom—I believe never—acquires the monstrous volume which it does in the disease arising from compression of the colon; and when the abdomen does get enlarged in ileus, that event takes place towards the termination of the ailment—while, in the innominate disease under review, enlargement of the belly is an early, as it is an unvarying, peculiarity. In the one disease, the pain is of inflammatory, or at least of congestive origin, and is increased by pressure; in the other, it arises from inordinate intestinal distention, is seldom accompanied with fever, and is relieved, at all events is not increased, by pressure. In the one disease, vomiting, at first of the ordinary contents of the stomach, and ultimately of stercoraceous matter, is a common occurrence; in the other, it is an exceptional event,—having only been noticed, of the cases recorded in Scotland, in the one detailed by Dr Abercrombie, first in the sixteenth volume of the "Edinburgh Medical and Surgical Journal," and subsequently in his work on the diseases of the intestinal canal, where it is given —in my humble opinion erroneously—as an example of ileus. Again, in ileus, the invasion of the disease is comparatively sudden, and not preceded by habitual alvine sluggishness, while in the disease now being considered the invasion is gradual, and the bowels have long been habitually costive.

Regarding the *treatment* which ought to be pursued in cases similar to that which forms the subject of this communication, having nothing to offer of my own, I feel that I shall do my readers the most service by transcribing the following remarks from the paper of Dr Buchanan, so often referred to:—

"From the account now given of this disease it must be obvious that, in treating it, all purgative medicines are useless, as well as all clysters administered in the ordinary way, so soon as the rectum has been emptied of fæces. The first and great object of our treatment should be to remove, if possible, the mechanical obstacle to the evacuation of the bowels. To facilitate this object, the patient should be placed upon his hands and knees, or standing, with his body inclined forward, which has the effect of freeing the end of the colon from the weight of the first turn of the sigma lying over it. An instrument should now be introduced into the intestine, for the purpose of dilating the compressed portion. The instrument which appears to me best adapted for this purpose is the œsophagus tube, armed with a large sponge at the end. The sponge is easily fastened by means of a string attached to it, and passing through the tube; while, by means of the other half of the string running outside the tube, the sponge can be withdrawn when we wish to use the tube as an injecting instrument. In the first instance, however, we are to use it as a probe and dilator. After carrying it up as high as we may deem necessary, it is to be withdrawn,—when, by examining whether or not the sponge be smeared with fæces, we can ascertain whether or not it has passed the compressed portion of the intestine. If it have passed, we should reintroduce it; and, by passing and repassing it up and down through the compressed part, we should endeavour, if possible, to form a free passage, by making an indentation in the soft feculent sac, lying, in the position of the body here supposed,

below it. Very probably we may succeed, in this way, in procuring a discharge of fæces, which, without any contraction of the muscular fibres, would be protruded along the passage by the elastic force of the condensed air in the surrounding intestines. If we do not succeed, the tube must be again carried up as far as possible, and the sponge withdrawn, when we must inject tepid water, or air, as may be best found to answer the purpose.

"Suppose that we do not succeed in procuring any evacuation by these means, or that we succeed only partially, so that, the tension remaining undiminished, it is still obvious that death must ensue,—is the patient to be abandoned to his fate, or are there still other means of relief in our power? I answer, without hesitation, that, in the circumstances just supposed, we should puncture the bowels. With what view should this operation be put into practice? Not, certainly, for the evacuation of fæces,—which could not be prevented from escaping in part into the peritoneal cavity, and would so cause death. It should be performed, therefore, solely with the view of discharging air from the bowels, which will have the double effect of relieving the sufferings of the patient, and facilitating subsequent attempts to evacuate the fæces through the natural passages. In what situation should the puncture be made? This question reduces itself to the more simple one,—In what part of the colon should the puncture be made? For there is no other part of the intestine in such cases accessible to the puncture. The whole of the sigmoid flexure, and greater part of the left colon, containing only fæces, are excluded from consideration, and the transverse colon is excluded by the uncertainty of its position. The right colon is the part of the intestine most uniform in its seat, and it is likewise distended almost entirely with air. I would, therefore, recommend puncturing the right colon, about the middle of its course, with a small trocar. On withdrawing the stillette, the confined air should be allowed fully to discharge itself, after which the attempts to evacuate the bowels may be renewed, probably with better success."[1]

Though I doubt not that the plan recommended in this quotation is the only one whereby pressure can be taken off, for the time, from the compressed portion of bowel, and evacuation of the intestines promoted, yet whether, as the malformation still continues, and subsequent impacting may consequently ensue, the ingenious treatment here recommended can be viewed in any other light than as merely palliative and provisional, is a point which I leave to the consideration of the reader. This much, however, will I think be conceded, that in those cases, as in mine, where the pressure from nearly solid superposed intestine is increased by the superadded weight of a gravid uterus, no change of position, no dilatation by mechanical appliances,—no means, in short, whatever, are likely to prove available in averting the fatal result; and, in conclusion, I may be permitted to add, that this consideration of the utter hopelessness of my case, even under the most judicious treatment which could have emanated from a correct apprehension of the nature of the disease, is the principal consolation afforded under the misfortune of having made a wrong diagnosis.

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[1] Since this was written, it has been proposed—and the operation has more than once been performed—in cases of a different kind, such as ileus and intussusceptio, to open the colon in the situation here indicated, by an incision behind the peritoneum, for the purpose of forming an artificial anus. This operation, then, may be added to our remedial resources in cases of this description.

ARTICLE III.—*Cases illustrative of the Power of the Cold Douche in Promoting Absorption, and Arresting Diseased Action.* By C. F. SLOAN, M.D., Ayr.

IN publishing the following cases illustrative of the power of the cold douche in promoting absorption, and arresting diseased action, my wish is to call attention to an agent which latterly has been rather neglected by regular practitioners, and to suggest an extension of its use. The cases,—viz., an encysted tumour, a gôitre and a scirrhous mamma,—are the only morbid growths which I have submitted to its action, and in all there was decided improvement. It is, therefore, reasonable to suppose, that other tumours are equally amenable to its powers. I find this plan of treatment is not novel as applied to the cure of gôitre. In Copland's Dictionary, article Bronchocele, there is a cursory mention of it and cold bathing among other remedies; but the conclusion he comes to is, that if iodine fail recourse must be had to surgical means,—such as seton, tying the thyroid arteries, or extirpation of the tumour. From this we may conclude, that in the course of his extensive reading (the article in question concluding with a list of thirty-four authorities consulted), he had not met with an instance in which the douche had been of much avail. I hope the perusal of the second case may prevent, in many instances, recourse being had even to the mildest of these operative measures. I have no experience of the efficacy of nettle-seed, which has been recommended in bronchocele; it was while waiting for a supply of this medicine, with the intention of giving it a trial, that I thought of the douche.

I shall shortly describe the mode in which I employ the douche, but I by no means advance that method as being the best, my experience having been too limited to admit of comparative trials. Ayr, the town in which I practise, is supplied with water by gravitation, from a distributing basin 100 feet above the level of the lowest street; this affords ample pressure. I generally direct the douche to be applied by means of a gutta percha tube, half an inch in diameter, fitted to the end of one of the pipes supplying the house. The stream is allowed to strike the object at from six inches to two feet from the end of the tube; at all events, before the column of water is broken. The power, of course, varies with the situation of the house, but a pressure of less than forty feet is capable of producing excellent effects. The application is continued till a decided sense of pain is induced, which takes place generally in less than five minutes. It is possible that the astounding douches of the water cure establishments may produce proportionately good effects; but I rather suspect all the benefit to be expected may be obtained from a moderate current, as being less likely to be followed by too strong reaction.

CASE I.—The first case was one of encysted tumour in a male, aged 36, situated on the sacral origin of the left gluteus maximus, of such a size, that a

mould taken by sheet gutta percha was capable of containing seven fluid-drachms. It had existed ever since the patient could remember, was latterly increasing perceptibly, and from its situation gave considerable annoyance. The douching was commenced on the 6th September 1849, and continued daily for a minute or two till the 6th December of same year. At this date the tumour was reduced to about one-fourth of its original size, a mould now taken being capable of containing only two fluid drachms, and the annoyance had almost disappeared. The improvement was most marked in the early part of the treatment, which was prolonged in the hope of a perfect cure. I have examined the tumour lately, and find that it still retains the last-mentioned size, so far as can be determined by manual examination. Of course excision was the preferable plan here, but the trifling pain and confinement were objected to. This case is chiefly valuable as showing the power of the douche over a class of tumours not very amenable to other treatment than the knife.

CASE II.—The next case was a bronchocele of the description sometimes called hydrocele of the neck, occurring in an otherwise healthy girl, aged 20, and which had existed since childhood. This, after a long continued treatment by iodine and seton, still retained a size which caused considerable difficulty of breathing, and of which the mould could contain six fluid drachms. The first effect of the douche in this instance was to cause a rapid and general deposition of fat, the amount of which I was able more accurately to determine from the comparison of two casts of the neck, which were taken on the 25th September and 31st October 1849 respectively. This did not arise from the discontinuance of the iodine, as she had taken none of that medicine for six months previously. The increase of the adipose tissue may tend to confirm one of the opinions entertained with regard to the use of the thyroid gland, and even to indicate an influence that the imperfect discharge of its functions may exercise in inducing cachectic disease. The douching was interrupted during winter, when the tumour remained stationary, and was resumed in spring. On the 29th June 1850 the difficulty of breathing had ceased, and the mould then taken could contain only one fluid drachm. The treatment was continued, though irregularly, till 29th September last, when the tumour was still further reduced, the mould being capable of containing only forty minims. As, in the first case, the treatment was prolonged in the hope of a perfect cure, but in this I was disappointed.

CASE III.—The last case was a scirrhous mamma, with retraction of the nipple and sanious discharge, in a female, aged 45, otherwise healthy, with all the functions regular, and the glands of the axilla unaffected. The tumour had been pronounced cancerous by a high authority in such cases; but excision was not thought advisable. It had been kept in check for three years by local and general means, chiefly leeching and periodide of mercury; but, in July last, she presented herself with the skin over the tumour becoming red and tuberculated. I recommended the douche, but took no cast, as I scarcely expected any improvement. I was agreeably surprised, however, by seeing the patient some weeks later with the tumour decidedly less, and the tuberculated appearance much diminished. The douching at first increased the sanious discharge, which afterwards diminished. The treatment has been continued ever since with occasional intervals of a fortnight. I had an opportunity of examining this case a few days ago, and found the skin, that had formerly been red, of a natural colour, and only one tubercle out of three or four remaining. I have no doubt that the douching, in this instance, has considerably retarded the stage of ulceration. The treatment was conducted by means of a syphon drawing its supply from a tub, elevated only a few feet.

I am aware that it may be objected to the foregoing cases, that they are meagrely reported, and incomplete in their results. My answer to the first of these objections is, that when I commenced the treatment I had little expectation that the results would have been

even so satisfactory; and the casts, with their dates, were all that I thought necessary for private reference. With regard to the second objection, it holds perfectly in the case of encysted tumour, which should, in ordinary circumstances, have been got rid of by a very simple operation. In the second case, however, the alternative was not of so light a character: tying the thyroid arteries is neither an easy nor a certain operation,—and extirpation of the whole gland is a most formidable undertaking; yet, if the difficulty of breathing had much increased, these are the only measures that could have been fallen back upon. In the case of scirrhous mamma, I regard the check given to the progress of the disease as a most welcome respite from a scene of hopeless suffering.

Sandgate Street, Ayr, November 1, 1850.

Article IV.— *Case of Hydrophobia.* By James A. Sidey, M.D., Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 20th Nov.* 1850.)

George Wilson, æt. 15, was bitten on the 5th of July 1850, in the right arm, by a large Newfoundland dog. The arm was bare at the time, and the wounds inflicted were seven or eight in number; three of them were about a quarter of an inch in depth,—the others were mere superficial abrasions. There were also three slight wounds on the fingers, and one on the chin. The wounds were immediately washed, and then cauterised with nitrate of silver. Slight inflammation of the arm followed, which was subdued by poultices, and the wounds healed in about a fortnight.

At the time of the accident the dog was reported to be perfectly sound. However, on particular inquiry after the boy became affected, it was found, that on the previous night the dog had torn a great part of the door of the warehouse where he was locked up, and had also destroyed several packages of paper. It also appeared that several of the men were afraid to go near the dog that morning. The dog was unfortunately destroyed shortly after the accident.

On Friday, August 16th, the boy complained of a tingling or sleeping sensation in the cicatrices. This symptom continued on the 17th, and became accompanied with a feeling of weakness in the arm, so that he was obliged to leave work. On the 16th he had witnessed the public execution of a murderer, which had frightened him, and greatly affected his spirits.

On Sunday, 18th, he seemed very dull and listless,—complaining still as before of the arm. He went to church in the forenoon; and, while crossing the street in going home, he gave an involuntary scream,—and on being asked what was the matter with him, he said, "he could not help it—it was the wind." In the evening, he said he did not feel well, and went to bed, asking for a draught of

water, but before it could be brought to his bedside he ordered it to be taken away, as he said "it took away his breath." He passed a very restless night.

On Monday, 19th, was seen about 10 A.M. On examination, the patient had a frightened suspicious appearance. The eye very bright and clear; pupils natural, and intolerant of light; skin warm, with considerable moisture; appetite lost; great thirst; tongue white and moist; bowels and urine natural; pulse 120, soft. The cicatrices were natural, and without any redness or swelling; the rest of the limb in every respect natural. He complained of a tightness over or across his chest, of a tingling or sleeping sensation, and weakness in the right arm, and in the back of the neck and tongue. He also complained of a feeling of general weakness.

On offering him a little milk and water, a violent spasm and difficulty of breathing came on, manifested by several quick inspirations, with difficulty of expiration. He attempted most willingly to take the fluid, but could not prevent the spasm from coming on; he then suddenly, and with a deep inspiration, seized the cup and took a little, but was immediately seized with the same train of symptoms.

The wounds were then laid freely open, and bled well to the extent of several ounces. To have five grains of calomel and a turpentine enema. Head to be shaved, and a blister to the nape of the neck. He was also ordered ice to suck.

2 P.M.—Seems more calm; is rather easier and inclined for sleep. He has no headach, but is intolerant of noises. Says that the arm is better and not so weak. Has taken the ice freely, but with slight difficulty, and complains that his tongue sleeps when he takes anything. A gust of wind blowing on him when the door is opened causes him to sigh frequently. He feels also chilliness, although the skin is warm, and he is well covered with bed-clothes. There is a quantity of thick saliva in the mouth; bowels have been acted on; skin still moist; pulse 84. To have two grains of calomel every hour, and mustard poultices along the spine every four hours.

8 P.M.—Has been very restless and loquacious, "complains still of the wind," or any motion of the air in the room caused by a person moving. He complains also, that when a person goes near him "it takes away his breath." Pulse 120, very small.

The wounds were again opened and bled freely. The application of the first fomentation caused a deep inspiration, but this did not continue. The wounds were then rubbed with potassa fusa. The blister to be dressed with mercurial ointment, and the axilla and groin to be rubbed with the same every four hours. To have ʒj. of Battley's solution, and twenty drops of tincture of Indian hemp.

20th, 9 A.M.—Has passed a very restless night, and only slept for an hour, between four and five. Has been talking incessantly, and tossing his arms and legs about. Pulse 84, soft, and very irregular. Saliva copious and frothy, but not expectorated. Tongue still white and moist; skin moist; bowels have not been moved. He com-

plains still of the cold air. He has taken a little porridge without much effort. To have a turpentine enema. Four issues to be made in the scalp with caustic potass.

12 A.M.—So very restless that he makes no complaint. Pulse 120, very irregular and soft; heart's action tumultuous; bowels have been moved; urine natural. Mustard poultices to the chest and feet.

5 P.M.—Continues much the same; but pulse 84, still very soft and irregular; violent action of the heart continues. By Dr Alison's advice was bled from the arm to six ounces, and had forty drops of Battley's solution and twenty of tincture of Indian hemp.

10 P.M.—Was quieter, and did not complain so much of the cold air after being bled, but has again become restless. The other symptoms the same as before. Mustard poultices to be continued, and to have ʒij. of Battley's solution as an injection.

12 P.M.—Is very restless, and complains of his breathing. Other symptoms much the same as before. ʒj. of chloroform was poured on a handkerchief, and on his inhaling it, the anæsthetic effect was produced with great rapidity; but from the accumulation of mucus in the throat and mouth, the difficulty of breathing became so urgent, with lividity of the face, that it could not be persevered in.

21st. 6 A.M.—Has passed a very restless night, with much talking and tossing about. Is sensible, and knows his friends well, but says that one man is going to hurt him, and will not allow him to remain in the room. The features are somewhat collapsed. Pulse 120, very small; saliva copious. Has vomited about a quart of bloody frothy sputa, and seems relieved in his breathing. Has taken several pieces of sugar with brandy dropt on them, as he is still intolerant of fluids.

9 A.M.—In much the same state, but weaker; has attempted ineffectually to eat a little porridge; but has taken the sugar with the brandy.

12 A.M.—Has continued very restless, pulse weaker. Large quantities of bloody saliva are still evacuated without expectoration. He has had a very slight convulsion. About a quarter to one, he suddenly turned on his side, and became completely insensible; and quietly breathed his last about one. He had no spasms of any kind; and the mucus continued to flow from his mouth and nose.

It may be mentioned, that the patient seemed fully aware of the nature of his malady, which was inferred, not so much from his direct statement, as from several very significant and affecting allusions.

Sectio Cadaveris, 24 hours after Death.

Present—Drs Bennett, A. Struthers, and Mr Drummond.

A considerable quantity of sanguinolent frothy mucus had, since he expired, exuded from the mouth, some of which still adhered to the lips and face.

Head and Spinal Column.—The membranes and substance of the brain appeared to be perfectly healthy. The pons varolii and

the origin of the eighth pair of nerves were examined with great care, but nothing abnormal was discernible. The three branches of the eighth pair, as well as the gray and white substance of the pons varolii and pyramidal bodies, were also examined microscopically; but all these portions of the nervous structure were healthy. The spinal cord was examined with the naked eye, as well as structurally under the microscope, without the discovery of any morbid change whatever.

Neck and Chest.—The circumvallate and fungiform papillæ at the base of the tongue were greatly elongated and enlarged. The salivary glands were also enlarged. The mucous membrane of the fauces and upper part of the pharynx was of a purplish red colour. The membrane lining the trachea and bronchi to their ultimate ramifications was of a deep-red mahogany colour, and covered with sanguinolent frothy mucus. This mucus completely filled up the smaller bronchial ramifications. The texture of the lungs was healthy. Heart also healthy.

Abdomen.—All the abdominal organs were healthy.

Note by Professor Bennett.

The above case is valuable, from the very clear manner in which Dr Sidey has detailed the facts connected with it, as well as from the very unequivocal nature of the case itself. The cause, progress, phenomena, and termination, can leave little doubt that this was a decided case of hydrophobia. I, therefore, took especial pains with the post-mortem examination, and, in addition to the careful inspection of every organ in the body, dissected out the branches of the eighth pair of nerves on both sides for some distance down the neck, which, with their origins, were removed for further observation. Subsequently I examined different portions of their structure, and of the restiform ganglion, with a microscope, under a power of 300 diameters linear, but was unable to distinguish the slightest change from their healthy character. Several portions of the white and gray substance of the cerebrum, cerebellum, and spinal cord, were also examined with a like negative result.

I have elsewhere stated (Library of Medicine: article, "Hydrophobia,") that the experiments of physiologists on the branches of the eighth pair of nerves fully warrant the idea, that irritation at their roots would cause most of the symptoms of hydrophobia; for instance, the spasms of the pharynx and larynx, dyspnœa, secretion of frothy mucus, &c. &c. I therefore suggested that, in future, post-mortem examinations should be particularly directed towards this view of the subject. The case of Dr Sidey presents us with an important fact; for, whilst all the leading symptoms of hydrophobia were present, the most minute and careful examination of the origins, roots, and trunks of those nerves, failed to discover any organic change in them.

In the "Monthly Journal" for August 1849 (p. 1021) there are

some notices of cases in which chloroform seemed to be beneficial. Theoretically, there is every reason to suppose that a diminution of the extreme sensibility which is manifested in this disease might retard its progress, and diminish the sufferings produced. In the present instance it was tried, but at the time there was such an accumulation of mucus in the air-passages, and such diminished strength, that its use could not be persevered in from fear of suffocation.

ARTICLE V.—*On the Nature and Primary Changes of Cancerous Exudations, and on the Development of Fibrous Structure in Cancerous Formations; with Remarks on the Diagnosis of Tumours of the Neck.* By P. REDFERN, M.D., London, Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

NOTWITHSTANDING the great advances which have been made in the study of the nature and development of cancerous growths by the labours of Bennett, Walshe, Müller, Lebert, Sédillot, Bruch, and others, there yet remain several considerations of great importance in pathology, for the complete elucidation of which other observations are necessary. Certain parts of the processes alluded to admit of a rational exposition, though we may not yet have been able to confirm our conclusions from analogy by actual demonstration. Amongst these may be ranked the formation of cancerous structure from a fluid blastema exuded from the blood-vessels, but not hitherto examined until after coagulation had reduced it to a finely molecular and granular mass, as figured and described by Professor Bennett.[1] Such exudations appear to occur in the same general manner as inflammatory ones; and yet, whilst every stage of effusion and growth of inflammatory products may be almost daily examined on the free surface of serous membranes, a cancerous exudation certainly occurs with extreme rarity in such a position, if, indeed, it is ever noticed as a primary affection. Further, inflammatory exudations possessing the characters of those effused under ordinary circumstances in healthy individuals, are frequently seen passing through their customary stages of growth in persons who are the subjects of the most extensive cancerous infiltrations, deposited from the same blood—an argument possessing great weight when urged against the doctrine of the occurrence of such morbid depositions, as the simple consequence of the circulation of diseased blood. In ordinary nutrition, development, and secretion, particular tissues exercise an undoubted power of election or appropriation of the various materials circulating through them, in some cases changing the arrangement of the elementary particles of these, if they do not actually form certain substances by their own immediate action; and so may an organ which has either become the seat of a cancerous deposition, or predisposed

[1] On Cancerous and Cancroid Growths. Ed. 1849, p. 61.

to it in some way unknown to us, exert its peculiar elective force upon the same blood as supplies other parts with materials for the formation of healthy tissue.

The following appear among the desiderata for the perfection of our knowledge of the anatomy and pathology of cancerous growths, viz., to ascertain whether fibres exist in all cancers, and are really essential for the completion of a structure, the attendant phenomena of which shall be indistinguishable from those of cancers in general. And, whether the fibrous tissue of cancerous formations bears any constant proportion in its amount to the fibrous character of the organs in which these are developed, and is regulated by the law of analogous formations. These inquiries cannot be satisfactorily answered, otherwise than by numerous and accurate microscopical examinations of the textures of organs so affected, and especially of cancers which occur in parts that contain no fibrous tissue whatever in their normal state, and in such as contain so small a proportion of it that an increase in its quantity may readily be detected. The particulars of a case in which secondary cancer of the brain occurred will be here given; and as cases of this kind are excessively rare, and no other microscopical examination of the structure of such cancers is on record, as far as I am aware, it is to be hoped that those into whose hands other cases may fall will take especial care that their characters be accurately recorded, bearing in mind that a particular density of structure is by no means a necessary attendant on a cancer possessed of a certain amount of fibrous tissue,—and that this is, consequently, a character of little service in the solution of the question proposed. In another case, it will be shown that several ounces of a fluid blastema, of the characters of liquor sanguinis, were effused into the centre of a large cancerous tumour,—that the fluid was speedily reproduced after evacuation, and that, in a few weeks, the whole was converted into a creamy cancerous liquid—having thus become completely changed in its chemical and structural characters. This case is also of importance in connection with the diagnosis of tumours of the neck and diseases of the thyroid body, which will be further illustrated by a case of cystic tumours in the position of the thyroid.

OBSERVATION I.—*Cancer of the Liver, Brain, Kidney, and of every other organ except the Heart; Cancerous deposits in the Right Psoas Muscle, on the transverse processes of the Lumbar Vertebræ, and on the Right Ilium and Femur; Tubercle and Cicatrices at the Apex of both Lungs; Granular Degeneration of both Kidneys; Anormal Nutritive Changes in the Cartilages of the Articulating Processes of the Lumbar and Dorsal Vertebræ, and in many of those of the larger Joints; Epileptic Convulsions; Partial Paralysis; Death.*

CASE.—The patient was a widow, æt. 48, who had a son when very young, and before her marriage, but no children afterwards. She gained her livelihood by carrying large baskets of fish to the country, and by selling dulse (Rhodomenia Palmata), which she herself pulled, standing in a considerable depth of water.

The following account of her history and symptoms was obtained from a very

intelligent neighbour, who lived on the same floor with the patient, and waited upon her when she required it. It is corroborated by several medical men, who saw her at different times during her illness.

Up to January 1849 she was stout and healthy, and regularly carried large baskets of fish into the country, remaining from home two or three days on each occasion. She lived very regularly, and was never known to have taken spirits of any kind. In January she began to complain of pains in the right hypochondriac, epigastric, and lumbar regions, and of swelling of the lower extremities; her appetite was variable; bowels constipated; the complexion became sallow, and emaciation came on. After this time she could not carry such heavy baskets as before. She applied for relief at the Dispensary, but it was occasionally thought that she complained unnecessarily, and no particular notice was taken of her for some time. She applied mustard poultices to her side on several occasions without relief, took castor oil regularly, and at last was in the habit of taking laudanum for the pains. Her sufferings were of variable intensity at different times, and she continued to go out into the country with fish until little more than a month previous to her death. Before the last occasion on which she went out, she was confined to the house for a week; she then went from home for several days, and after her return was very ill until the time of her death. She never complained of pain or difficulty in micturition. The œdema of the lower extremities increased, and she went to the hospital on April 5th, where a blister was applied to her side, and powders of chalk and opium were given for diarrhœa. She was dismissed on April 10th at her own request, and returned home. Between 8 and 9 o'clock on the evening of the same day, the neighbours heard her fall in her own house, and struggle. When they went in she was in a state of perfect insensibility on the floor, tearing her hair, struggling, and frothing at the mouth; she had a wild expression of countenance, and the corneæ were rolled under cover of the upper eyelids. The struggles continued for more than ten minutes. She was lifted into bed, and remained in a state of partial insensibility until the day following; indeed she never became as sensible as before. Her speech was indistinct; she had no recollection of having had a fit,—and though she fancied that she could hold things in her right hand, whatever food she attempted to take with it was spilled on the bed. She never walked afterwards, but stood up on the 21st, when she was removed to the Poor's-house. At this time her legs were much more swollen; she was in a state of partial insensibility; spoke incoherently, and not more than two or three words at a time. She appeared to be sinking, and wine was ordered and continued up to the time of her death. She could lift nothing, and required to be fed the whole time. On the 26th she was believed to be in a fit; froth appeared at the mouth, and there were slight movements of the limbs. This state continued an hour and a-half, and terminated in death. The body was removed to the anatomical rooms, when the following appearances were observed:—

Post-mortem Appearances.—The body is well developed; the mammæ and abdomen present numerous lines in the substance of the skin, indicative of former distension; the abdomen and lower extremities are œdematous, and fluctuation is perceived in the former. The sharp margin of the liver can be distinctly felt, an inch and a-half below the costal cartilages, in the right hypochondrium, and the epigastric and a considerable part of the left hypochondriac regions are occupied by a large, dense, nodulated tumour, having a sharp edge. A distinct, rounded, and moveable tumour, unconnected with the other, exists in the epigastric region. The os uteri admits the point of the finger, and has distinctly thickened lips; by its side, a nodulated tumour, attached to the posterior part of the cervix, can be felt. The body was injected from the femoral artery, which readily admitted the largest injecting pipe that can be used for an adult male. Twice the usual quantity of injection was thrown in without any extravasation, or escape of the injection into the veins or right side of the heart.

Head.—The brain and its membranes are perfectly healthy in every part, except on the upper and lateral region of the posterior lobe of the left hemisphere, where there is a rounded tumour two-thirds of an inch in diameter, projecting one-eighth of an inch beyond the surface, covered by arachnoid and pia mater, which are very vascular at one spot, but appear otherwise healthy. The tumour appears to lie in a sort of cyst, owing to the cerebral matter surrounding it being very soft and friable, though of the usual colour. The white substance is diffluent as far as to the middle of the lobe.

Microscopical Examination.—The softened white substance of the brain contains its varicose tubes as perfect as in healthy brain, and, except that it is very much softened, no structural change would be observed. The gray matter around the tumour is very soft, reddish, and vascular. Its ganglion corpuscles are perfect, but it presents an occasional compound granular cell, half filled with granules; also, numerous shreds of cell-membranes, like very fine epithelial scales broken up.

On making a thin section of the tumour with Valentin's knife, a copious milky juice exudes from the cut surfaces, and shows all the structures which are found in the mass. The external parts of the tumour are resistant, and cut somewhat like cheese; the central part is granular, soft, and yellowish. The section is composed of the following structures:—Numerous mother cells, reaching $\frac{1}{340}$th by $\frac{1}{700}$th of an inch in size, containing one or two other cells with large nuclei, or a number of smaller cells with two or more nuclei in each; large, round, or oval cells with single large nuclei, both the cells and nuclei being finely granular; similar large cells with small and highly refractive nuclei; large cells containing two or three fat globules of large size; large flat cells, irregularly rounded, stellate or bifurcated, containing one or more large nuclei, with one or two bright nucleoli or oil globules; compound granular cells, usually $\frac{1}{1400}$th of an inch in diameter; small, irregular, and finely granular cells; free nuclei, molecules, and granules. There are also a few fusiform cells, with elongated granular nuclei, in some places; and in others, patches of granules, which appear to have been set free from cells.—(Fig. I.)

Fig. I.

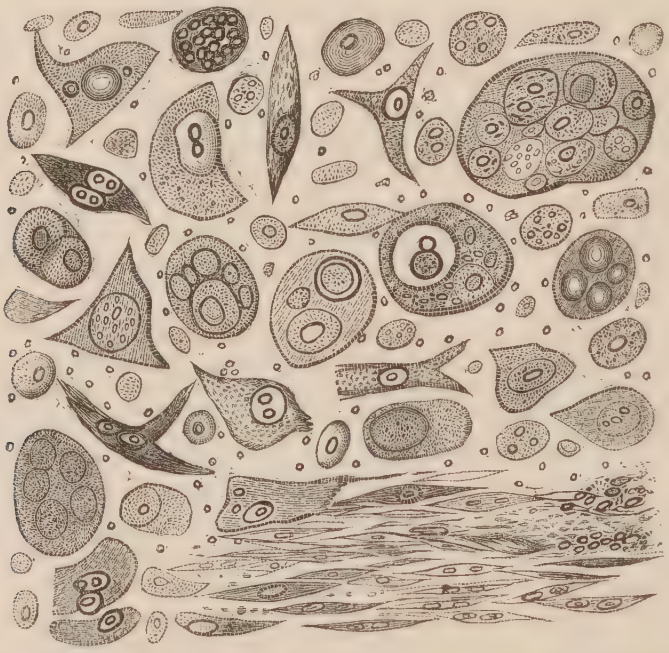

Fig. I.—Substance of cancerous tumour of the brain, showing its variously formed cells, free nuclei, and granules.

On acting upon these structures by acetic acid, the walls of the cells become very transparent; but they still remain finely granular, and the nuclei are

better seen than before; the compound granular cells are slightly more transparent, but their granules are unchanged. Bright and loose granules still remain in the field.

After washing away the cells from a portion of the mass, and dissecting it, nothing is seen but shreds of membrane, and an indistinct granular mass.—(Fig. II.) When acetic acid is added, the mass becomes very transparent, and traces of the walls of cells become visible, with a few nuclei, and a number of granules in different parts.—(Fig. III.)

Fig. II. Fig. III.

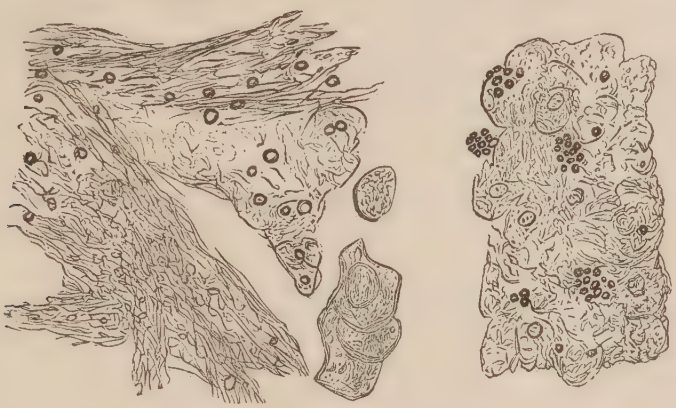

Fig. II.—Remains of the substance of the tumour after dissection and washing; it consists almost wholly of shreds of cell-membranes and of granules.
Fig. III.—A patch similar to the last after the action of acetic acid.

Chest.—The lungs have not collapsed. The right pleura, covering the lower lobe of the lung, is adherent to the diaphragm, and to the 5th, 6th, and 7th ribs, by a dense fibrous mass, one-half to three-fourths of an inch thick. At the apex of the right lung there is an extensive puckering on the surface. The pleura is thickened, but there is neither tubercle nor calcareous matter under it. In the substance of the lung, at different parts, there are yellowish-white friable deposits, which are about a line in diameter, and possess a structure precisely similar to the cancerous masses in other parts. The left lung is slightly adherent in several places for a limited extent, by thin and long bands, which break up easily. Under the pleura there are three or four small cancerous nodules, precisely like those in the opposite lung. At the apex there is a distinct puckering on the surface, under which is a yellowish-white friable mass, of the size of a pea. On dissecting a part of this in water on a slip of glass, it is at once seen to present an appearance very different from the cancerous masses; and the microscope shows it to consist of a multitude of extremely irregular, rounded, oblong, or triangular corpuscles, varying in size from $\frac{1}{7000}$th to $\frac{1}{2500}$th of an inch, the greater number being $\frac{1}{3500}$th of an inch in diameter. They contain granules, but have no nuclei in their interior. Amongst them are a number of small granules floating in a sort of blastema.—(Fig. IV.) Acetic acid renders the corpuscles transparent, but it neither changes their shape nor affects the granules.—(Fig. V.)

Fig. IV. Fig. V.

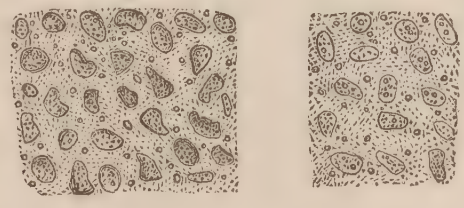

Fig. IV.—Tubercle corpuscles and granules from the small mass under the cicatrix at the apex of the lung.
Fig. V—The same after the action of acetic acid.

The heart and pericardium are healthy, except that the parietal pericardium has four or five small, elongated, and projecting white spots upon it at the upper part. These consist of fibrous tissue.

Abdomen.—Two pints of reddish serous fluid are found in the peritoneal sac, without flakes of lymph. The peritoneum is studded, especially in the right iliac region, in the pelvis and left hypochondrium, with dense yellowish-white tumours, many of which hang by a small pedicle. The tumours vary in size, from that of a millet-seed to that of a filbert, the majority being of the former size. Some are of a crimson colour, owing to their great vascularity. The peritoneum of the liver is extensively adherent to the parietes and contiguous organs ; that covering the spleen has two or three flattened masses in its substance ; the peritoneal covering of the left extremity of the pancreas has some nodules in it, and that of the rectum is connected by a broad and firm band to the posterior part of the uterus. The whole omentum is contracted, and cannot be made to reach the pelvis ; it is studded in every part with yellowish-white tubercles, from the size of a millet-seed to that of a large pigeon's egg. They are all of considerable consistence, and those above the size of a large pea are nodulated on the surface.

The stomach is healthy, except its peritoneal coat, which has about a dozen of the smallest sized tubercles on different parts. The small intestine is healthy, except in having a few of the small tubercles on its peritoneal coat. The appendix vermiformis has an irregular tumour, of the size of a walnut, at its extremity. On the colon, in different parts, there are many small nodules, and one or two of these are precisely of the colour and appearance of a small and ripe cherry. The colon is small throughout. The mesenteric glands appear healthy in texture, and none of them is larger than a small hazel-nut.

Liver.—The tumours felt before opening the abdomen were produced by the liver, both lobes of which are of a reddish-mottled appearance, and have large, rounded, yellowish-white masses within them and appearing on the surface, forming nodules. The right lobe measures eight inches and three-quarters from before backwards, five inches from side to side, and three inches in thickness. It presents no trace of other fissures or depressions than the transverse fissure, very ill marked; and its surface presents slightly projecting nodules, of an inch in diameter on an average. The left lobe measures eight inches from side to side, and from before backwards ; and it is three inches thick. It is filled with rounded and yellowish-white cancerous masses, the largest of which is of the size of a turkey's egg. The gall-bladder lies between the right and left lobes of the liver. It is twice its usual size, and contains an irregular dark-brown calculus, of the size of a pigeon's egg ; fifty-four yellow and prismatic calculi, each of the size of a large pea ; and four similar ones, much smaller, and also uniform in their size.

The spleen weighs about eight ounces, but appears healthy in its interior. Substance of the pancreas healthy.

The left kidney is of the usual size and shape ; the right one measures six inches long by two and a-half broad. The capsule is firmly attached to both, and their surfaces are mottled, uneven, and tuberculated, presenting almost precisely the appearance of a liver in which the tubercles of cirrosis are beginning to appear. After stripping off the capsules, small yellowish-white masses, of one-twelfth to one-eighth of an inch in diameter, are seen on the surface of both kidneys. A section shows their cortical substance to be very distinctly granular, and the yellowish-white nodules to be interspersed through their substance. The right kidney is much more tuberculated on the surface than the left ; it is also more granular in its interior, and presents many more of the yellowish-white nodules. These masses are loose in texture, and are readily picked out from the surrounding parts.

Generative Organs.—At the right and posterior part of the cervix uteri there is a mass of nodules, of different sizes, joining together ; and on the left side there are many similar ones, of the size of large peas. The body of the

organ is healthy. The left ovary is small, yellowish-white, hard, and granular; the Fallopian tube of this side is thickened and dark in colour, and its peritoneum is studded with nodules. The right ovary and Fallopian tube adhere together. On the former there are two small cysts, filled with fluid, and two or three nodules of the same size.

The peritoneum of the posterior part of the bladder presents several patches of beautiful, crimson nodules, of all sizes up to that of a large pea.

Cancerous deposits are also found on the ventral and dorsal surfaces of the right ilium, over a space of the size of a half-crown piece,—behind the trochanter major of the right femur,—and in the substance of the right psoas muscle, resting on the transverse processes and sides of the bodies of the second and third lumbar vertebræ. All these deposits appear to have taken place in the substance of the periosteum, or under it, so as to destroy its texture. Small ossific nodules exist on the ilium and femur at the edges of the masses of cancer, but no evident infiltration of the osseous structure has taken place, except in the case of the upper articulating and the transverse process on the right side of the second lumbar vertebra. The right articulating processes of the third and fourth lumbar vertebræ are enlarged, but dense in structure.

On examining the joints, considerable changes were found to have taken place in the cartilages of the articulating processes of the lumbar and several of the dorsal vertebræ,—in the fibro-cartilage of the right sacro-iliac synchondrosis,—in the cartilages of the right hip and knee-joints,—and in both elbow and carpal articulations. This affection of the cartilages will be found described and figured in a former paper ("Monthly Journal," vol. ix. p. 1071), showing that it is the result of anormal nutrition. It must be noted here that no trace of infiltration of the texture of the cartilage with cancerous matter existed in any part.

Microscopical Examination.—A section of a tumour, of the size of a pigeon's egg, removed from the omentum, has cheesy matter outside, and a yellow, more friable substance in its centre. A copious milky juice exudes from the cut surface, and contains rounded or irregular cells, with nuclei and nucleoli, some being so densely crowded with dark granules that their nuclei cannot be seen, and varying in diameter from $\frac{1}{850}$th to $\frac{1}{590}$th of an inch;—rounded or irregular cells, of various sizes down to $\frac{1}{2000}$th or $\frac{1}{3000}$th of an inch in diameter, containing a nucleus or a number of granules; a few compound granular cells, with large and bright granules; and also free granules and shreds of cell-membranes. (Fig. VI.) After dissection and washing, a fibrous stroma

Fig. VI.

Fig. VII.

Fig. VI.—Structure of large cancerous tumour of the omentum.
Fig. VII.—Fibrous stroma and patches of large granules, left after dissecting and washing the same structure.

is brought into view, with rows and masses of dark granules amongst its fibres. (Fig. VII.) Acetic acid has no action on the granules; but it renders the fibrous tissue transparent, and brings into view a number of cells.

The yellowish-white deposits in the kidneys contain irregular and flattened cells, which vary in size from $\frac{1}{1400}$th to $\frac{1}{700}$th of an inch, and contain a nucleus with one, two, or three bright nucleoli,—also numerous caudate and fusiform cells, with large granular nuclei. In many of the spots no fibrous tissue can be found, though a mass, composed of shreds of cell-membranes, is left on dissection; in others, a few fibres are observed, without any regular arrangement. The tubes of the kidney are perfectly healthy in some places, quite close upon the cancerous deposits; in others, they are filled with epithelial cells, which are compressed into various shapes, and very decidedly granular. Free nuclei are also found in the tubes. The average diameter of the tubes is $\frac{1}{470}$th of an inch, and that of the Malpighian bodies $\frac{1}{150}$th to $\frac{1}{128}$th of an inch.

The cancerous deposits in the other glands, in the liver, lungs, and peritoneum, present a similar structure to the tumour in the omentum, before described.

Remarks.—The most important feature in this case was the deposit, in the substance of the brain, ascertained to be cancerous by a microscopical examination. That this is *the only test* of the real nature of any tumour of the brain, every one who has endeavoured to discriminate between them, by the appearances they present to the naked eye, will be ready to admit, and consequently all the cases which have been recorded without such examination are to a great extent valueless. In this instance, the tumour was sufficiently friable to present characters rendering it impossible to say whether it was cancer or tubercle, when examined by the naked eye; but a single glance at the cells and other structures shown in Fig. I. refers the growth to cancer, as in no other known structure whatever have similar elements been found associated in such a manner. Indeed, the structure of many of the individual cells renders them absolutely characteristic; and yet it must not be forgotten that cancerous growths by no means constantly contain such cells. The absence of fibrous tissue next calls for notice; and here it will not do to account for the absence of this element by supposing that the fusiform cells required more time to enable them to form fibres; for, in many instances, tumours, which have existed for years, and have acquired a weight of several pounds, contain no fibres, though made up of fusiform (fibro-plastic) cells. This tumour did not show a single fibre, though several slices were washed and dissected with the greatest care, with a view to ascertain their existence or absence; nothing but the mass shown in Figs. II. and III. could be seen, and this evidently consisted of shreds of cell-membranes, and portions of their contents. Yet such an isolated negative observation must not have too much importance attached to it, and we must anxiously await the occurrence of other instances. The cancerous deposits in the lungs, liver, and kidneys, presented nothing unusual; but I failed, as in several former instances, in endeavouring to ascertain what was the precise relation of the cancerous elements to the

normal structure of the organs,—an inquiry which I believe to be of the greatest importance, and one of considerable difficulty, especially as regards the liver. In a kidney otherwise healthy, there would be less difficulty, but the secreting structure of both kidneys was extensively diseased in this case, and presented the most formidable obstacle in the examination of the relations of the cancerous deposits. Notwithstanding the existence of such extensive cancerous disease, and the granular degeneration of the kidneys, the patient was able to follow her usual occupation until little more than a month previous to her death.[1] Though she was evidently sinking under the general cancerous affection, there seems good reason to believe that the epileptic paroxysms were the result of the development of the cancerous tumour of the brain, and that the death was directly produced by it.

The tubercular nature of the deposit in the apex of the left lung admits of no doubt, but it is impossible to say at what period this exudation occurred, though it is obvious that the cicatrices had existed for years.

As the changes in the structure of the articular cartilages were precisely such as are frequently found in the bodies of persons beyond adult age, there is no good reason for ascribing them to the development of the cancerous formations as to a direct cause.

Observation II.—*Large and rapidly-developed Encephaloid Tumour of the Neck; Cancer of Bronchial and Lumbo-Aortic Glands; Anasarca and Hydrothorax; Cirrosis of Liver; Death.*

Case.—Martin Cuthbert, æt. 70, pensioner,—general appearance that of a strong and healthy man; complexion ruddy until lately, but now somewhat sallow,—applied to me for advice regarding a tumour of the neck on April 11th, 1850.

States that he always enjoyed good health until December last. He entered the army in 1798; was at the Cape from 1805 to 1814, when he went to New Orleans, and lost his arm on January 8th, 1815. Of late has travelled about the country as a hawker, carrying a very heavy box, suspended on his left side by a strap passed over the shoulder of the same side. He continued perfectly well until Decemher 1849, when he noticed his general health not so good as usual. He takes spirits at times, but has not been habitually intemperate. He first noticed the tumour on the left side of the neck, at a little distance from the clavicle, in January 1849, since which time it has enlarged regularly and become painful.

April 11th, 1850.—A large rounded tumour, elongated from above downwards, extends from the clavicle up to the os hyoides on the left side of the neck. The skin over the tumour is very thin, and covers numerous veins, which are seen through it everywhere. Under the skin of the upper part of the same side of the chest, there are also large and tortuous veins. The tumour is perfectly even on the surface, very tense, painful on pressure, and presents very distinct fluctuation over a large portion of the surface. The sterno-mastoid

[1] In another instance, which occurred in the practice of my former pupil, Dr Polson of Old Aberdeen, the liver attained a weight of above twenty lbs., owing to the formation of melanotic cancer; and the patient, a female, æt. 48, mistook the disease for pregnancy, and went about until a fortnight before her death.

muscle is tightly stretched over the tumour; the trachea and larynx are displaced to the right side of the neck, and the left carotid artery cannot be felt. There is no difficulty of deglutition or respiration; no cough; no anormal dulness on percussion of thorax, or unusual respiratory sounds or cardiac murmurs; no tumours are felt in the abdomen, and percussion elicits no increased extent of dull sound over it; appetite indifferent; pulse frequent.

April 17th.—The tumour is still excessively tense, and is now painful on pressure over a large portion of the surface. On entering a grooved exploratory needle into the anterior and lateral part of the tumour, it at first met with great resistance, as if from the wall of a dense cyst, and then passed with the greatest readiness. A glairy fluid escaped along the groove of the needle; and on slightly enlarging the opening with a narrow bistoury, three and a-half or four ounces were obtained. At last, the fluid passed with some admixture of blood, but that first obtained was of a slightly yellowish colour, and nearly transparent, containing small curdy masses here and there. After about ten minutes, the whole fluid withdrawn had coagulated into a firm fibrinous clot, which adhered to the vessel so firmly as not to allow of being poured out.[1] The clot, on microscopic examination, presents a finely molecular mass, full of primitive filaments, and it certainly could not be distinguished from a clot formed in liquor sanguinis. The coagulating fluid arrested my attention most firmly; and owing to other engagements, the curdy masses did not receive the attention they deserved, but round and caudate nucleated cells, about $\frac{1}{1000}$th of an inch in diameter, were found, and, at the time, were believed to be similar to those found in the interior of the glandular processes in Obs. III., Fig. X., though there were certainly no such processes seen during the short examination of the curdy masses. The opening was carefully closed by dry lint and strips of adhesive plaster, slight pressure being also made.

April 18th.—Experienced great relief from the evacuation of the fluid from the tumour. The cavity again contains a considerable quantity of fluid, as evidenced by distinct fluctuation; but there is not the same amount of tension as before, and there exists much less pain on pressure; plasters not disturbed; some œdema of feet and legs; urine in large quantity, and loaded with urates.

April 20th.—Tumour more tense and painful; distinct fluctuation over a large portion of the surface; no discharge from the opening; plasters not disturbed; patient very desirous of further relief by puncture, which was refused. My colleague, Dr Kerr, then very kindly examined the case at my request; and considering the growth malignant and likely to open very shortly, confirmed the opinion, that much injury might result from further interference.

April 22d.—Tumour excessively tense and painful; health evidently giving way; œdema increased in extent.

The further progress of the case may be shortly stated. The tumour became more diffused some days after last report, and was never afterwards so tense or painful as before; the general indisposition steadily increased; occasional attacks of diarrhœa came on, and he was much troubled with cough, both of which symptoms were much relieved by small doses of squill and opium; the œdema steadily increased, and extended to the body. After about the 10th of May he was confined to bed; on the 30th, drowsiness came on, the pulse was scarcely perceptible, and the patient appeared to be sinking. On the morning of the 1st of June, he was evidently more feeble; he insisted on getting out of bed, and died in the chair almost immediately. The tumour did not ulcerate, and the plasters remained on until after death.

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[1] A considerable increase in the quantity of fibrin in the blood of cancerous patients was noticed by Andral, Heller, and by Drs Lenzberg and Morthier. See "Monthly Journal," vol. vii. p. 70, for a condensed account of Heller's observations, showing the constant, absolute, and relative increase in the amount of fibrin, and that this may equal 16 parts in 1000 of blood.

Post-mortem Examination, forty-six hours after death.—Body very much emaciated; slight general œdema.

Examination of the Tumour.—The skin and superficial fascia of the left side of the neck being dissected off, the clavicular and sternal portions of the sterno-mastoid muscle are seen to be separated from each other, and the whole muscle is much thinned and spread over the tumour. This is now seen to extend to the middle line, displacing the trachea and larynx—downwards under cover of the clavicle, and backwards under the trapezius muscle. The deep fascia of the neck appears to have given way where it covers the upper part of the tumour, but below it gives it a very tense covering. The inner and anterior part of the tumour is covered by the sterno-hyoid and thyroid muscles, and posteriorly and internally is the carotid artery, the back of the tumour resting on the vertebræ. After dissection, the surface is very soft, and in many places, a brownish creamy juice exudes from it, the softest parts now projecting in nodulated masses. The dimensions of the tumour are six and a-half inches vertically, five inches transversely, and three inches in thickness. A section being made through the whole mass, three and a-half ounces of thick, reddish, creamy fluid escaped, leaving an irregular cavity in the centre. The whole body of the tumour consists of cerebriform matter of the same characters throughout, not even excepting the most external part, which exhibits no trace of a cyst. Several small glands at the lower part of the neck contain a creamy fluid like that from the tumour. The thyroid body is of its usual size and healthy.

Thorax.—Twenty ounces of serous fluid exist in the left, and six in the right pleural cavity. Lungs healthy; heart small but healthy; several bronchial glands full of creamy fluid, and one situated between the bronchi is as large as the half of a pullet's egg.

Abdomen.—Liver small, probably not weighing three lbs.; its surface is covered with small projecting tubercles, each of the size of half a pea; structure very firm and mottled (cirrosis.) Stomach, intestines and pancreas, spleen and kidneys, quite healthy. Two enlarged lumbo-aortic glands are full of cerebriform matter; one measures one inch and a-half long, and the other, which is spheroidal, nearly three inches in its diameter. In the centre of the latter is a quantity of matter of a yellow colour, and very friable.

Microscopical Examination.—The fluid and solid parts of the tumour of the neck, and the cerebriform and creamy matter of the glands have a similar structure. This consists of numerous round, oval, caudate, or irregularly-shaped cells, measuring from $\frac{1}{1760}$th to $\frac{1}{850}$th of an inch in diameter, granular on the surface, containing nuclei and nucleoli in their interior; also of a number of free nuclei, and of a blastema, in which these structures are lodged.—(Fig. VIII.) The yellow, friable matter found in the lumbar gland consists of smaller and more irregularly-formed cells with one or two nuclei in each, and of a number of free nuclei and granules.—(Fig. IX.)

Fig. VIII. Fig. IX.

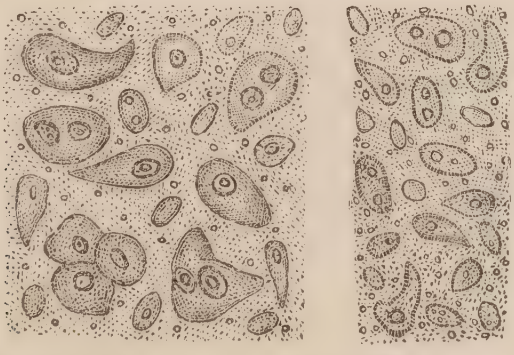

Fig. VIII.—Cells, nuclei, and granules from the tumour of the neck.
Fig. IX.—Cells, nuclei, and granules from the yellow friable matter in the lumbar gland.

Remarks.—The rapid development of this tumour, the numerous veins shining through the skin over it and the upper part of the same side of the chest, the existence of pain, the fixity of the swelling, and its position in the neighbourhood of lymphatic glands, indicated strongly its cancerous nature; but the distinct fluctuation, the smooth and rounded surface of the tumour, the absence of difficulty of respiration and deglutition, of distinct cancerous cachexia, and of disease elsewhere, appeared rather to refer the disease to a cystic origin in the thyroid body or its immediate neighbourhood; and when three and a-half to four ounces of serous fluid flowed from an exploratory puncture, the latter view of the case gained an amount of evidence in its favour, which it was difficult to resist, in the absence of any positive proof that the disease was cancer. But it will be asked, why the cells of the curdy masses, which escaped with the fluid, were not recognised as cancerous by the microscopical examination? To this it must be replied, that no single element in the structure of cancer is sufficient in all cases to decide its nature, as has been so fully shown by Professor Bennett and other histologists; and that, admitting that the examination was not made with the same care as would have been deemed essential had the glairy fluid not been met with, and that a drawing ought to have been made of the structure of the curdy masses, it is not improbable that nothing characteristic of cancer might have been found in the cells, and that some doubt might have remained under any circumstances, owing to the close resemblance of many of the cells to those found in Observation III., notwithstanding that the tubular processes found in the latter instance were not discovered in the former. No greater error can be committed than to argue against the systematic use of the microscope in the practice of medicine, because, though it may furnish us with the most absolute physical diagnosis in nine cases, it will not do this in the tenth; nor can any better excuse be brought forward for the neglect of such an invaluable aid, in the hands of practitioners conversant with healthy structural anatomy, than that which applies with equal force to the use of the stethoscope, speculum, and every other physical examination whatever,—namely this, that all such methods are liable to error, and therefore it is better to adhere to the more remunerative and convenient mysticisms of ancient medicine. The most important practical lesson which this case teaches us, is, that the escape of several ounces of a transparent and spontaneously coagulable fluid from a large tumour by no means indicates that it is not cancerous; whilst, when considered pathologically, it demonstrates, in the most positive manner, that the several structures found in cancer grow up in a fluid and spontaneously coagulable blastema; for there is every reason to believe that the fluid withdrawn by the puncture was replaced by a liquid of similar characters in the space of twenty-four hours; and it was shown, in six weeks afterwards, that the same position was occupied by three and a-half ounces of a creamy cancerous fluid, not capable of spontaneous coa-

gulation, and thus differing most decidedly in its chemical, its ordinary physical, and its microscopical characters, from the fluid to which it owed its origin. Here we have another instance in which chemical and structural changes go hand in hand in the formation of tissue,[1] showing how much physiology and pathology have to hope for from the united labours of the chemist and histologist,—how little from either independently of the other. There could be no mistake as to the oneness of the structure throughout its whole progress; for, on the most careful examination, no cystic structure could be detected; and the thyroid body, a very common seat of such formations in the neck, was perfectly healthy. In considering the origin and cause of this disease, it must be stated, that, after the patient's death, I ascertained that his mother was believed to have died of cancerous disease; and that, on the last occasion when he was in the country pursuing his usual avocation, he found the pressure and tension exerted upon the tumour by the strap which suspended his box so insupportable, that he was obliged to return home and leave his goods in the country. It thus appears that, besides a very strong hereditary predisposition, there existed a very important exciting cause, acting on that part alone where the tumour was developed; and, therefore, to these causes, in the absence of more probable ones, we must ascribe the disease. It is not to be overlooked that secondary deposits were found in lymphatic glands at the lower part of the neck, in several bronchial, and in two lumbar glands, while no trace of cancerous disease was detected in any organ.

Observation III.—*Large Cystic Tumour in the neighbourhood of the Thyroid Body, with Fungous Projections into the Trachea and Œsophagus; Discharge of Bloody Fluid after Tapping; Great Difficulty in Respiration and Deglutition; Death in a Paroxysm of Dyspnœa.*

Case.[2]—George Sutherland, æt. 43, saddler, of tall and spare habit, complexion pale, applied as an out-patient at the Aberdeen Royal Infirmary in November 1847, for advice regarding a tumour of the anterior and left side of the neck in the position of the left lobe of the thyroid body. States that the swelling began about six years ago, as a small tumour on the fore part of the neck, and that it has gradually increased since that time, without becoming painful. He knows no cause for it; but says, that a little before it appeared he had chancres and buboes, for which he took mercury until his mouth was very sore. Secondary symptoms followed, and at present he has a node on the left shin, and some copper-coloured spots on the left arm and hip. His child, born since he had the syphilitic affection, has always been quite healthy.

The tumour is now of the size of the clenched fist, and can be handled freely without pain. It extends under the left sterno-mastoid muscle, and obscures many of the parts on that side of the neck; but the os hyoides, the thyroid and cricoid cartilages, and the trachea, are still to be felt. Numerous enlarged veins are spread over the whole surface, which is irregularly nodulated, the substance

[1] See former remarks on this head in the "Monthly Journal," vol. ix., p. 1282.

[2] The particulars of this case, and of the post-mortem examination, have been very kindly furnished me by Dr Kilgour from his case book. The microscopical examination was made by myself, and the account here given of it, together with the woodcuts in Fig. X., have been taken from my own notes and drawings made at the time.

of the tumour being dense in some places and fluctuating indistinctly in others. Behind the sterno-mastoid there are two small tumours, each of the size of a large almond, but neither glandular enlargements nor other tumours can be detected elsewhere in the body.

December 26th.—Has been applying mercurial ointment to the tumour, and taking iodide of potassium internally for some time. Tumour somewhat softer in two places, and fluctuating. Complains of increasing difficulty in deglutition and of headache. Expectorates a quantity of blood, which communicates a sensation as if coming from the windpipe, about the lower part of the tumour. Has no cough.

January 31st, 1848.—A small opening was made yesterday into the most dependent part of the tumour, where fluctuation was most distinct; four or five ounces of a deeply-red serous fluid escaped, and coagulated very abundantly on the addition of nitric acid; the opening was closed with adhesive plaster.

February 1st.—Swelling very tense. A reddish discoloration exists around the puncture, from which no discharge has issued.

February 27th.—Swelling as large and tense as before; fluctuation more distinct; headache frequent, especially on the left side; expectorates a small quantity of bright red blood daily, and thinks it comes from the throat; has no cough; general health not so good as formerly. Ordered to take a grain of quinine night and morning.

March 12th.—Ordered to take fifteen drops of liquor potassæ three times daily.

May 14th.—Fluctuation in the tumour very distinct; patient very desirous to have another opening made. On passing an exploratory needle into the most depending part, near the former puncture, four ounces of a dark bloody fluid, without smell, escaped, and the size of the tumour is much diminished. A hard tumour, fully of the size of a pigeon's egg, can now be felt at the lower and right side of the swelling, and dense bands appear to cross from it to the sides of a thick cyst. Has had no expectoration of blood for the last fortnight; no cough; general health good.

May 20th.—Tumour has burst several times since last report, and discharged a quantity of bloody fluid on each occasion; wound small, and shows no disposition to ulcerate; swelling now much as before, and gives no pain, even on pressure; fluctuation more distinct; general health good; compresses ordered to be applied.

June 11th.—Wound has been healed up for some time; swelling exactly as before, and not painful; directed not to allow other punctures to be made, and ordered to take iodine and iodide of potassium.

November 13th.—Admitted into St Luke's Ward on October 30th, for ulcers on the left tibia, resulting from periostitis, and the formation of two abscesses. He has been taking the decoction of sarsaparilla, and the sores have improved. Tumour of neck much the same, and not painful; more glands behind the left sterno-mastoid, and some at the lower and outer part of the neck on the same side, are enlarged; has slight cough; again occasionally expectorates bright red blood. To have nourishing diet, and to continue the decoction.

December 7th.—Has remained in bed and taken the decoction, animal food daily, and wine; improvement continues; sores nearly healed; expectoration of blood still going on; tumour enlarged and very tense; fluid in separate cysts easily detected. Dismissed.

December 26th.—Patient said to be much worse, and when visited at home found to be labouring under great dyspnœa, so much aggravated by paroxysms, which are brought on by any excitement, that suffocation appears imminent; the tumour is larger, and now extends to the upper part of the sternum; cough frequent and very severe; voice much more hoarse than formerly; has expectorated a considerable quantity of blood at different times since he left the hospital; difficulty of deglutition very great, and has increased much since he was last seen.

January 5th, 1849.—Died to-day in a paroxysm of dyspnœa.

Post-mortem Examination, thirty hours after death.—The tumour appears somewhat less in size; the skin, removed from the neck without exposure or incision of the substance of the tumour, separated readily from every part, except in the neighbourhood of the punctures, where firm adhesion to the deeper textures had taken place. The trachea and œsophagus were divided close to the sternum. A transverse incision was made above the os hyoides, and the parts between these sections were removed. The handle of a scalpel passes with some difficulty through the œsophagus from below upwards, and on opening the tube it is seen to be obstructed or narrowed by a serous cyst, which presses on it from without, and is of the size of a small walnut,—the remaining cervical part of the œsophagus is narrowed; two fungous tumours, each of the size and shape of half the kernel of a hazel-nut, exist on the left side of the tube, at its point of junction with the pharynx, and are also seen from the inside.

On opening the larynx and trachea from behind, the mucous membrane presents a deep-red colour, and a covering of mucus; much swelling and infiltration exists in the parts in the vicinity of the glottis; a fungous tumour, of the size of a large pea, projects into the trachea near to the sternum; the thyroid and cricoid cartilages are carious or softened, but the tumour penetrates the air-tube only in the spot above named.

The tumour itself consists chiefly of three large and prominent cysts, the largest being of the size of a hen's egg. Two of them communicate, and these were emptied by opening one. The whole three contain about seven ounces of thin bloody serum. The walls of the cyst are thick, and densely fibrous in front, the spaces between them being occupied by fibrous tissue in bands. As the hasty dissection was made by gas-light, and the thyroid body was not seen, it cannot be stated with absolute certainty whether or not the disease originated in it; but the site of the thyroid, and the whole space extending from the upper part of the thyroid cartilage to the sternum, are entirely occupied in front by the cysts, and behind by the soft reddish mass which covers the sides of the larynx and œsophagus, extending also between the œsophagus and trachea, and protrudes into the largest cyst behind, as well as into the trachea and œsophagus. The mass exists in largest proportion on the left side of the neck, and, except in the position of the protrusions last named, is covered by a dense fascia. A small prolongation of it passes behind the sternum into the chest. Two or three enlarged cervical glands exist on the outer side of the tumour; but these are quite free from it, and present no traces of similar disease. One of them is larger than a pigeon's egg.

Chest.—Pleuræ and right lung healthy; upper lobe of left lung slightly hepatised. Heart healthy.

Head and abdomen not examined.

Microscopical Examination of the Tumour.—The reddish-brown soft fungous mass is of the same structure throughout, and consists of villous processes, which float from each other in water, and can easily be separated with needles. These processes are formed of a central branched duct, each of the divisions of which terminates in grape-like bunches of lobules (Fig. X., *a*), indistinguishable from portions of an ordinary compound gland. The ultimate lobules consist of single or branched cœcal tubes, or of tubes ending in flask-shaped or otherwise dilated extremities (*b*, *c*, *d*). Their basement membrane is distinct, though thin, and supports on its interior a beautiful and complete covering of spheroidal epithelium, the cells of which measure from $\frac{1}{1250}$th to $\frac{1}{1000}$th of an inch in diameter, are finely granular, and contain a bright nucleus. They are immediately distended on the addition of water (*e*), and become caudate (*f*) on the application of the slightest pressure. The mass contains also a few short and nucleated fusiform cells (*g*), and two small round cysts, attached to

a common stalk. Each of these cysts measures $\frac{1}{15}$th of an inch in diameter, and consists of a thin membrane, on which is a beautiful vascular network, derived from the vessel of the common stem, the interior being occupied by a clear fluid.

Fig. X.

Fig. X. shows the structure of the reddish-brown soft substance in the tumour :—*a*, appearance of villous processes, when slightly separated and viewed by a lens; *b*, edge of one of the villous processes; *c* and *d*, tubes lined by epithelium, and terminating in bifid or dilated extremities; *e*, epithelial cells of the tubes and vesicles, somewhat distended by the action of water; *f*, the same rendered caudate by pressure; *g*, fusiform cells, found in very small number.

Remarks.—The tumour, in this instance, differed from that in the last case in commencing in the middle line of the neck,—in being painless throughout its progress,—in enlarging very slowly,—in occasioning but little inconvenience for many years, and then causing great difficulty in respiration and deglutition,—characters which, with the distinct fluctuation towards the last, and the history of the case, referred it to the class of cystic tumours of the thyroid body. Yet, it was covered by large veins,—for a while, the fluctuation was indistinct,—and glandular enlargements took place in its vicinity; whilst, after death, fungous growths proceeding from it were found to have penetrated the trachea and œsophagus,—in all which respects the growth partook of the general characters of cancerous disease. The fluid removed by tapping appears to have been albuminous, and rapidly reproduced; such tendency to hemorrhage from the walls of the cyst resulting from the evacuation, that its repetition became exceedingly dangerous, and was strictly forbidden. The patient died from asphyxia, produced by the protrusion of the fungous mass into the interior of the trachea. The post-mortem examination revealed the compound cystic character of the disease, and disclosed the soft, reddish-brown mass projecting into the posterior wall of the largest cyst—into the trachea and œsophagus. The ultimate structure of this soft mass resembled that of secreting glands; but its cells presented no characters which could lead to the belief that they

were capable of producing others like themselves, nor did they offer any explanation of the mode of extension of the growth into the trachea and œsophagus. So far as is at present known to us, the disease was irremediable; for iodine had no effect in producing absorption, the tapping was attended with serious danger from hemorrhage, and every other operative procedure was quite out of the question.[1]

General Remarks on the Diagnosis of Tumours of the Neck.—This always presents great difficulties when the tumours are of large size, only slightly painful, and fluctuate indistinctly. If a complete and accurate history of the case cannot be obtained, more than a general diagnosis may be impossible; but when the precise position of the tumour at its commencement can be made out, when its relations to the larynx and trachea, and its mobility in regard to them and the surrounding parts when it was of small size, can be ascertained, together with the rapidity and manner of its growth, there will be little difficulty in arriving at a satisfactory conclusion.

A *tumour* developed in the substance *of the thyroid body* presents itself in the front of the neck, is usually larger on one side than the other, is firmly connected with the larynx and trachea, moves freely with the larynx in deglutition, and when it is displaced laterally by manipulation. The other features vary with the nature of the tumour.

In ordinary bronchocele (hypertrophy) the swelling is soft, projecting, elastic; without fluctuation, pain or tenderness on pressure; it occurs usually in early life, in the female sex, and in particular districts of country; it is simple in its nature throughout, and presents no tendency to degeneration or change of structure; it in no way interferes with respiration or deglutition, nor does it affect the patient's health or comfort until it becomes of very large size, when difficulty of respiration and deglutition, with frequent headaches, occasion the greatest distress, and may end in the death of the sufferer.

In *cystic disease of the thyroid* the nature of the tumour becomes manifest, sooner or later, by the presence of fluctuation in one or more cysts, by a glairy, serous, or sero-sanguinous fluid escaping readily along a grooved needle when introduced, the fluid containing no cellular formations when examined microscopically, or having

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[1] A somewhat similar case will be found noticed in the catalogue of the Museum of St Bartholomew's Hospital, pp. 353 and 354. The patient, an elderly woman, had a large cyst in the right lobe of the thyroid body,—a spontaneously coagulable fluid was evacuated from it on two occasions, after which its wall inflamed, pus and lymph filled the cavity, ulceration of the posterior wall of the cyst and the adjacent part of the pharynx took place, and the patient died suffocated by the sudden discharge of the contents of the tumour, and the passage of some of them into the larynx.

such a structure as is inconsistent with the idea of the existence of cancer,—by the formation of the tumour taking place at or after the middle of life,—by its slow and painless growth, and by the slight inconvenience it occasions as long as its size is not very great.

In cancerous disease of the thyroid (usually scirrhus) the tumour appears between forty-five and sixty-five years of age, is of great and uniform density, and generally painful; it is developed rapidly, and may attain a large size in the course of a few months; it accompanies the larynx in its movements, but shortly limits their extent by attaching the organ to the surrounding parts; it occasions great difficulty of deglutition and respiration from an early period;—hoarseness, cough, and spasmodic action of the muscles of the larynx and pharynx come on and increase in their intensity,—the distress and anxiety of the patient, his sallow complexion and emaciation, marking him out as the subject of a steadily advancing and destructive malady.

In *medullary cancer of the thyroid* the surface of the tumour may be even and tense, or indistinct fluctuation may be perceived; the other characters, depending on the steady infiltration of the surrounding textures, distinguishing the disease from other tumours of the same part.

Enchondromatous tumours are to be recognised by their great density, the slowness of their growth, and the absence of any signs of the extension of the affection to the surrounding parts, and of general evidence of the existence of malignant disease.

The diagnosis of *tumours of the neck, not connected with the thyroid body*, is to be established by reference to the general characters which distinguish them in other situations, every particular of their history and mode of growth being carefully ascertained as essential points, and sufficient care being exercised lest the presence of a quantity of coagulable fluid, in the interior of a cancerous tumour, as in Obs. II., lead to the belief that it is of a cystic character.

I shall complete this paper by a short consideration of the important question,—*Whether or not fibre is an essential element in the structure of cancer?*

Much difference of opinion still exists as to what are the parts of a cancerous growth which are essential to it. Professor Bennett states that fibres, cells, and a viscous fluid, are the three essential elements of a cancerous formation. Lebert regards the cancer-cell as the only distinctive, constant, and essential element, the predominance of one or other of the accidental and secondary elements determining the varieties of form and appearance; yet he regards the fibres as next in point of importance and frequency, and speaks of them in encephaloma as pale, fine, and in small quantity. Müller says that the fibres of encephaloma are indistinct, and that the fusiform cells are arrested in their development into fibres;—whilst Vogel states, that in encephaloma fibrous structures are wholly absent. As

has been before remarked, it is excessively difficult to state whether the fibres which are found in many tumours are really cancerous, or whether they belong to the proper structure of the organ in which the tumour has been developed; and, consequently, careful examinations of cancerous formations in organs which contain no fibrous tissue in their healthy state, become of extreme importance in leading to a true determination of the mode of development and actual position of the fibrous element in cancer.

From the examination of the cancer of the brain here recorded, and of numerous cases of encephaloma, I am led to believe that fibres are by no means invariably to be found in such growths, and that their fibrous element is accessory and non-essential. If this be so, the existence of fibrous tissue in most cancerous structures remains to be accounted for in either of two ways,—viz., by hypertrophy of the normal fibrous tissue of the part, or by a new development of fibre from the recently diffused blastema,—a development commenced and completed under the influence of the determining energy exerted by the fibrous tissue of the part itself, agreeably to the law of analogous formations. Presuming that the fibrous element of cancer is developed in either of the methods just indicated, its absence in cancer of the brain is readily accounted for, as in that organ there is no fibrous tissue to acquire an unusual development, or to determine the formation of new tissue of its own kind from cells. *If* proper *cancer cells*, of a fusiform shape, *ever become transformed into fibres*, their presence in cancer of the brain, unaccompanied by the fibrous element, may be owing to a deficiency of the stimulus necessary to ensure such development, and may possibly be dependent on the absence of fibres in the original and healthy texture of the organ.

In conclusion, I would suggest that a carefully conducted statistical inquiry into the relative amounts of the various elements of which cancerous deposits are composed, viewed in connection with the normal structure of the organs in which each one is found, would be of great service in the further examination of this difficult problem, and that it might perchance lead to its satisfactory solution.

Part Second.

REVIEWS.

Lectures on Inflammation, delivered in the Theatre of the Royal College of Surgeons of England. By JAMES PAGET, Professor of Anatomy and Surgery to the College. London, 1850. Pp. 57.

Guy's Hospital Reports. Second Series. Article on the State of the Blood and the Blood-Vessels in Inflammation; ascertained by Experiments, Injections, and Observations by the Microscope. (The "Astley Cooper" Prize Essay for 1850.) By T. WHARTON JONES, F.R.S.

General Pathology, as Conducive to the Establishment of Rational Principles for the Diagnosis and Treatment of Disease, &c. By JOHN SIMON, F.R.S., &c. London, 1850. Pp. 288.

As the science of pathology advances, the more it becomes evident that what has been called inflammation—that process which gives rise to so many important changes in the animal economy, should be thoroughly understood. Indeed it must be evident that this is the fundamental question,—the very basis on which all our ideas of disease are formed. Unless we know it, we know nothing. Theoretically, the subject mingles itself with all our reasonings; practically, we encounter it in almost every case that is presented for treatment. During the last twelve years,—that is, since the period when Schleiden and Schwann opened up to us the theory of cell growth,—our ideas of nutrition, secretion, excretion, and other vital processes, have undergone a complete revolution. The microscope and organic chemistry have enabled us to penetrate more deeply into the mysteries of structure; and there have in consequence been evolved laws of development which exhibit a wonderful identity, not only between vegetable and animal forms, however diversified, but what is most important to medical men, between healthy and diseased actions,—between a correct physiology and a sound pathology. At present, nothing can be more clear, than that physiology and pathology are only branches of one subject. They must be studied together; for it is as impossible for a man to understand the phenomena of inflammation, tuberculization, and morbid growths, without a knowledge of the manner in which nutrition is effected in plants and animals, as it would be for him to work out a mathematical problem in ignorance of the axioms on which his reasoning must be founded. Hence when, in 1842, Dr Hughes Bennett first maintained inflammation, from its commencement to its termination, to be a process of "abnormal nu-

trition," he not only supported a doctrine which the majority of succeeding inquirers have confirmed, but introduced an expression into pathology which, though severely criticised at the time, has since been found to be so happy, as to have been very generally adopted.

The progress of histological pathology has taught us, that during normal nutrition, the blood in the blood-vessels is continually acted upon by the tissues to which they are distributed. It is in consequence of a force from without, and not as a result of a power from within, that the fluid matters of the blood are continually exuding through the capillaries to build up and maintain the tissues. This force must be a vital force,—selective and attractive,—inherent in the parenchymatous structures of organs and tissues. In health, the blood in the capillary vessels is everywhere the same, and the structure of the numerous capillaries themselves is also everywhere alike. By what possible agency, therefore, can there be selected from this blood, and attracted or drawn through these capillaries, structures and fluids so various? Why should one organ select the materials which form bile, another those which constitute urea, and a third such as will produce the acid gastric juice, if it be not an agency in the first case connected with the cells of the liver; in the second, with those of the kidney; and in the third, with those of the gastric follicles? Again, if these various secretions be unusually increased in quantity, is it not because the selective and attractive force is augmented in energy? Does not hypertrophy of an organ or of a limb imply increased action in the parenchyma of the one and of the other, as the cause of that hypertrophy? If so, we cannot ascribe the increased nutrition primarily to the blood or to the vessels. Besides, do we not know that nutrition is carried on, and the formation of the embryo commenced, in the germinal membrane long before the existence of either blood or blood-vessels? Of this there can be no doubt; and the same observation applies to all new formations, healthy or diseased. Hence the blood-vessels themselves are the results, and not the essential causes, of growth. Increased vascularity always follows, and never precedes, morbid actions.

Now, as regards the subject of inflammation, the great question we have to discuss is this,—Do the various changes which constitute that morbid process essentially depend on perversion of the vital selective and attractive power so necessary for nutrition, or do they depend on altered physical conditions of the blood or blood-vessels? To this question the three authors whose works are named at the head of the present article, have favoured us with three different replies, and we now proceed to examine and report upon each.

Mr Paget considers that the maintenance of any part by nutrition depends upon four conditions—1st, A regular, and not distant, supply of blood; 2d, a right state and composition of that blood; 3d (at least in most cases), a certain influence of the nervous force;

and, 4th, a normal state of the part in which nutrition is to be effected. He believes that inflammation may have its beginning in any one of these conditions,—as in an alteration of the blood in rheumatism, in an alteration of the nervous force in irritation of the retina, in an alteration of the proper elements of the tissue in inflammation of the cornea; but that probably it is never fully established without involving in error all the conditions of nutrition. He considers such alterations should be considered as concurrent events, rather than as a series of events of which each stands in the relation of a consequence to one or more of those that preceded it. Hence, if we understand him rightly, inflammation may sometimes arise from one cause, sometimes from another, but is only perfected when several altered conditions operate together.

Mr Wharton Jones's inquiry is limited to the state of the blood and the blood-vessels in inflammation; and the conclusion he has arrived at seems evidently to be, that the changes they undergo are the true causes of that morbid process. He is of opinion that, owing to some physical change in the part, the blood becomes more viscous, from an increase of its fibrin. That, in consequence, the blood corpuscles exhibit a great tendency to adhere together, as well as to the sides of the vessels, whereby congestion and stagnation within them are produced; and, as a result of this, the vis-a-tergo pressure causes the serum and liquor sanguinis to exude. Hence, according to this author, the well-known increase of fibrin in the blood during inflammation is the primary change, giving rise to variety of physical phenomena. How this change takes place is not very clearly stated. He points out that, after section of the ischiatic nerve in the frog, stagnation of the blood is not so readily produced in the web as when it is entire. Hence it is not owing to paralysis of the limb. At p. 40, he seems to ascribe it to exosmose of the serum, which may indeed serve to explain the action of saline solutions, but completely fails in the case of other mechanical irritants, such as a grain of sand. He seems to doubt its being caused by solution of the coloured or colourless corpuscles (p. 68). Indeed, so far as we can discover, this essential point is left in the greatest obscurity.

Mr Simon is distinctly of opinion, that the first change which occurs in an inflaming or an overgrowing part, and which leads to its becoming loaded with blood, is not a reflex change operating through the nerves, but it is a direct change, effected by the living molecular structure of the part on the blood which traverses it, or on the vessels which convey that blood (p. 77). The growing elements of the part—hurt by physical violence, or affected by extremes of temperature, or thrown into rapid chemical changes, or overburdened with their own specific stimuli from the blood—strive to grow more, or in their manner of growth to differ from their previous state. The sudden origination of this effort (as it occurs, for instance, after mechanical injury), suffices apparently in itself to

derange the currents of the capillary circulation, to flood the tissue with serous exudation (?), and to lead to those phenomena which are considered pathognomonic of inflammation (p. 82). Hence, then, according to Mr Simon, it is not to the nerves, blood, or blood-vessels, we must look for the primary cause of inflammation, but altogether to the structure of the part inflamed.

From what we have previously stated, it will be readily understood, that of the three opinions before us, we consider Mr Simon's to be the correct one. We cannot agree with Mr Paget, that various causes may operate, or that the changed conditions to which he alludes, must all necessarily concur. A changed state of the nervous influence, and of the blood, may certainly arise, and very commonly does; but this is a result, and not a cause. Then, so far from increased spissitude of the blood always preceding inflammation, we have no proof that this is ever the case. That such spissitude is owing to an augmentation of fibrin, is unlikely, for the facts and arguments of Zimmerman have sufficiently shown, that the increase of this principle is also the result, and not the cause, of inflammation,—an opinion which has been adopted by Mr Simon. The more, then, we examine this question, the more do we feel satisfied that the primary change is in the vital properties of the tissues inflamed. In hypertrophy, their selective and attractive powers are both increased—in inflammation the selective force is destroyed, while the attractive one is augmented in energy. Hence the exudation, thickening of the blood, and the phenomena of congestion and stagnation, which have been so well described by Mr Wharton Jones. Blood is not sent or determined to the part, as has long been supposed—it is drawn or attracted there; and it is only by using the latter expressions that the real cause of the increased amount of that fluid in inflamed parts can be understood.

Whilst we consider that Mr Simon's exposition of the nature of inflammation is, on the whole, very just, he himself must be fully aware that it is in no way original. In point of fact it is the very doctrine which has long prevailed in the Edinburgh school of medicine. It was very ably maintained by Dr Alison, when Professor of the Institutes of Medicine, and is the one which for some years has been taught by the present holder of that chair. Their views, supported by various facts and arguments, have been published,[1] and yet it is curious that while Mr Simon adopts them, the Edinburgh professors are no where alluded to. Again, whatever differences may exist in opinion between the three authors under review, there is a wonderful unanimity among them in *suppressing* (we use this word advisedly) all allusion to the labours of the Edinburgh pathologists. We regret to say that this tendency to injustice among many of the London teachers, is increasing, and has of late been

[1] Article "Inflammation," in Dr Tweedie's "Library of Medicine." Treatise on Inflammation as a Process of Abnormal Nutrition : 1843.

more and more forced upon our attention. Whether it arises from the incessant demands of the three weekly London journals, for any kind of hurried compilations that will fill their pages—whether the high tone that should ever characterise the lovers of truth is weakened by the commercial spirit of so vast a metropolis—or whether the custom of teaching there as a mere means to practice, leads to the imposition, on the unlearned, of a temporary appearance of originality, at the expense of a permanent reputation, it is difficult to determine. Perhaps it is no bad indication of the depression of the London schools, when we find such able professors as Messrs Paget and Simon, not only delivering but publishing lectures which expose them to such imputations.

But to return. An increased attraction of the parenchyma of the tissues affected, being the ultimate cause of inflammation, so far as we can determine it, we may next ask, what constitutes that morbid change—in other words, what is inflammation? Mr Paget gives no reply to this question. Evading a definition, he simply describes the phenomena, for he observes, "we may indeed say that stagnation of blood, or effusion of liquor sanguinis, shall be the condition *sine qua non* of inflammation; we may call whatever falls short of this 'active congestion,' 'irritation,' or by any other name; but in practice such distinctions are often impossible, and sometimes untrue, and in study the terms are convenient for the sake of brevity rather than of clearness." Now, we think this sentence betrays much bad reasoning and false assumption. The question really is, not what is distinguishable in, or coincides with, that nondescript something styled by many persons "practice," but what is really and absolutely true. Is either the stagnation of blood, or exudation (not effusion) of liquor sanguinis, really an essential phenomenon of inflammation? If it be, why not say so? if it be not, why avoid demonstrating the fact? Mr Paget is perfectly aware that it has been maintained that exudation *is* the essential phenomenon, the *sine qua non* of inflammation; yet instead of discussing this subject fairly and honestly, he gets rid of it in the manner above quoted. As a consequence, he has no opinion as to what inflammation is—he professes the most indefinite notions as to its essential nature—and his conclusions, both in theory and practice, are impotent in the extreme.

Dr Alison, in his article "Inflammation," in the "Library of Medicine," observes, "in order to give the requisite precision to the general notion of inflammation, as a local change of the condition of any part of the body, it seems only necessary to include in it, besides the pain, swelling, heat, and redness, the tendency always observed, even when the changes in question are of short duration, to effusion from the blood-vessels of some new products, speedily assuming, in most instances, the form either of coagulable lymph or of purulent matter." We consider this passage to contain the germ of the doctrine afterwards brought forward by Dr Hughes

Bennett, when he maintained that exudation of liquor sanguinis was the essential phenomenon of inflammation. And here it may be observed, that when pathologists speak of an exudation—a term synonymous with that of the Germans (*exudat*) — there ought always to be understood transudation of the liquor sanguinis through the walls of the blood-vessels. It should be separated from the terms effusion and extravasation, which refer, the first to the transudation of serum only; and the second to hemorrhage from the smaller vessels by rupture of their walls. It is the more necessary to understand this, because we observe that Messrs Paget and Simon use the terms exudation and effusion synonymously. The former even speaks of inflammatory effusions (!) of blood. Those who speak thus loosely, and who seem to have formed no clear distinction between the *effusion* of serum, the *exudation* of liquor sanguinis, and the *extravasation* of blood, must, indeed, find it very difficult to understand what is meant, when it is contended that the word inflammation ought to be banished from medical language, and the term exudation substituted for it. But this is what the progress of pathology is slowly but surely bringing about.

Again, Mr Wharton Jones says,—"At the present day, when the question 'What is inflammation?' is asked, the word is usually employed in the theoretical sense; and the answers commonly given may be reduced to two, viz., 1. Inflammation is an increased action of the vessels, with acceleration of the flow of blood in the affected part. 2. Inflammation is a diminished action of the vessels, with retardation of the flow of blood, or actual stagnation of it in the part" (p. 2). Now we wonder whether Mr Wharton Jones was really unaware that inflammation had been disproved to consist in either one or the other of these conditions, whilst it had been argued that exudation was its essential phenomenon. We charitably suppose that he was ignorant of these things, otherwise he also will become chargeable with *suppressio veri.*

That inflammation does not essentially depend either upon increased or diminished action of the vessels, or upon acceleration or retardation of the flow of blood, has been shown by all those who have made careful experiments on the living blood-vessels, and may be proved by those instituted by Mr Wharton Jones himself. Here we must do this author the justice of stating, that he has taken great pains to investigate the structure and arrangement of the blood-vessels in the web of the frog's foot, as well as the effects produced upon them by different stimuli, under various circumstances. He has justly entitled himself to the reputation of an accurate and indefatigable observer, by numerous works well known to the scientific world; and although we cannot, in the present instance, coincide with him in all his conclusions, we have been enabled to confirm, by our own investigation, most of the facts he has described in the essay before us. Thus, the following observations on the small arteries are well worthy consideration :—

"1. Constriction may slowly take place, and be slowly succeeded by the normal width. This I have found to be the effect of the action of the sulphate of atropia. A solution of this substance in the proportion of three or four grains to the ounce of water, applied to the web, causes constriction of the arteries, in about the same time that it causes dilatation of the pupil when dropped into the eye. In other words, it causes contraction of the circular fibres of the muscular coat of the arteries in about the same time that it causes contraction of the radiating fibres of the iris. As in the case of the iris, the arteries slowly recover from the action of the atropia.

"2. Constriction may quickly take place, and be soon succeeded by the normal width, or a width not much exceeding the normal. This has been found to be the effect of the moderate application of cold, mechanical irritation, and galvanic irritation. When a drop of cold water is applied to the web, the arteries are seen to become constricted, but they soon resume their previous state. Slight pressure on the web, with a blunt point, for example, in the situation of an artery, excites constriction of it, but by and by the vessel becomes dilated as before. When the web is subjected to the influence of a single pair of plates, constriction of the arteries is induced, succeeded by dilatation, as in the preceding cases.

"3. Constriction either does not take place at all, or, when it does, it very rapidly gives place to great dilatation. This is the effect of such agents as the following:—a solution of sulphate of copper (gr. xvj—ʒj) with wine of opium (ʒj) dropped on the web, I have generally found to cause primary dilatation of the arteries. A strong solution of common salt I have found to have a similar effect, only, perhaps, momentary constriction has more frequently preceded the dilatation. Battley's *liquor opii sedativus* usually first excites constriction of the arteries, but dilatation quickly supervenes. A drop of spirit of wine immediately causes constriction, but this is soon followed by dilatation of the arteries.

"4. Dilatation, preceded or not by momentary constriction, may slowly yield to constriction, which remains permanent. This is the effect of sulphate of copper in a concentrated solution. When a point of blue-stone is rubbed on the web, in the neighbourhood of, or over an artery, the vessel slowly but steadily becomes completely constricted, and remains so."

Whilst these changes may be observed to take place in the arteries, we are told that he has not been able to observe that the capillaries undergo any change in width, attributable to contractility of their wall. He has not been able to perceive that when capillaries are cut across, the mouths become constricted, much less their canal as a whole. Under such circumstances, it is true, they do not pour out blood; but this he thinks is owing, not to any constriction of their mouths, but to the elastic and tonic closing in of the tissue around them. With regard to the veins, he asks—

"Are the walls of the veins endowed with contractility, by virtue of which their width is made to vary? On this question I would first observe, that I have not been able to satisfy myself, that when a vein is cut across, any actual constriction of it at the place of section occurs. Blood certainly does not always, or even generally, escape from the orifices, not even the lower; but this is owing to the adjacent tissue, by virtue of its elastic and tonic reaction, closing in around the open mouths of the cut vein, as appears from the fact that blood is sometimes seen extravasated into the tissue around the orifices, especially the lower.

"Nevertheless, I think it must be admitted, that variations, though slight, in the width of veins, do take place, and that by virtue of the operation of a contractile power, which we must suppose to be seated in the outer or cellular coat

of their wall. The facts on which this admission is founded, are the following: first, veins may sometimes be seen with their diameter less, and their wall thicker, than similar veins usually are; secondly, in anemic frogs, in which, from the emptiness of the vessels, dilatation cannot be attributed to distension, veins may sometimes be seen unusually wide, with their wall thin, and presenting none of those slight constrictions here and there which, under other circumstances, are presented by veins, and which are similar to, though in a very much less degree than, what are presented by arteries; thirdly, the tissue of the outer coat is one which in certain other organs manifests contractile power; fourthly, the connection between the outer coat of veins and the adjacent tissue is, as in the case of the arteries, of such a loose nature, as to allow of a diminution of the caliber of the vessel by contraction of the wall. In the case of the capillaries, it is, as we have above seen, different."

Notwithstanding our confidence in the general accuracy of Mr Wharton Jones as an observer, we are by no means satisfied that the capillaries undergo no change in caliber when exposed to the action of irritants. We confess that we have been unable to make exact measurements of these minute vessels so circumstanced, because we have always found that immediately after the application of a stimulus, the animal struggled, and the parts were greatly displaced. Besides, it must always be difficult to make accurate observations on such minute tubes in the living animal. But we have no doubt that the circulation, on adding weak acetic acid, is greatly accelerated in the capillaries; and our impression is, that they are first narrowed in caliber, and afterwards somewhat dilated. On this point, however, farther investigations are required.

Many of the effects on the small arteries, so well described by Mr Wharton Jones, have previously been noticed by Bruch of Heidelberg, Brücke of Vienna, and others. They have also been confirmed by Mr Paget, who has made similar observations on the blood-vessels of the bat's wing; concerning the arrangement of which he says—

"The principal arteries and veins lie side by side at each border of the metacarpal bones and phalanges, and the intervening membrane contains their numerous branches and capillaries. But, very generally, the arteries of the second and third order of branches pass into veins of corresponding size, without any intermedium of capillaries. The capillaries are rather in the position of off-sets from the continuous channels of arterial and venous loops, than in their more ordinary relation as intermediate canals, leading from arteries to veins. I know not to what this condition may have reference, or what purpose it may serve; but, in relation to the phenomena of inflammation, it appears to have no other effect than that, through the abundant anastomosis, a great obstacle to the movement of the blood is scarcely likely to occur."

We suspect that Mr Paget has over-estimated the importance of the arrangement of blood-vessels he describes. The real capillaries are very minute, and, besides, so much obscured by pigment cells, as to render accurate observations upon them impossible. Under a power of 800 diameters linear, however, we have observed them, in a favourable specimen, to form a dense plexus of vessels, intermediate between the arteries and veins, where they doubtless perform

exactly the same functions as in all other tissues. No argument, therefore, can be drawn from observations made on the contractility of small arteries in the bat's wing, as to the movements of the capillary vessels there or the blood contained in them.

It has been observed, both by Mr Wharton Jones and by Mr Paget, that when the small arteries dilate, the flow of blood in them is accelerated. Here, however, the former makes a distinction between uniform and partial enlargement, in which latter case the current of blood is retarded. These observations on the arteries have led to various speculations as to the influence they exert over the circulation in the capillaries. The views of Brücke of Vienna on this subject are reported in our last number; the following is the account given by Mr Wharton Jones:—

"In observing the circulation in the web of the frog's foot, we can see distinctly that the flow of blood in the capillaries is accelerated or retarded by acceleration or retardation of the flow in the arteries. Now, we have above seen, that the heart's action being the same, the flow of blood in the arteries is accelerated or retarded according to variations in their width, which variations, on their part, are owing to the contractility of the middle coat of the arteries. The arteries are thus the great regulators of the capillary circulation, though they have no primary share in the propulsion of the blood.

"In accordance with what has been above said, we may, for the sake of conveniently describing the influence of the arteries on the capillary circulation, recognise four degrees of width which may be presented by those vessels with corresponding degrees of rapidity of the flow of blood, viz.—1st degree, Artery so much constricted that there is no room for the passage of the blood-corpuscles. 2d deg., Artery just wide enough to permit the blood to flow, though with difficulty. 3d deg., Artery neither constricted nor much dilated, with the blood flowing freely. 4th deg., Artery much dilated, and the flow of blood stronger and more rapid than in the 3d degree.

"In consequence of the peculiar connection between the arteries and capillaries on the one hand, and the capillaries and veins on the other, we can observe the influence which a given artery especially exerts on the flow of blood in the capillaries to which it leads. Thus, when the width of an artery is reduced to the first degree by constriction, and *vis-a-tergo* no longer transmitted, the direct flow of blood is of course arrested in its still pervious terminal part, and in the capillaries to which it leads; but, if the constriction continue, blood is soon seen to regurgitate from the anastomosing arteries, and from the veins through the capillaries into the terminal part of the affected artery. The red corpuscles of the regurgitating and slowly moving blood aggregate together, and in some places become stagnant.[1] If, however, the constriction of the artery should cease, so that its width returns to the third degree, the flow of blood becoming re-established in the same degree, we see the retrograde stream in the capillaries and terminal part of the artery changed into the natural direct one, and the stagnation, when it exists, dissipated. When the width of an artery is in the second degree, the stream may continue direct, but the circulation in the capillaries of the part is sluggish; in some perhaps the blood may be stagnant.

"When all the arteries of the web are more or less in the second degree of width, there is sluggish flow, with great accumulation of red corpuscles in the capillaries and veins, with here and there stagnation. In such cases, on the application of one of the agents above mentioned, the fourth degree of

[1] "This stagnation takes place the more readily when the part is kept exposed to the drying action of the air, and still more when the plasma of the blood is rich in fibrin and albumen, as it appears to be in autumn."

width of artery and rapidity of flow is produced, the consequence of which is, that the congestion and stagnation are quickly dissipated. Dilatation of the arteries, and acceleration of the flow of blood, in somewhat the same degree, I have found to be the effect of section of the ischiatic nerve.

"In one case, the arteries of the web were more or less constricted, the circulation sluggish, the blood in the capillaries here and there stagnant. A drop of the solution of sulphate of copper, with vinum opii, was applied, whereupon the arteries immediately became dilated and the circulation brisk.

"In another case, an artery was much constricted and varicose from the application of a solution of atropia to the web, and the blood was just flowing. A drop of Battley's *liquor opii sedativus* was applied, whereupon there took place full dilatation of the artery, and a brisk flow of blood.

"In a third case, the two larger webs of a frog's foot were very much congested; the blood stagnant in a great many capillaries, and flowing slowly or oscillating in others. The flow in the veins sluggish. The arteries all more or less constricted. Sometimes one artery would be seen to dilate a little; and in proportion as the flow of blood in it thus became free, the circulation in the capillaries to which it led was re-established. In the second largest web, one artery was seen dilated and constricted at intervals. At two of the dilated places, red corpuscles were seen adhering to the wall of the vessel. The application to the web of solution of salt, was followed by uniform dilatation of the arteries, a brisk flow of blood, and the dissipation of the congestion."

Mr Wharton Jones makes another observation on this subject, however, which deserves comment. He says (pp. 17 and 18) that the constrictions in the artery take place from the branches towards the trunk. We think this important, because it implies that the changes proceed from the smaller vessels to the larger; and we are of opinion, that although, as previously stated, similar changes in the capillaries have not been accurately observed, it will be found that the morbid alterations, whatever these are, which induce inflammation, commence there, and retrograde towards the smaller arteries. In other words, the capillaries are affected primarily, not secondarily.

But whatever influence may be exerted on the capillary circulation by the uniform or partial contraction of the smaller arteries, it is clear, even according to the description of Mr Wharton Jones, that they only cause temporary congestion and stagnation. Now, these conditions, neither separately nor conjunctly, constitute inflammation—they may lead to or accompany this state, but in themselves they must be distinguished from it. In short, something else is requisite; and this Mr Jones acknowledges when he feels obliged, as we have previously seen, to suppose an increase of viscidity in the blood from augmentation of the fibrin, as well as a power (with him physical, and acting on the blood by *vis-a-tergo*) whereby exudation is produced.

For the reasons already given, we cannot subscribe to these opinions. The observations made on the smaller arteries are certainly important, and will lead, doubtless, to a more accurate investigation of the effects these produce on the capillaries. But we feel satisfied that the ultimate cause of inflammation must be sought in a perversion—that is, an increased attraction—of the vital forces

regulating nutrition in the parenchyma of organs, and not in any physical obstruction of the vessels from within. All three authors repudiate the notion of the colourless corpuscles being increased in number, and causing the stagnation,—a doctrine which, although for a time it may have been received in the London schools of medicine, has, from the moment of its production, been in Edinburgh repudiated as fallacious, and opposed to observation. The theory of Mr Wharton Jones, concerning the increased tendency to clinging together of the coloured corpuscles, is more consistent with fact, but, in our opinion, equally unsound. We still think the essential phenomenon of inflammation to be exudation,—a theory which none of the authors under review has thought proper to notice, but which we, nevertheless, regard as the most probable yet propounded, and as quite consistent with every known fact.

If, then, the first and essential changes in the inflammatory process are explicable on the principle of an abnormal nutrition, the subsequent results are undoubtedly so. But into the discussion of these we have no space to enter. In conclusion, we consider that the work of Mr Wharton Jones fully supports his high reputation, and that the facts he has accumulated will tend to advance our knowledge of the inflammatory process. The descriptions are rendered unusually clear by numerous wood-cuts and coloured plates. Of these last we have to remark, that although the outlines are correct, nothing can be more unnatural than the rose-pink tint communicated to the blood corpuscles and vessels. The lectures of Messrs Paget and Simon are elegantly written, in some places suggestive, and may be perused with advantage.

Gout: Its Causes, Cure, and Prevention. By an Original and most Successful Treatment, founded on the Organic Changes in the Human Solids, and on the Functions of the Skin; without the Use of Colchicum. By ABRAHAM TOULMIN, M.D., Member of the Royal College of Surgeons of England, &c. &c. London, 1850.

WE are always ready to look to London for information on the subject of gout. With us the disease is comparatively rare; whilst in the English metropolis,—with its numerous and wealthy aristocracy, many of them born to gout as well as more comfortable inheritances, and many more acquiring it by conquest; its "respectable tradesmen," pursy both corporeally and pecuniarily; its pampered menials, little addicted to work and wholly addicted to ale and porter,—the disease presents itself to the medical practitioner with a frequency quite unknown, we believe, in any other locality. At the same time, however, we suspect that the gouty diathesis is often made more of in London than it deserves, and that it is frequently alleged to be present, and to be exerting a modifying influence on other diseases, when it has nothing to do with

the matter. Making all allowance, however, for a little undue tendency on the part of our London brethren to see gout where it does not exist, we consider them to be better acquainted with the disease than the majority of practitioners elsewhere, and we have received our most valuable information, both as to its pathology and treatment, from London physicians.

It was in the hope of adding something fresh to our knowledge on this subject that we opened the book modestly lettered on the back "Toulmin on Gout;" but, alas! we were miserably disappointed. The title page rather startled us, and the very first paragraph of its preface—which, by the way, is not English grammar—showed us that the book is not intended so much for us as for that most important personage, the Public; and a further perusal demonstrated that it is essentially an advertisement of a hot-bath establishment kept by the author somewhere about Blackheath, and that it therefore falls within the category of that class of books, which it is our duty strongly to reprehend. We append the exordium of the preface.

"In submitting to the public what, I flatter myself, will be found to be a greatly improved method of treating gout, and its congeners, it is not with the view of superseding the use of medicine, but as an auxiliary to it, and to its application in a way which has of late years been almost, if not altogether, lost sight of. I allude to the didermic mode of treatment."—P. vii.

The first chapter relates to the chemical constituents of food, and is a kind of abstract of Dr Prout's "Bridgewater Treatise." There is, however, such a strange admixture of what is incorrect and unintelligible, that, even although the well-known albuminous, oleaginous, saccharine, and aqueous principles, are there paraded before us in large capitals, we doubt if Dr Prout himself, were he alive, would recognise his own doctrine. Here it looks like a child carried off by gypsies, and so disfigured that its own parents would repudiate it.

The following account of the albuminous principle will serve to illustrate the above remarks:—

"As the first or most important of these, stands what is called the Albuminous Principle, which is distinguished from the rest by possessing for its chief element or base a large proportion of nitrogen.

"It comprehends all those articles of diet most essential to animal nourishment, and includes vegetable gluten as well as animal flesh; viz., fibrin, as also albumen and gelatine—as the curd of milk, the white of egg, &c. These substances, although differing widely from each other in external circumstances, are essentially of an homogeneous character, for which reason Liebig comprehends them all (or rather the essential principle of each) under the generic term *protein*. They are destined to form the constituent principle of all the animal solids, and are chiefly derived from the animal kingdom."—Pp. 4, 5.

We frankly confess that we do not know what are the characteristic "external circumstances" of the white of an egg, nor did we know that animal flesh was homogeneous; but if it were ever so

perfectly homogeneous, we do not see why this should be a reason for Mulder—for it was not Liebig—using the term protein to express the organic proximate principle of which he considers the various azotised constituents of our food to be compounds or derivatives. What does Dr Toulmin understand by the word homogeneous?

Here is our author's version of Dr Prout's account of the saccharine principle:—

"The third and last is the Saccharine Principle, which includes a very large class of substances, of apparently dissimilar character, yet all capable of reduction to the same chemical principles: thus, all fruit and herbs not containing gluten, as well as sugar, starch, and gum, are included in one class of chemical equivalents, and are all resolvable into the same elements, namely, carbon and water, in various proportions. They are all also capable of easy conversion into one another—as starch into sugar, and sugar into vinegar, and are all chiefly derived from the vegetable kingdom, and hence may be called vegetable aliment.

"The starch, or amylaceous class of vegetables, seems to be more directly nutritious than any of the others, inasmuch as it may, for a time, be made the means of subsistence, although not of nourishment. From its having no proportion of nitrogen in its composition, it would seem incapable of entering into the structure of the muscular, or other albuminous tissues, and more probably, perhaps, when we consider its component particles (carbon and water), helps to supply the materials for oxygenation."—Pp. 6, 7.

We are sorry that we have no explanation vouchsafed to us of the meaning of sugar, starch, and gum, being included in one class of chemical equivalents; nor of what Dr Toulmin understands by a chemical equivalent. As to starch supplying materials for oxygenation, we presume it is meant as a very succinct account of the theory of elements of respiration; but we doubt very much if the profession, and still more, if the public, will be able to extract so much information from the sentence in question.

The following, which is the concluding paragraph of this chapter, has caused us no small uneasiness:—

"It is well to know the basic relations of food in general, as we shall then be the better able to judge of the value and suitability of each article of diet as a source of nourishment."—P. 10.

We are sorely grieved to think that we are not acquainted with the basic relations of food, especially as it is well to know them if we wish to improve our dietetical knowledge; and we are still more distressed to find that Dr Toulmin cruelly withholds from us all explanation regarding them.

The next chapter is on digestion and assimilation, and the next again on metamorphosis of tissue and vital heat,—both of which might as well be away, as they contain nothing, with the exception of one concluding sentence, which bears specially on gout; and after this we arrive at Chap. IV., and now begin really to grapple with our subject, this chapter being devoted to the proximate cause of gout. That we may do Dr Toulmin all justice, we extract the following *resumé* of his views on the pathology of the disease:—

"It has been stated that, in most disorders of an inflammatory type, uric acid is largely secreted; this is particularly the case in the gouty diathesis, but with this remarkable difference, that, whereas in other disorders of a similar character, it is found in combination with ammonia, *in gout it is united with soda.*

"This salt, the *urate of soda*, which assumes the appearance of a white chalky powder, is almost as insoluble as lithic acid itself, and is therefore very rarely discovered in the urine; but, on the contrary, is frequently found to be deposited in the joints and other parts in a *solid form;* and, indeed, those peculiar concretions which, in the gouty subject, are known by the name of *chalk stones*, are proved by chemical analysis to be composed of this substance.

"If this, then, be the fact, and of which there can be no doubt, the exciting cause and morbid phenomena of gout, may, I think, be easily explained. In the first place, as has been mentioned already (and will be more fully shown when we come to speak of the predisposing causes of the complaint), uric acid is always abundantly secreted in the gouty diathesis; but from some cause not yet understood, this salt, instead of combining, as it usually does, with the ammonia of the metamorphosed tissue, and thus by its solubility readily passing through the kidneys, unites with the soda of the blood, and thereby forms a salt very difficult of solution, indeed generally insoluble in the human fluids. Urate of soda, therefore, circulates in the system, suspended in the blood, and being a morbid or abnormal product of a highly irritating nature, causes all those uneasy feelings, and anomalous sensations, so well known to the gouty subject.

"As it increases in quantity, the system would naturally feel the importance of getting rid of a matter so injurious to its healthy action; and, not being able to expel it by either of the usual outlets, would adopt the only alternative left her, that of depositing it in some part of the body, *out of the way of her operations*, and farthest removed from the centre of vitality; and hence the extreme joints of the feet and hands are generally first affected."—Pp. 21-23.

Our readers will have no difficulty in recognising, in the essential part of this doctrine (the existence of urate of soda in the blood), the conclusions attained to by Dr Garrod, from a series of careful experiments. The public, however, though it may read Dr Toulmin's book, which is addressed to it, does not read the Medico-Chirurgical Transactions, which are addressed entirely to the profession; and finding, instead of Dr Garrod's observations being quoted, that Dr Toulmin puts this forth as what he thinks, will naturally suppose that it is by his penetration and reflection, and not by another man's labours, that this point in the pathology of gout has been made out. We transcribe the results of Dr Garrod's experiments, as published by him in 1848, that it may be seen how curiously they coincide with the thoughts of Dr Toulmin, as published by him in 1850:—

"1st, The blood in gout contains uric acid in the form of urate of soda, which salt can be obtained from it in a crystalline state.

"2d, The uric acid is diminished in the urine, immediately before the gouty paroxysm.

"3d, In patients subject to chronic gout, with tophaceous deposits, the uric acid is always present in the blood, and deficient in the urine, both absolutely and relatively to the other organic matters, and the chalk-like deposits appear to depend on an action in

and around the joints, vicarious of the 'uric-acid-excreting function of the kidney."—*Medico-Chirurg. Trans.*, XXXI.

It is only fair, however, to Dr Toulmin, to say, that though the essential part of his pathology is Dr Garrod's, there are some portions of it which are undeniably his own; such, for instance, is the averment, that the urate of soda is generally insoluble in the human fluids, and that it is suspended, not dissolved, in the blood. If this be the case, Dr Garrod was certainly wrong in looking for it in the serum, where he found it. It ought rather to have been sought for in the coagulum, in which this insoluble suspended matter would surely have been entangled; and we may remark, *par parenthèse*, that a fairer estimate of the proportion of morbid matters present in the blood, such as sugar, urea, &c., would be obtained, if the whole blood, and not the serum merely, were examined.

To Dr Toulmin, also, we award the exclusive merit of the hypothesis which accounts for the smaller joints of the fingers and toes being generally the first affected. Dr Garrod, it will be observed, contents himself with merely stating, that the chalk-like deposits in and around the joints are vicarious of the excretion of uric acid by the kidney. Dr Toulmin is not content with this—he goes a step farther, and explains why the vicarious action should take place in fingers and toes rather than elsewhere; and he obviously attaches no small importance to this information, for he gives it in italics. It seems that all this is the result of a certain natural feeling in the system,—which, we may remark *en passant*, is neuter in one sentence, but acquires the feminine gender two lines farther on. The system, then, whether she or it, has a natural feeling that she must get rid of urate of soda somehow, and she sends it to the toes or fingers, because they are "out of the way of her operations." The laws of the system, as thus explained by Dr Toulmin, appear to be very much after the model of part of the penal code of our country. As the Central Criminal Court, or High Court of Justiciary, banishes incorrigible evil-doers to the antipodes, out of the way of the operations of the metropolitan judicature, so the system banishes urate of soda—a notorious disturber of the constitution—to the toes and fingers, out of the way of her operations. We do not exactly know how far the operations of the system extend, therefore we are not entitled to say if the mere fact of urate of soda being deposited in any part of the body, proves it to be out of the way of the said operations. The cartilage of the ear, for instance, is a common seat of tophaceous deposit. This seems to us to be too near headquarters to be out of the way of "her operations." It may therefore, perhaps, not be exactly a penal settlement—a Norfolk Island—for urate of soda, like the great toe, but rather a Pentonville Prison, or Perth Penitentiary, where tophus is temporarily incarcerated, till it becomes so irreclaimably vicious, that nothing but transportation beyond the ancles is adequate treatment for it. It is new to us that the fingers and toes are out of the way of the operations of the

system—it is no less new, that they are "farthest removed" from the centre of vitality. This discovery must be cheering intelligence to the physiologists. So long as we can remember, they have always been struggling to determine something precise about vitality, and have hitherto been only moderately successful. Dr Toulmin seems to have come near to something like a geometrical certainty regarding it, for he has found out its centre. As it appears, from his statement, that the fingers and toes are the points farthest removed from this, we apprehend it must be somewhere nearly equidistant from both; and as this is a rather interesting subject, we have the pleasure of informing our readers that, according to a rough measurement, the centre of vitality must be very near the spinous process of the twelfth dorsal, or first lumbar, vertebra.

We have, however, something more in the way of the pathology of gout from Dr Toulmin:—

"But this is not all; it has been mentioned that the uric acid unites with soda instead of ammonia. This soda can only have been obtained from the blood, in which fluid it exists united with phosphorus (phosphate of soda) as an essential ingredient. If it be so abstracted, the blood ceases to be in a normal state, and loses in part its important property of absorbing carbonic acid. This alone admits a flood of light to the eye of the reflective practitioner, as to the cause of many of the symptoms of gout. The soda of the blood, also, in uniting with uric acid, parts from its phosphorus, which, in the form of phosphoric acid, must now be circulating free in the vital fluid, and how far this active agent may contribute towards the irritation existing in the system, may readily be conceived."—P. 24.

Whether it arises from our not being reflective practitioners, or whether from something organically wrong with our mind's eye, we do not know, but we must own that we remain quite blind as to the symptoms of gout which are traceable to the blood losing its power of absorbing carbonic acid; and notwithstanding that Dr Toulmin thinks it an easy mental operation, we have not been able to conceive how far free phosphoric acid circulating in the blood contributes to the irritation existing in the system. Orfila, in his experiments, found that when a strong solution of phosphoric acid was injected into the veins, it coagulated the blood, and caused death in two or three minutes; but when a dilute solution was thrown in, it produced no inconvenience at all. As we presume that, at the very worst, it is only in a very diluted state that phosphoric acid can be circulating in the blood in gout, we are led to conceive that it does not in the least degree contribute to the irritation existing in the system. But, in fact, the whole idea of free phosphoric acid in the blood is visionary. Dr Toulmin has been reading Liebig's Chemistry of Food, and has been mystified by the metaphysical sort of discussion—not very intelligible, we must say—in which the Baron indulges, as to the relation of carbonic acid to the phosphate of soda of the blood; and he forthwith takes it for granted that uric acid can set free phosphoric acid, and thus add another to the already existing elements of the pathology of gout.

When Dr Toulmin finds that uric acid has a greater affinity for soda than phosphoric acid, and when the blood is acid in gout, then he may have some grounds for this chemico-pathological doctrine.

We pass over a chapter on the predisposing causes of gout, and one on the functions of the skin, in which we learn that one of the functions of the skin is to excrete (we call attention to the italics, which are ours) "hydrogen, both *in a gaseous state* and in the form of water; as also united to sulphur, as sulphuretted hydrogen; nitrogen, with many of its compounds, and carbon, both *pure* and in the form of carbonic acid gas;" and then we arrive at Chapter VII., which introduces Dr Toulmin's new and successful treatment of gout.

This consists in putting the patient into the thermaclin, the nature and effects of which therapeutical Hellenism we shall allow Dr Toulmin to describe in his own words:—

"The thermaclin is simply a heated bed, air-tight, in which the patient is surrounded by an atmosphere hotter than his blood; and the temperature of which can be raised or lowered at pleasure. In some complaints, it may be used in its simple form, merely for the purpose of producing free diaphoresis; in others, the atmosphere is impregnated by medicinal substances, in a gaseous form, and affords a most valuable means of introducing into the system any medicine capable of volatilization."—P. 48.

We may observe, that among the substances which Dr Toulmin finds capable of volatilization are arnica and bryony. The chief use of the thermaclin, however, is not to introduce useful, but to get out noxious, matters,—Dr Toulmin's doctrine being, that as the urate of soda, the *materies morbi*, will not go out by the kidneys, and cannot get out by the bowels, the only alternative is to coax it out through the skin; and that he can get it out this way will appear from a fact in the history of one of his cases, which we shall not fail to notice in due time.

Our readers, however, must be informed how to use the thermaclin with complete advantage:—

"But we have not yet completed the process necessary in using the thermaclin, nor have yet shown the full analogy it bears to the Roman thermæ. We have brought the patient to a state in which his pores are all open, and in which exposure to the atmosphere would be sure to produce some one of the various effects of taking cold. There are three means available for obviating such effect:—first, anointing the body generally with some stimulating oleaginous liniment; secondly, letting him cool down gradually, but completely, in a warm atmosphere; and, thirdly, by placing him at once under the cold shower-bath.

"In chronic complaints, when it is desirable to repeat the process, or where the object is to withdraw from the system morbid matter, it is necessary to let the patient cool down gradually, for which purpose he should be transferred from the thermaclin to bed, when it is better to use the former in the evening, as by this means he is not confined to the latter during the day: and in this way its use may be continued for months. In other cases, where it may not be desirable to repeat the process, the anointing the body will be a sufficient safeguard against cold.

"But in a great many cases, where there is no danger of repelling morbid matter, and where the object is to improve the general health by giving tonicity to the secerning system, by the absorption of all superfluous matter, the best way is, for the thermaclin to be given in the morning, and for the patient immediately to step from it under the shower-bath.

"This mode of bathing, I believe, I may claim the merit of having introduced into this country, although it is commonly used in Russia, and, as will be seen, was formerly practised at Rome."—Pp. 51, 52.

Dr Toulmin's originality, then, we apprehend, consists entirely in his occasionally making his patients step from the thermaclin into the shower bath. The thermaclin itself, except in respect of its being air-tight, the mechanism of which is not explained to us, is not at all new. It will be found described in no more inaccessible works than Pereira's Materia Medica and the Cyclopædia of Practical Medicine, under the name of Dr Gower's *Sudatorium*.

An acute attack requires three or four repetitions of the thermaclin. The shower bath is given after the last one in the morning; the patient is duly rubbed down with coarse towels, and dressed, and now comes the triumph:

"The process is now completed; the patient may expose himself to the open air with perfect safety, and it is desirable he should take some exercise. He may then set [*sic in orig.*] down to a hearty breakfast, to compensate for his previous abstinence."—P. 59.

For chronic gout, the thermaclin is the cure also; and to eradicate the gouty diathesis, the patient ought wholly to devote from six to eight or twelve months to the purpose, but the disease may be kept in check, though not eradicated, on somewhat easier terms. Here is what is promised them,—

"It must also be a great consolation to a large proportion of individuals suffering from gout, to those whose avocations will not permit them to devote sufficient time to the radical cure, equally with them who do not choose to be deprived of their accustomed luxuries: it must, I say, be a great satisfaction to such to be told, that by the regular use of the thermaclin periodically,—that is to say, for a week,—every three or four months, at the time they are free from an attack, they may, in most cases, keep the enemy at bay; and thus continue free from the complaint for years."—P. 64.

It may, perhaps, be a still greater consolation to many persons to know that, if they will only follow an appropriate regimen, they may have a fair chance of keeping down the complaint—nay, sometimes even of getting rid of it—without the thermaclin at all. We apprehend that still less time will be required if they will only live as they ought, whilst they are attending to their business, instead of spending a week every three or four months in Dr Toulmin's bathing establishment.

Passing over a chapter on the prevention of gout, we come to Dr Toulmin's cases; but our limits forbid us from entering on any further analysis of them than to say that they are eight in number; that cases I., II., III. and VI. were probably true cases of gout; that case IV. is one of flatulent colic, where the patient, besides

being thermaclined, had a warm stomachic aperient, followed by the very comfortable prescription of a wine glassful of brandy, with ginger and cajeput oil, by which he was felicitously relieved of much wind; case VII. is one of Bright's disease, proving fatal; and case VIII. is that of an "absurdly fat" gentleman, who was improved in appearance and health by having ten pounds sweated off him by the thermaclin. One half of the cases are thus not to the point at all.

We must, however, specially notice Case II. It is that of a gentleman, regarding whom we have the very particular information, that a maiden aunt left him L.18,000 (a capital case for sweating in more senses than one), that he was a single man, kept his cab, and belonged to a west-end club. These very accurate data will prepare our readers for finding this gentleman to be affected with gout; but we doubt if anything will prepare them for the effect which the thermaclin produced upon him. We shall, therefore, let them know it at once in Dr Toulmin's own words :—

"A remarkable circumstance occurred in this case: after the second thermaclin, the bath attendant called my attention to the peculiar appearance of the blanket on which the patient had lain, on which a quantity of a white chalky powder was deposited, marking where the perspirable fluid had fallen from the body. I desired him to collect it, and on examination it proved to be *urate of soda*."—P. 88.

This, to use an expression more common than elegant, is a clincher. Here is the very *corpus delicti* brought to light; or rather, here is urate of soda in *propria persona*,—a convict, returned from transportation to the toe, without a ticket of leave, captured by that active officer, Thermaclin of the detective force, and forthwith banished from the body for life. Had we confidence in Dr Toulmin's chemistry, we might be persuaded that this was really urate of soda; our present belief is, that it was epidermic scales. Is it to this case that he alludes, at p. 42, when he says that "a copious deposit of calcareous matter has been seen deposited on the bed-clothes after using the thermaclin"?

We have quoted so many of the advantages of the thermaclin, that we must, in fairness, take notice of its only difficulty :—

"The only difficulty with the practitioner will be, the time devoted to his patient whilst in the thermaclin. When used at the house of the patient, this is an important consideration; for his attention will be required throughout its operation. This, together with the greater facilities afforded in all respects by using it at the house of the practitioner himself, induced me originally to make arrangements for receiving patients into mine; by which means other important advantages are also obtained."—P. 103.

"Other important advantages"!—to the patient or the doctor?

In a chapter, headed "Remarks," we find some information which forms a curious appendix to a work on gout :—

"Before laying down my pen, I would avail myself of this opportunity to suggest, that other diseases besides gout may perhaps have their origin in some abnormal product of the metamorphosis of the albuminous tissues, aris-

ing probably from a want of oxygen, compared to the amount of nitrogenous matter requiring to be removed, and where this abnormal product cannot be excreted by the usual outlets. That thus salts are formed, richer in nitrogen than even uric acid, and, like it, perhaps, insoluble in the human fluids. And that in this way, and in like manner to urate of soda, nature deposits, so to speak, this morbid matter, on parts not necessary to the carrying on of the vital functions, as the glandular system and uterus; and thus rids the circulating fluids of that which otherwise might, perhaps, put a stop to the vital functions.

"In this way one may easily conceive that scirrhus and cancer have their origin in some still more mischievous compounds of urea than uric acid, probably cyanuric acid; and that, in this condensed form, the system is relieved of morbid matter, which otherwise might produce more fatal consequences."—Pp. 104, 105.

To illustrate this pathological doctrine, which seems to be founded upon a hypothetical suggestion thrown out by Dr Prout, there is given a comparative table of the constitution of urea, uric acid, and cyanuric acid; but it does not enlighten us as to how such a condensed form of mischief as scirrhus or cancer can be said to be a relieving of the system, unless, to be sure, it be that the mammæ and other parts obnoxious to cancer, are "out of the way of her operations;" nor do we gather any information as to what that may be, which can produce more fatal consequences than scirrhus and cancer. Dr Toulmin's concluding paragraph, however, relieves us of all anxiety on this point:—

"Be the fact, however, what it may, it is certain that by the careful use of the thermaclin, as an antiphlogistic remedy, in addition to medicine, an appropriate regimen, and an abstemious diet, which such cases demand, I have succeeded in removing many tumours, both in the uterus and breast, which I believe some practitioners would have pronounced as having a malignant, if not cancerous, character.

"Moreover, in many cases where, from the premonitory symptoms, and from the constitution of the individual, I had reason to suspect that such might, at a later period, be the case, and where I have had sufficient influence to persuade them to make use of these means before any mischief had actually taken place, I believe I have been successful in preventing it, and in thus conducting them safely through the most critical period of their lives."—Pp. 106, 107.

An antiphlogistic remedy, which can not only remove but prevent the formation of malignant tumours, needs no commendation of ours.

Our readers will, we think, now know enough of the character of this book. Perhaps we may be thought to have wasted ink and paper needlessly upon it; but why is it that so many books are published only to bring discredit on the profession, but because the medical press does not sufficiently condescend to show them up in their true colours? If a regular system of exposing books made up to advertise hot-bathing and other establishments kept by doctors were organised, something might be done towards keeping our medical literature from this contamination. But, in the want of this, we must look to another quarter,—our hopes are in

the Chancellor of the Exchequer and the next budget. Let us have a fixed duty—not a moderate one—on all such books. Why should the "Cordial Balm of Syriacum," and "Holloway's Pills," and the "Revalenta Arabica," &c., pay duty, merely because they come forth in the broad sheet of a newspaper, and the thermaclin escape it, because it appears in independent duodecimo? If this were done, the finance minister might not only put down undesirable books, but might, like the brandy and ginger in Case No. IV., do something towards "raising the wind."

Chambers' Educational Course—Chemistry. By GEORGE WILSON, M.D. Edinburgh: W. & R. Chambers.

A Manual of Qualitative Analysis. By ROBERT GALLOWAY, F.C.S., Lecturer on Chemistry, Queenwood College, Hampshire. London: Churchill.

THESE two little works are introductions,—the one to the study of general chemistry, the other to the methods of qualitative analysis—and both are excellent in their way.

Dr Wilson's work, which forms one of the volumes of Chambers' Educational Course, is intended as a simple introduction to chemistry for the youth of both sexes; but it deserves a higher place than the author claims for it, from the excellence of the spirit in which it is written. Most works of the class attempt to do no more than to give an account of the strange and striking phenomena of the science, and rarely venture to discuss its principles; but Dr Wilson has entered with considerable fulness, and in a remarkably clear, simple, and intelligible manner, into the general doctrines of chemistry, and has explained many matters which are generally, but as we believe erroneously, considered too abstruse for the popular student. We quote the short account given of isomerism and metamerism, as an illustration of the mode in which such subjects are treated:—

"At first sight, it seems impossible that two bodies should differ so much in character as gum and sugar do, and yet contain exactly the same proportion of the same elements. The fact admits, so far, at least, of an explanation, if we suppose the two bodies to contain their elements differently arranged, so that the function of the same constituent is different in each compound. The one body is thus like an anagram on the other; and as the letters in a word acquire a different significance according to their places in it, so the atoms in a compound will give it different characters according to their mode of arrangement. Thus *ape* and *pea* on the one hand, *ate*, *eat*, and *tea*, on the other, have the same alphabetical elements, but the difference in the disposal of the letters totally alters their verbal value. There is reason to believe that similarly, whilst the formula for gum may be written $C_{12}\ H_{11}\ O_{11}$ that of sugar is $C_{12}\ H_9\ O_9 + 2\ HO$.

"In the volatile oils, on the other hand, although we have in each 5 atoms of carbon for 4 of hydrogen, we have good grounds for inferring that each of the oils has a different equivalent; so that, though all have the same propor-

tion of the same elements, the combining quantity of each—that is, its atomic weight—is different. Thus one may contain C_5 H_4; another, C_{10} H_8; another, C_{20}, H_{16}; and so on. The following table of a series of isomeric bodies, all containing carbon and hydrogen in the proportion of one atom of the one element to one atom of the other, but all exhibiting multiples of the lowest conceivable numbers,—C, H, will illustrate this :—

	Equivalents.
Olefiant gas, ...	C_4 H_4
Gas from oil, ...	C_8 H_8
Naphthene, ...	C_{16} H_{16}
Cetene, ...	C_{32} H_{32}

"Isomeric bodies of the last class may be likened to words which, as compared with each other, contain the same letters arranged in the same way, but a different number of these letters. Thus in the words *ma, mama; tar, tartar; do, dodo;* we have in each couple the same syllable, but the second contains it twice as often as the first."

Mr Galloway's book is a very good manual for the student of analytical chemistry, in which the author has pointed out and explained all those little difficulties which are so puzzling to the beginner, and has substituted a very simple scheme of analysis for the complex and cumbrous systems employed in some of the other treatises on qualitative analysis. There are some points on which greater fulness would probably have been advantageous; these, however, may easily be extended in a future edition. The work, however, is a valuable addition to the treatises on the subject we already possess, and seems likely to find its way into our laboratories as a student's text-book.

Gregorii Conspectus Medicinæ Theoreticæ. Editio Undecima. Edinburgi. 1850. 12mo. Pp. 432.

A WORK in the Latin tongue, and by a Scotch professor, which, during the last sixty years, has gone through *ten* editions, must possess very peculiar merits to warrant the favour with which it has been received by the public. That it is a model of what medical Latinity should be, seems universally acknowledged; and competent judges, acquainted with its contents, can testify, that it is equally admirable as furnishing a compendious view of the state of medical science towards the close of the last century. Long after examining boards shall have elevated Cæsar and other Roman worthies to the honourable position which Gregory has so long held as the arbiter of fate in the Latin medical examinations of this country, the classical work of our great physician will maintain a handy place upon the shelf of every medical library. The volume before us has been recently revised by an excellent scholar, who has cleared the text of a number of errors which had crept into it, and now introduces to the public an "editio undecima, omni parte perfecta, prioribus emendatior."

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

THE MICROSCOPE AS A MEANS OF DIAGNOSIS.

THERE are still some teachers who consider the microscope as useless in the investigation of disease; but as a knowledge of its advantages extends, those who hold such opinion will diminish in number, and at length become extinct. I believe, on the contrary, that our acquaintance with the ultimate structure of the human body, in its healthy and diseased conditions, is now so advanced, and good microscopes are so common and cheap, as to warrant their introduction among the instruments of the medical practitioner. You must not suppose that an additional method of gaining information implies abandonment of those, the utility of which has stood the test of experience. Men must learn the every-day use of their senses; must know how to feel, hear, and see, in the same manner as they did before instruments were invented. We don't see the stars less clearly with our naked sight, because the telescope is necessary for an astronomer. Neither should a physician observe the symptoms of a disease less accurately because he examines the chest with a stethoscope, or a surgeon be less dexterous with the knife, because it is only by means of the microscope he can determine with exactitude the nature of a tumour. But it is unnecessary to enter into a lengthened argument to prove that the science and art of medicine are greatly indebted, in modern times, to the invention and proper application of ingenious instruments. The following examples will serve to convince you that the microscope is one of these:—

Example 1.—Some years ago I was summoned to see a Dispensary patient labouring under bronchitis, who was spitting florid blood. On examining the sputum with a microscope, I found that the coloured blood corpuscles were those of a bird. On my telling her she had mixed a bird's blood with the expectoration, her astonishment was unbounded, and she confessed that she had done so for the purpose of imposition.

Example 2.—A gentleman, for some years, had laboured under a variety of anomalous symptoms, referable to the head and digestive systems, under which he had become greatly reduced. He had consulted numerous practitioners, and visited innumerable watering places, in a vain search after health. On examining the urine with a microscope, I found it crowded with spermatozoa. He evidently laboured under spermatorrhœa, a disease which had never been suspected, but which was readily cured on the employment of an appropriate treatment.

Example 3.—A boy was brought to me with an eruption on the scalp, which was of so indefinite a character that its nature could not be determined. He had lately been elected to occupy a vacancy in one of our charitable educational establishments, and the question to decide was, whether the disease was or was not contagious. On examining the scab with a microscope, I readily discovered the *Achorion Schoenleini*, or fungus constituting true favus; and as this has been experimentally proved to be inoculable, I had no hesitation in preventing his admission to the school.

Example 4.—A child was supposed to be affected with worms, because it passed in abundance yellowish shreds, which, to the naked eye, closely resembled ascarides. All kinds of vermifuge remedies had been tried in vain. On examining the shreds with a microscope, I found them to consist of the undigested spiral vessels of plants; and they ceased to appear when the vegetable broth used as food was abandoned.

Example 5.—An eminent surgeon, in London, was treating a case, of what he considered to be pharyngeal abscess. Before opening it, however, he

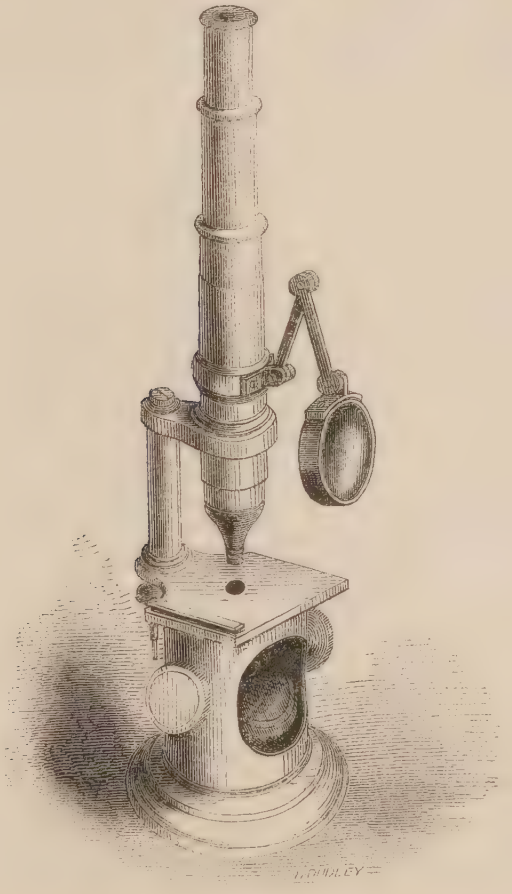
scraped off a little of the matter on its surface with his nail, and took it to Mr Quekett, who told me that, on examining it with a microscope, he found it to contain numerous cancer cells. The tumour was allowed to progress uninterruptedly; and on the death of the individual, some months afterwards, the bones at the base of the cranium were found to be enlarged, from a cancerous growth.

Examples of this kind could be readily multiplied. No doubt, mistakes will be made with this instrument in the hands of inexperienced persons, in the same manner as the use of the stethoscope, or of a knife, may lead to a false conclusion, or to an accident. But this, so far from being an argument opposed to their employment, only proves the necessity of becoming more skilful in their use. Certainly there is none which requires more expert management in itself, or more caution in drawing conclusions from its employment, than the microscope.

How to choose a Microscope.

It is not my intention to enter upon a description of the optical principles on which microscopes are constructed, although you will find a knowledge of these very useful. I shall suppose that you are desirous of obtaining an instrument that will answer all the purposes of the anatomist and physiologist, as well as afford you every possible assistance in the way of diagnosis as medical men. For this purpose, you should learn to distinguish what is necessary from what is unnecessary, in order that you may procure the former in as convenient a form, and at as moderate a cost, as possible.

A microscope may be divided into mechanical and optical parts. The former determine its general form and appearance. Of the numerous models which have been invented, the one here figured, exactly one-fourth its real size, appears to me the most useful for all the purposes of the physiologist and medical practitioner. The body consists of a telescope tube, eight inches in length, held by a split tube, three inches long. It may be elevated and



depressed with great readiness by a cork-screw movement, communicated to it by the hand, and this constitutes the coarse adjustment. It is attached to a cross bar and pillar, at the lower portion of which last, very conveniently placed for the hand of the observer, is the fine adjustment. The stage is three inches broad, and two and a-half inches deep, strong and solid, with a circular diaphragm below it. The base of the instrument is heavily loaded with lead to give it the necessary steadiness.

This form of microscope possesses all the essential qualities required in such an instrument. These are,—1st, Steadiness; 2d, Easy power of adjustment; 3d, Facility for observation and demonstration; and 4th, Portability.

1. *Steadiness.*—It must be evident that if the stage of the microscope possesses any sensible vibration, minute objects, when magnified highly, so far from being stationary, may be thrown altogether out of the field of view. Nothing contributes more to the comfort of an observer than this quality of a microscope, and great pains have been taken to produce it. In the large London instruments this end has been admirably attained, but at so much cost and increase of bulk as to render it almost useless. In the small model I have recommended, all the steadiness required is present in the most convenient form.

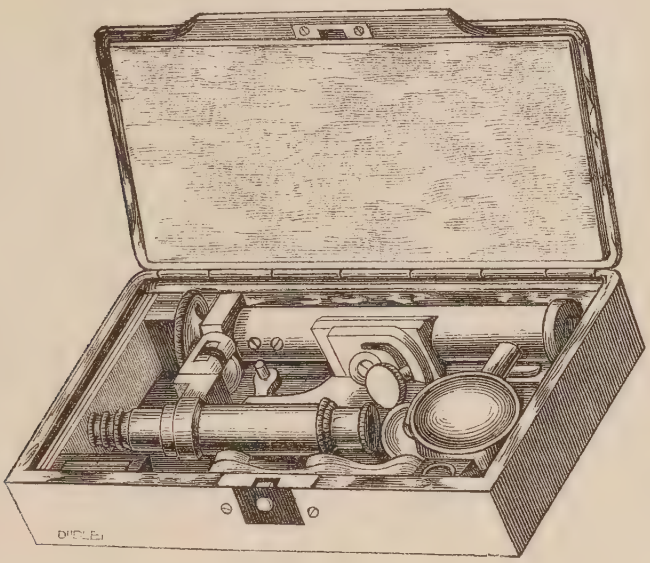
2. *Easy Power of Adjustment.*—It is a matter of great importance to those who use the instrument much, and work with it for hours together, that the adjustments should work easily and rapidly, and be placed in convenient situations. Nothing can be more commodious than the manner in which these ends are arrived at in the model figured. By insertion of the body of the instrument within a split tube, you may, by a spiral movement, elevate and depress it with the greatest rapidity, and even remove it altogether if necessary. The necessity of continually turning the large screws affixed to most microscopes, becomes fatiguing in the extreme. Then the fine adjustment, placed conveniently behind the microscope, near the hand which rests on the table, is in the very best position; whereas, in some London instruments, it is placed on the top of the pillar, so that you must raise your hand and arm every time it is touched. In other London instruments, it is placed in front of the body, so that you must stretch out the arm and twist the wrist to get at it. No one could work long with so inconvenient a contrivance.

3. *Facility for Observation and Demonstration.*—For facility of observation and demonstration it is necessary that the instrument should be of a convenient height, and that the stage on which the objects are placed should be easily accessible. Here, again, nothing can be more commodious than the microscope I have recommended, for, when it is placed on a table, its height is almost on a level with the eye, and we can look through it for hours without the slightest fatigue. On the other hand, the stage is elevated, just so much as enables the two hands, resting on their external edges, to manipulate with facility all kinds of objects placed upon it. The large London instruments are so high, as to render it necessary to stand up to see through them. To obviate this disadvantage, a movement is given to the body, by which it can be depressed to any angle. But this movement renders the stage oblique, and removes it to a distance, where it becomes very inconvenient to manipulate on its surface. To obviate this difficulty, the stage itself has been rendered moveable in various ways by different screws, so that in this way complexity has been added to complexity, until a mass of brass work and screws is accumulated, to the advantage of the optician, but to the perplexity and fatigue of the observer. But by no contrivance is it possible to avoid the aching arms which such a position of the stage invariably produces in those who work with such a cumbrous machine for any length of time.

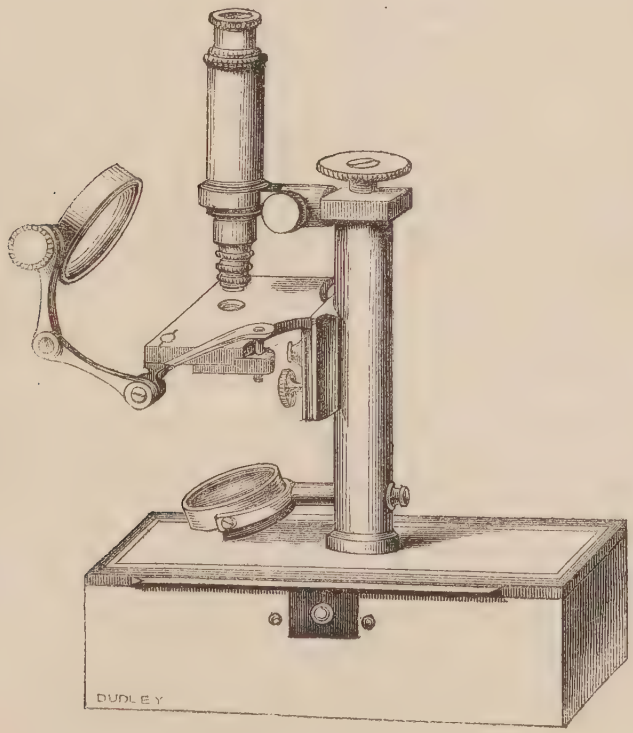
4. *Portability.*—This is a property which should by no means be overlooked in instruments that are intended more for utility than ornament. A medical man is often called upon to verify facts in various places; at his own house, at an hospital, at the bed-side of his patient, or at a private post-mortem examination. It is under such circumstances that the value of portability is

recognised. The large London instruments require an equipage or a porter to transport them from place to place; even the putting them in and out of the large boxes or cabinets that are built around them, is a matter of labour. In short, notwithstanding the splendour of the screws, the glittering of the brass, and the fine workmanship, there can be little doubt that, on the whole, they are very clumsy affairs.

There are many occasions on which a medical man may find it useful to carry a microscope with him, especially in the case of post-mortem examinations. Many attempts have been made to construct a pocket-microscope; and



for the purposes above alluded to, I myself planned one some years ago, which, with its case, resembled a small pocket telescope. Dr Gruby of Paris, however, has constructed the most ingenious instrument of this kind, which possesses most of the properties we have enumerated, and will be found very use-



ful for those accustomed to microscopic manipulation. It is contained in a case, the size of an ordinary snuff-box, and possesses all the conveniences of the larger instruments, with various lenses, a micrometer, slips of glass, needle, knife, and forceps, in that small compass. The prefixed wood-cuts, exactly one-half the real size, will give an idea of this ingenious microscope ; for a more minute description of which I refer you to the "Monthly Journal" for December 1846.

There is a general feeling among the public that the larger a microscope is, the more it must magnify ; but I need not tell you this is error. A very imposing mass of brasswork and mechanical complexity, is no guarantee that you will see objects better, or, what is of more consequence, become good observers. On the contrary, the more unwieldy the instrument, the less disposed will you be to use it. Besides, the habitual employment of artificial methods of moving about the object, as by the screws of a moveable stage, will prevent your acquiring that dexterous use of your fingers and accuracy of manipulation which is at all times so useful. Nothing, indeed, can be more amusing than to see a man twisting his screws, pushing his heavy awkward stage about, and laboriously wasting time to find a minute object which another can do in a moment, and without fatigue, by the simple use of his fingers. But perhaps you will consider the weightiest objection to the large instruments is the expense they necessitate,—the cost being necessarily in proportion to the amount of brass and mechanical labour employed upon them. If, then, you have to choose between a complex model, the price of which alone is fifteen guineas, and a simple one which you can obtain for two guineas, I strongly advise you, as a matter of real economy, to choose the latter. Indeed the former, to a practical histologist, is worthless.

(*To be continued.*)

INTRODUCTORY ADDRESS DELIVERED AT THE OPENING OF THE MEDICAL SCHOOL, SURGEONS' HALL, EDINBURGH, ON WEDNESDAY, NOVEMBER 6, 1850. BY GEORGE WILSON, M.D., F.R.S.E., LECTURER ON CHEMISTRY.

[WITH the exception of the notes, and a few trifling verbal alterations, this address is published exactly as it was delivered. I must ask those, accordingly, who read it, to consider it as a nearly improvised discourse, which I should not have consented to publish, had I had any prospect of leisure and opportunity to rewrite it. The subject is one with which I am scarcely entitled to meddle ; for only a medical man, experienced as a practitioner as well as a teacher, is qualified to do it justice. I may state, however, in extenuation of any assumption of the former character, that I spent the greater part of each day, for four years, in the Infirmary here, and witnessed its medical and surgical practice for that period. I can thus claim experience enough to entitle me to address beginners, especially as I have, for the last sixteen years, spent a large part of my time, first as student, and then as teacher, among students of medicine, both in Edinburgh and London.]

In rising to address you on this occasion, I have to crave your very special indulgence. It was not till last Friday night that I was requested, by my colleagues around me, to follow Dr Wood's example of last year, and open this session of the school by an introductory prelection. It has been impossible for me, in the short space of three days, and these greatly interrupted, to prepare an address worthy of this occasion. The practice, however, of delivering such addresses, is one so excellent, that, when I found that my refusal to appear here to-day would lead to the omission, for this year, of a general address altogether, I thought it better to trust to your forbearance and kindly judgment, on which I throw myself, than to stay away.

I have chosen for this address, a consideration of the temper or spirit in which the study of medicine should be entered on and prosecuted. It will allow me, without, I trust, seeming to be presumptuously dogmatic, or disposed to pronounce judgment *ex cathedrâ*, to offer you some remarks which may not be without service. You will not expect from a merely nominal member of the profession, like myself, who has learned more of its true nature as a patient than as a practitioner, any advices as to the mode in which, *after* you are surgeons or physicians, you should practise it; and, on the other hand, I shall be careful to abstain from giving undue importance to that branch of professional knowledge of which I am one of the teachers in this school. Into detailed references, indeed, to any of the branches of knowledge taught here, I shall not enter; for, although there are few subjects more tempting for an occasion like this, than an inquiry into the mutual dependence of the various sciences, and the mode in which they contribute to the extension of medicine, this question, or any similar one, is after all inadmissible for discussion, as those who are only on the threshold of their medical studies, or have advanced but a short way in prosecuting them, could not, with either interest or profit, follow an inquiry which instituted a comparison between all the branches of medical knowledge. Medicine, in truth, is the sum or complement of so many sciences, and so many arts, that were it necessary that every medical man should become a proficient in them, the profession must soon become extinct; for art is long, and life short, and every science is widening its horizon faster than it can be followed. And even if we had it in our power to arrest the sciences at their present stage of progress, and forbid that any more discoveries should be made, we should find what the students of a single science have already achieved more than sufficient to overtask us. Fortunately for us, something very far short of universal knowledge will avail for our professional requirements; nor is it necessary that I should consider how far short of omniscience we may safely be. This question has been practically decided for us, by the seniors of the profession; and the courses of study which they have laid down, for the guidance of students and their teachers, represent not the minimum, and still less the maximum, but the well considered medium amount of training in science and art which, according to the judgment of the profession, is needed to make a man a trustworthy medical practitioner. I shall not, therefore, say one word as to whether or not it is well, that a student of medicine is required to attend chemistry, surgery, midwifery, or the like. The probabilities are all in favour of the belief, that the course of study laid down for us is a wise and well-chosen one, since it is the scheme of medical education constructed for us by the wisest members of the profession; and in this spirit you should enter on it. It may not be the best, but at least it is good; and its laws are not those of the Medes and Persians, which cannot be changed. It has proved its ability to train up to efficiency and eminence, hundreds of medical practitioners, and that ability it still possesses. With this we may all be content. There are few follies greater than that common one, of refusing what is good, because it is not the best. There is a well-known class of painters, who are constantly changing their studios, seeking for a good light. I have known chemists who were all their lifetime in search of a *better* balance; anatomists, who were constantly exchanging their microscopes for new ones; and doctors, who could never discover a stethoscope that exactly fitted their ears,—whilst other men were painting, and analysing, and microscopising, and stethoscopising to purpose, with the instruments they could not use.

Our course of study is, in like manner, an organon or instrument for advancing us in medical knowledge. It is excellent for the purpose, and that may surely suffice us, without querulously asking, Is it the best? I will add, however, that a student of medicine must have gone the whole round of prescribed study, before he can be in a condition to determine whether that course was a wise one, and he can do himself no service by doubting its wisdom

before he has tried it. The beginning of all knowledge is faith. You must trust the medical authorities and teachers. You may afterwards lose confidence in them, and be justified in your distrust; but the study of no profession can begin with scepticism. The spirit of docility is a spirit of belief, nor is it incompatible with due caution and a wise scepticism.

It thus appears, then, that you are required to learn certain things, and we are required to teach them; and I desire now to offer you some suggestions as to how you may profit most by their study. I would begin by observing, that to medicine, more I believe than to any other profession, belongs the peculiarity, that, from first to last, it teaches, side by side, science and art,—the theory of knowledge, and the application of knowledge. It teaches you, for example, that there are great laws of physics, of pneumatics, of mechanics, of hydraulics, without which, the theory of the circulation of the blood, of the respiratory murmurs, of the movements of the joints, and much else important to the science of your profession cannot be understood; and at the same time it desires you, so far as circumstances will permit, to take into your hands the saw, the plane, and the chisel, and so to educate your fingers, and learn to handle tools; for you are to be surgeons or chirurgeons, literally, hand-workers. So, also, it teaches you a science of anatomy, full of elaborate details concerning form and structure; and it simultaneously gives you a scalpel, and bids you learn for yourselves an art of anatomy on the dead body. It requires of you a knowledge of the science of chemistry, with its atomic theories, electrical hypotheses, doctrines of compound radicals, and the like; and it also puts into your hands a specimen of blood, and some pieces of apparatus, and says, Tell me if there be sugar or urea there?

I need not multiply examples. It would be easy to do so; but it is unnecessary. I have purposely chosen those given from the earlier studies which engage the student, because it is needless to proclaim that surgery, practice of physic, and midwifery, involve, on the one hand, the recognition of very refined and subtle theories which constitute their science; and on the other profess, as arts founded on experience, to furnish the practitioner with methods and instruments of cure, which, within peculiar limits, may be called *certain;* for true science, and honest art, would both object to the term infallible.

The course of study, then, through which you will pass, in obeying the requirements of any of our surgical or medical examining bodies (which differ from each other as to what they demand, only so far as comparatively unimportant details are concerned), is one which may be pronounced excellent, in proportion as it exhibits the peculiarity referred to. A student of medicine has this advantage over the students of most, if not all, other professions, that he is as carefully trained to the law as to the fact, to the theory as to the practice, to the reason as to the rule, in every branch of his calling.

He undergoes, moreover, a training which, if wisely submitted to, secures a more equable and systematic education and cultivation of his nature, than the training of any other calling accomplishes. The study of the law will quicken certain intellectual faculties, more than the study of medicine will. The study of theology will cultivate the moral sympathies, in a way our studies cannot. The cultivation of the fine arts, music, painting, poetry, will refine the taste, and educate the æsthetical feeling or sense of beauty, to a degree unattainable by those who are only our votaries. The practice of the mechanical arts, will give the eye and the hand a precision, a delicacy, and a strength, which our exercises will not develop. Law, however, and theology, as well as art, whether æsthetical or economic, as studied professionally, cultivate but parts of a man. Medicine can claim to herself that she (though not, certainly in equal degree) educates simultaneously the intellect and the senses; the head, the heart, and the hand; nay, I may say, the whole body and the whole soul.[1]

[1] I am anxious to guard against seeming to recommend those who seek to educate themselves in the full sense of that word, to be content with the round of study

Let me offer you, then, a few remarks on the spirit in which this great circle of study should be traversed, especially in reference to the motives which should most influence us in traversing it. I prefer this to any attempt to lay down rules as to taking or not taking notes, rising early, or going to bed late; if we can secure the right spirit, these matters will arrange themselves.

I take for granted that the majority of those I address, are not amateur or dilettante students of medicine, but have taken it up as a vocation or calling, by following which, they are to save themselves from disobeying the command (including the penalty) of St Paul, that "he who does not work shall not eat."

This motive to professional study does not call for much reference. It can plead its own cause, and does so twice or thrice every day with each of us. It is as much moral as intellectual, and more physical perhaps than either. Its seat is in that important organ, which some of the ancient philosophers thought was the residence of the soul, namely, the Stomach, and it has two active prompters—Hunger and Thirst—who quicken its demands, and at short intervals keep always renewing them. The Germans, with their flexible expressive tongue, have devised a very happy phrase for this motive to work. They call a man's labours for sustenance, "brodwerke"—breadwork—the work or labour by which he procures his share of that daily bread, which we ask each day from our Father in Heaven. This motive to professional study, as it is perennially renewing itself, wears better, than perhaps any other which influences us. It is common to us with all other craftsmen and honest workers, and is based upon the equitable and divinely sanctioned principle, that "the labourer is worthy of his hire." Robert Southey the poet, who was neither a wealthy nor a sordid man, declares, in his curious and delightful book "The Doctor," that there are few more innocent ways of spending time than making money! There is a world of truth in the remark, if you will only understand it in the sense in which the genial poet intended it. He had no purpose of recommending doctors to think only of their pecuniary gains. A medical man who has an eye only to fees, is not considered an ornament to his profession. Avarice, however, or greed, is not the vice of our calling. A greater amount of gratuitous labour is exacted, and often quite thanklessly, from physicians and surgeons, than from any other class of professional men. If, therefore, you are bent upon a pecuniary recognition of all your services, you will find other professions more promising and more fruitful than ours. This mere need to work, which lies upon us all, is, however, to be highly valued, apart from the money or the bread it brings us. It anchors us down to our duties, by bonds which we cannot escape; it keeps us working at investigations, which

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which medicine prescribes. It is plainly desirable, for example, to take one case, that psychology, which can throw so much light on mental diseases, should be studied by members of our profession, who have opportunity, capacity, and relish for following that branch of knowledge, to a greater extent than is required by any medical examining board. It is still more desirable, that the morality of the profession should be based upon Christianity, which has its own great Text Book profitable for the members of all callings. Medicine does not completely educate a man, either intellectually or morally, not to say æsthetically. Nevertheless, as discussing so many of the physical sciences, besides communicating some information concerning psychology, and affording great exercise in the use of logic, as well as opportunity for acquiring skill in the practice of various arts, it simultaneously disciplines and informs the intellect and the external senses, to a greater extent than the *merely imperative* studies of any other profession do; whilst the solemnising effect of the scenes to which it summons the student, and the appeals to his integrity, sense of responsibility, benevolence, and sympathy, which it constantly makes, furnish a most wholesome moral atmosphere, in which his character may expand. Even a student, accordingly, who passes at once from the schoolroom to the hospital, and from the examining board to a field of practice, has undergone, though limited to the narrowest circle of imperative study, an amount of instruction and training, intellectual and moral, which the medium curriculum of no other calling can supply.

otherwise we should have foresworn, but in which our compulsory study reveals to us things more precious than food or raiment. And apart from this, it procures us the natural and salutary pleasure of feeling, that by sweat of hand or brain, and honest toil, we have secured our own independence, and provided for those dear to us. I do not know whether the botanists have ever tried to determine, what plant Adam cultivated when he first fell to work outside of Paradise ; but I think it must have been Dulcamara, *Bitter-Sweet.* To practise medicine, then, with a view to pecuniary recompense, may, as Southey taught, be a very innocent and most laudable thing. But this motive is probably never the only one at work, nor can it be trusted alone ; and, considering the early age at which so many begin the study of medicine in this country, it seems certain, that this, which in later life is so powerful a motive, is by no means the most prominent in the mind of a student, as it ought not assuredly to be. Of it I think I may justly say, that whilst the necessity for adopting a calling, may be a main motive to the selection and prosecution of medicine, it cannot be the one which most frequently recurs to the student, as from day to day he studies a particular branch of professional learning. Neither can it be the determining one to that profession, rather than to another. Something over and above the perception of the necessity of a profession, must have been felt. Some powerful secondary motive must have determined the choice of medicine, rather than law, or theology, a mercantile, an artist, a military, or a naval life.

I take for granted, accordingly, that those of you who are not students of medicine by compulsion, but have intelligently selected it as a profession, have been influenced more or less by the following, besides other less prominent motives. First, The expectation that it will prove a congenial occupation, as falling in with your tastes, and afford a welcome exercise to your faculties. Secondly, That it will enable you to gratify your desire of distinction, and to obtain an honourable name among men ; and, Thirdly, That it will gratify that love of truth, that desire of knowledge for its own sake, apart from its beauty or its uses, which is inherent, in larger or smaller proportion, in us all. The very unequal success with which different students prosecute medicine, or any other liberal profession, is so greatly dependent upon the extent to which they are consciously or unconsciously swayed by these motives, that I will offer you a word upon each.

As to the first of these impulses, it is perhaps the most important of all, in reference at least to your professional happiness, if not also to your success. The synonymes in our language for profession, viz., *vocation* or *calling*, imply that a man should feel some call or summons to the work he undertakes,—some inward prompting, that he has a relish and a fitness for the department of labour on which he enters. We have another word at the present day : we speak of a man's "mission." I do not know that it is better than the older terms ; but its employment shows how universal is the acknowledgment of the wisdom of suiting men to their callings. Few things, certainly, are more unfortunate for any one, than to discover too late that he has mistaken his calling or profession. A sense of duty may induce a man to do task-work, and habit can reconcile us to a great deal, and the receipt of fees may gild the whole over. Many a worthy doctor has probably gone through life happily, with no other motives than those just referred to. It is an immense addition, however, to a medical man's happiness and to his success, if he has an intellectual relish for the study of those laws and phenomena which medicine as a science considers, and for the performance of the various duties which, as an art, it demands. I am sure there never has been a zealous, not to say a great surgeon or physician, to whom his profession was not a source of intellectual pleasure. In this pleasure, the two chief elements probably are,—the sense of gratification which attends the intelligent apprehension of striking phenomena ; and the sense of power which accompanies the conviction, that we can interpret what we witness in spite of its intricacy, and can interfere to change the na-

ture and order of the phenomena occurring in a living body, so as to arrest disease and restore health.

Now, many among those I address, can as yet have no means of judging whether they have acted wisely in selecting medicine as a profession; neither will they be able to decide this for months, nay for years, to come. They can do a great deal, however, towards giving themselves a fair chance of liking their profession, so far as it occupies the intellect; and considering through how wide a range of subjects your medical education conducts you, it is likely that the greater number of those present, even if quite neutral or indifferent as yet, may have a relish for professional studies begotten or strengthened in them. In connection with this point, I would urge upon you, what is too much forgotten, that the branches of knowledge you are required to study are not only intended to store your minds with truths, which are essential to the discharge of your professional duties; but are likewise designed to exercise, to educate, to discipline, and to strengthen all the bodily and mental faculties which, as surgeons or physicians, you will be called upon to employ. This character of study, as an intellectual gymnastic, is often neglected; and certain of the branches of knowledge you are required to cultivate are disrelished, because they are looked upon only as storehouses of facts, but a small number of which can be recollected, or will be needed in after-life, whilst their value as disciplining and strengthening the faculties is overlooked. Let me give you an example. A student at the commencement of his medical career, has the inlets and apparatus of knowledge of all kinds less perfect than they afterwards become. The external senses, for example, are much less acute,—or rather, perhaps, I should say, less accurate—than they may be rendered, and than they ought to be, in those who learn so much by the eye, the ear, and the touch, as the surgeon and physician do. I have been struck by observing how specially defective in beginners the sense of smell is. Some time after the eye can readily distinguish shades of colour, the nose is still at fault in distinguishing odours. Now, chemistry can educate this organ, in a way other sciences cannot. The chemist has no other method of identifying many substances, but the differences in their odours, and educates his sense of smell to appreciate them even when they are faint, and very like each other; so that, like a trained pointer, he may never miss his game. When you remember that poisoning with opium and with prussic acid, is often discovered solely by their odour, you will have a direct example of the value of an educated olfactory organ to a medical man. But I refer to these examples only to impress upon you the gymnastic value of all your studies, and the desirableness of allowing each to exercise its full disciplining, as well as instructing, influence on your minds, if you wish to secure that most important and desirable result, that your daily professional labour shall be intellectually a labour of love. Nothing will more conduce to this, I am sure, than your discovering, as you proceed with your studies, that they are not only adding to your knowledge, but that they are rendering your intellects clearer, your memories stronger, your senses more acute, your fingers more expert; and all this they can effect, if justice be done them.

The second of the motives which I have supposed chiefly to influence you, is the love of fame or desire of distinction. There is probably no man uninfluenced by this motive, even among those whose career least brings them before the public; and it cannot but be strong in early life, and especially at the age when we first become conscious of our powers, and revel in the exercise of them. It is an impulse to action which many will disown—in some cases exactly in proportion to the extent to which they are secretly conscious of being influenced by it,—from the dread of exposing themselves to the charge of vanity; in others, because they prefer to stifle the promptings of ambition, and conceal, under the plea of modesty and contentment with the faithful discharge of their duties, the gratification of indifference and indolence. They are willing enough to be distinguished, provided they could, like Byron, awake some

morning and "find themselves famous;" but, since they have not been born great, and are too lazy to achieve greatness, they are minded to wait till, like Malvolio, "greatness is thrust upon them." Let us be above these follies. By nature we are both vain and ambitious, and our business is not to deny that we are so, but to seek to shape to useful ends and wise purposes that desire of distinction which we cannot repress.

This desire seems of two kinds, the one, a restless craving for distinction, founded on vanity; the other, a rational wish for the acknowledgment of talents or performances, founded on a consciousness of original power or acquired energy. Of the former, which is sister or cousin to "envy, hatred, malice, and all uncharitableness," it may be said, that it is the most unworthy motive that can influence a student,—he cannot be called a lover of science or art. A worker, influenced by it, does not consider what is noble, or hard to be overcome, or strive to estimate his own powers, and what they are fitted for; but knowing, as his earliest and chief motive, a keen longing for the praises of others, gratifies this at the expense of every nobler desire. This vanity readily and frequently becomes morbid, being haunted, or else alternating with, a secret sense or dread of inferiority, which begets jealousy, caprice, and fretfulness,—alien alike to manly independence, intellectual quiet, and moral repose. Further, as its reward is not in itself, but wholly derived from an external tribunal,—its goal being *praise*, not performance,—it is ever a vague and uncertain motive, which will keep a man at study only so long as it promises him distinction, and tempt him away to whatever else seems most likely to insure large, and above all speedy reward.

A rational and well-regulated desire of reputation, on the other hand, is truly a sense of justice felt subjectively, or in relation to one's self. It is a consciousness that the gifts of intellect which God has conferred on us, and the cultivation and extension of them, which time and education have brought, give us a power to work for our own good and that of our fellow-creatures, which entitles us to a higher place than the ungifted and uncultivated around us.

A rational ambition like this, recognises the great principle, that the most gifted and cultivated mind shall stand at the summit of the republic of science, of art, and of letters; and that lesser minds shall take their places according to their deserts, measured by this standard. And it differs from the vanity before spoken of in this, that its aim is not to *obtain* reward, but to *deserve* it. The vain man's wishes concern none but himself, and he takes praise whether he deserves it or no; whereas the honestly ambitious man is as willing to obey as to command, as anxious to see those more noble and more worthy than himself above him, as to see the less noble below him. This ambition can work ill to none, and must benefit all; since it is not, when we analyse it, a striving to be above this man or that man, but only an earnest desire to see every man in his own place, accompanied by an exhortation to each, but especially to ourselves, to aim at deserving the highest place. It is your duty, then, to train wisely not to deny, far less to repress, that desire of honourable distinction, which is, or ought to be, in all our hearts. We are the worst judges of our own merits, and probably always over-estimate our deserts. Our wisdom will be, in lowliness to esteem others better than ourselves; but so long as we feel, that we humbly and cheerfully acknowledge the superiority of others, and rejoice to dwell upon the greatness of the great dead, and still more (for that tries our sincerity further), on the greatness of the great living, we may safely, I am sure, aim at being great ourselves. Were it morally wrong for one man to wish to be more thought of than his brethren, it would be the duty of all of us sternly to repress it. So far, however, is it from being wrong, that we are to blame for not sufficiently encouraging each other to good and to great works. I say this in the name of the whole profession. Ours is an honourable and an ambitious calling. We have in all ages, and among all civilised peoples, claimed and received a high place in the esteem of our non-professional brethren. And woe to the day when we shall forfeit this; when

the members of other callings shall despise us, and say that we, who once were famous, have become a reproach and a byword. It is for you to save us from this. Our profession is as ancient as suffering is, and our pedigree and genealogy are older than any enrolled upon parchments, or recorded by heralds. We shall be needed also, and, unless we are to blame, will be honoured, so long as there are sufferers upon earth. And as thus we look back on all the forgotten past, and forward on all the unimaginable future, there lies upon us and upon you the deepest obligation to see that in these restless, progressive days, our profession is not outstripped by others, or the high place which has hitherto been granted to us exchanged for a lower.

The third motive referred to was, the Love of Truth for its own sake, apart from its beauty or its utility. I speak here of truth as apprehended intellectually—in other words, of knowledge or science. My object is not to urge you to avoid falsehood, or the perversion of moral truth; but to avoid false science, or the perversion of intellectual truth. This is a matter of more importance to successful study than many of you, perhaps, are aware. It is a much less easy thing to observe accurately than we are apt to imagine. In a court of justice you may any day hear a dozen witnesses reporting the same occurrence which they have all seen, but of which each gives a different account. Sir Walter Raleigh is said to have burned a large part of the MS. of his "History of the World," on finding one day, that of some trifling occurrence which had happened at his prison door in the Tower of London, he could not obtain two accounts that agreed. It seemed to him folly to pretend to write the history of the world, if the truth could not be ascertained concerning a single simple event that had transpired at his own door.

Lord Bacon occupies a large portion of his "Novum Organon" with an exposition of the "idols," as he names them, which hinder men from observing the truth, even where they desire and intend to be honest observers. The history of all the sciences illustrates this on the largest scale. The ancient astronomers held, that the earth stood still, whilst the sun went round it. The older anatomists taught, that the blood did not circulate. The chemists of the seventeenth and eighteenth centuries, made chemistry retrograde for a century, by misinterpreting what happens when a candle burns. Yet these astronomers had the same skies; these anatomists the same bodies; these chemists the same combustibles, as we have before us, and were men of as great intellect and capacity as their successors.

Nor is this an obsolete error, which, in these wise days of ours, we can count upon escaping. If you will only consider how widely quackeries of all kinds flourish among us, and with what avidity any rumour about the sea-serpent, about a brilliant gas costing nothing which can be made from water, about magnets which discover silver and gold, or about any other similar wonder, is received, you will feel that the intellectual atmosphere of the nineteenth century is certain to give its own tinge to truths seen through it, and keep us from observing them by that "Lumen Siccum," as Bacon called it,—that pure untinted light of reason, which allows truth to be seen without giving it a colour.

I press this upon you, because in your studies accurate observation is more than half the battle. The common belief of mankind seems to be, that it is difficult to reason well, but easy to observe well; and men think they have settled a dispute when they tell you, that they saw so and so with their own eyes, or heard it with their own ears. Yet these same eyes of theirs have, in the majority of cases, been looking through the spectacles of some prejudice, or prepossession, or expectation; some fear, or hope, or other blinding or colouring motive, of which they have taken no heed. They tell you truly enough what *they* have seen; but this may be very different from what they should have seen,—from what was actually before their eyes. I am inclined, indeed, to think that men reason better than they observe—at least that educated men do.

Whether this be the case or not, you must remember, in studying anatomy,

chemistry, physiology, surgery, and the like, that it is necessary to keep a watchful eye against the risk you run of mistaking the actual nature of what is before you. A chemist probably never analyses a substance without some prepossession as to what it will prove to be. An anatomist, looking through the microscope at some structure new to him, unconsciously expects it to have a certain molecular arrangement. A physician or surgeon examining a patient, does so with some theory, however slight, formed at the first glance of the sufferer, of the nature of his disease. In this way we often see what we wish to see, and do not see what we wish to leave unseen. This is but one cause of the deceit which we are constantly, though unconsciously, practising on ourselves. I cannot enumerate all the causes of our failure to discover truth, nor is it necessary. Thus much may be said,—We are often at fault, through the imperfection of our instruments of observation or experiment; our hands are awkward and we dissect ill, or weigh inaccurately; our eyes are uneducated to discriminate colours or forms; our ear is dull to the more delicate gradations of sound; our touch is not an erudite touch; we are misled by optical phenomena in our microscopes; by impurities in our chemical tests; by misconstruction of our instruments and apparatus. We are untrained to think; unaccustomed to reason logically, and to balance conflicting evidence. Even when phenomena are presented to us in all their fulness, we do not understand their significance.

Again, we are careless and indifferent observers. Many things escape us. Think how long it was before men discovered that water consisted of two gases, or that electricity moved a compass needle, or that steam was a motive power. We might have had steam engines and electric telegraphs, a printing press, vaccination, anæsthetics, the tying of arteries in aneurism, and much else, centuries ago, had men only opened their eyes and observed what was staring broadly in their faces. How many discoveries we miss every day, our successors will point out.

Further, we are hasty and impatient observers. We often witness but a part of a phenomenon, and then reason as if we had witnessed the whole. The famous tale of the chameleon and the travellers is a case in point. The false direction in which chemistry, and with it medicine, was turned by the chemists of the Phlogiston School, arose from this. They observed that a burning body evolves light and heat; but they did not wait to observe, that in so doing it becomes heavier. The history of science abounds in similar examples.

It is, then, no easy thing to be an honest observer; and it is an essential part of all education to beget in our minds a conviction of this, and to train us to be patient, earnest, full, and accurate in all our investigations. My present object is not to suggest the discipline by which a student may become a truthful investigator. On that point it is enough to say, that the course of study prescribed by the medical boards is such, that if faithfully and intelligently pursued, it will go a great way towards teaching those who pursue it how to become accurate observers and honest workers. What I am anxious to insist upon is this, that it would be most perilous to our profession if the worship of knowledge for its own sake were to die out amongst us. To nothing else can we trust for the full conservation of what truth our forefathers have handed down to us, or for the extension of what is in our own hands.

The other motives to labour which have been referred to, are narrow in their aim and limited in their influence, compared with this. If a man take up medicine as a profession solely to make bread by it, he will consider himself excused from all studies, except those which have a money-value attached to them, and perhaps will count it better to invent some infallible pill, or tincture, or ointment, than to make laborious searches after truth, which is said to lie at the bottom of a well.

If, again, he choose it because it pleasurably occupies his faculties, and interests him, he will carefully avoid all its distasteful departments, and limit himself to those which yield him gratification, so that he will narrow

himself to some small corner of the great territory which medicine claims and cultivates.

If, further, his chief object be to win himself a name, he will select those branches of professional study which are least difficult or most popular,—and these are exactly the departments of medicine which may be most safely left to themselves.

Nor will moral motives, such as the study of medicine with a view to enable one to succour the distressed, so as to become a gratuitous practitioner or medical missionary, avail.

All those motives are good, whether considered singly or together, but every one of them can find enough in the present state of medicine to satisfy it, and has in it no promise of extension of our calling, or need to care for that. We must seek to cultivate, in every medical student's mind, reverence for truth as truth, even though it should seem barren of all result; nay, should appear destined to overturn all that he has been accustomed to believe. Some few, the more the better, we may expect to make this their great aim, and to them we shall chiefly look for the elevation and improvement of medicine.

I would remind you, therefore, that you should despise none of the studies prescribed to you. You should honour all of them, The surgeon who knows the laws of mechanics, the different orders of levers, and the principles of action of the mechanical powers, will be, *cæteris paribus*, a better surgeon than he who does not. The physician who understands the conditions of a vacuum, the law of diffusion of gases, the nature of gaseous transpiration, the composition of inspired and expired air, of arterial and venous blood, will learn more from the revelations of the stethoscope than his fellow physician, to whom that instrument is only a hearing horn. All the sciences are indebted to each other, and often where they are least conscious of the obligation. I remember a very clever physician once interrupting me among my laboratory pupils, to ask me why I took up the time of students of medicine teaching them the properties of the metal copper? I ought, he said, "to be instructing them how to analyse tubercle." And yet, had my demonstration been on the very matter this doctor demanded, I must have begun with teaching that tubercle could not be analysed without the employment of oxide of copper. Some years ago, Dumas and Liebig were busy in their laboratories with certain researches. I can imagine a *practical* medical man who visited either whilst at work, saying to him,—Here is medicine admitting of the greatest improvement from the application of chemistry to it, and here are you, an acknowledged chief in that science, caring nothing about medicine, but busy with lime, and chlorine, and spirits of wine, only anxious to add another to those endless organic compounds, of which we have more already than we know the very names of, far less know anything about. And yet, out of these same unprofitable researches, proceeded the discovery of that chloroform, which our surgeons now rejoice to have in their hands.

We must not stop the natural philosopher, the botanist, the anatomist, the chemist, the physiologist, the metaphysician, or the student of any other science which can profit medicine, with the idle question, *Cui Bono?* of what good is this? Or the still more idle taunt—this is not practical. No great advance can be made in natural history by which medicine will not be a gainer. We have an interest, as medical men, in every wild beast show and zoological garden in the country. Vegetable and animal physiology cannot progress without greatly adding to the resources of organic chemistry. Chemistry cannot go forward without improving physiology and pathology, and adding to the therapeutic stores of the materia medica; or any one science make progress and not give assistance and an impulse to the rest.

In this you should have faith. You cannot all be accomplished anatomists, botanists, chemists, physiologists, natural philosophers, or psychologists, but you should despise none of those investigations; you should heartily wish well

to them all. Science, gentlemen, is another word for the works of God. To have faith in it is to have faith in Him. We should reverently believe that all knowledge is precious, worth knowing, worth encouraging. If we cannot each add to it, let at least Anathema Maranatha be pronounced on him who hinders its progress.

And now allow me to offer you a few words on the moral spirit in which medicine should be studied.

This is not the place nor the occasion for a homily on the subject. It is too important, however, to be altogether passed over. The study of medicine has been declared to have a strong tendency to make men materialists and atheists. If it really deserved this charge, it could not but be warily approached by those who had a choice of occupation. As for the declaration, that it makes men infidels, which once found expression in the assertion, that wherever three doctors were together, you might be certain there were two atheists, it was so extravagant a libel, that the wonder is it ever found the slightest currency. "The fool hath said in his heart, there is no God," but I cannot believe that the members of a *wise* profession like ours, can in any age or any country have been to a great extent, guilty of that folly. So far as regards materialism, there is thus much of truth in the declaration:—medicine necessarily brings before the student the physical apparatus and visible organs connected with sensational, emotional, and intellectual acts much more prominently than it does the purely mental or psychical agencies, which are, or are assumed to be, also at work. Those, therefore, who forget that medicine has no concern, as a practical art, to pronounce upon the true theory of mind, or soul, or spirit, and who do not observe that she deals only with so much of the problem as affects her own position as the minister of healing, are liable to be misled into the belief, that anatomy, physiology, and pathology have undertaken to decide the question between the materialist and the immaterialist. It may be well, therefore, to remind you, that these branches of medical science do not pretend to pass judgment on the matter in dispute, and that they would err if they did. If, therefore, you are interested in questions of psychology, you must not forget that you have but half the means for coming to a just conclusion concerning the nature of mind in your hands; and that *your* studies have a certain tendency to make you materialists, as the tendency of the preliminary studies of theologians and lawyers is to make them immaterialists or idealists. This tendency, I think, should be kept in view, and guarded against, for the exclusive study of the physical sciences is liable to beget a one-sided judgment on all questions where there is room for referring an effect entirely to physical or entirely to non-physical forces. The corrective must be found in studies of an opposite class; but I do not conceive it needful to enlarge on this, for ours is a practical, not a speculative calling; and it is but a very small section of mankind who will be found, in any circumstances, taking sides on such a point as the one we have been considering.

Every profession has some evil tendencies as well as some good. In ours the latter preponderate, but the former are important, and must not be overlooked. Allow me to refer to some of them very briefly.

The sciences which make up medicine are so much and so necessarily occupied with the exposition of secondary causes, that they are apt to make us forget that what we call laws and forces—physical, chemical, physiological—are but the modes of working of Him who is uncaused, and the author and director of all. I have heard wonder expressed by non-professional persons, that medical students should be so indifferent to the evidences of design which their studies so amply supply. In this, I think, they have done you, in some respects, considerable wrong. They have forgotten the engrossing nature of your occupations, and the necessity under which every diligent student lies, of mastering the mere facts of various sciences, which are by no means very easy of apprehension.

They have forgotten, also, that the accompaniments of a dissecting-room

are not exactly such as conduce most to moral meditations, like those in which Paley or the authors of our "Bridgewater Treatises" loved to indulge.

And, further, they have forgotten what, when I was a student, used very much to strike me,—viz., that in the wards of the hospital, all the goodly design of which so much was said, *seems* to fail. Instead of gazing eyes, hearing ears, moving joints, lungs gently breathing, hearts quietly pulsating, brains actively working, we go round an hospital and witness blindness, deafness, palsy, madness, every variety of torture, and the end of all—death. We want a chapter added to our "Bridgewater Treatises" to reconcile this with what they declare.

Some of you may have felt as I have felt, or will yet feel, the perplexity alluded to. Let me remind you, then, that even in disease there is exquisite evidence of design,—that it is not doctors who cure the sick body, but the body that cures itself. The surgeon will whip you off a limb fast enough, and so save torture and prevent death; but he does not pretend to make the bleeding vessels hermetically seal their mouths, the rough bone granulate into smoothness, the muscles form new attachments, and the skin close over all. Kindly nature does this. The diseased body proclaims to the last, that it was intended to be healthy, and is struggling against all odds to be so, and dies, as it were, facing Death with arms in its hands. I would therefore say, let not the striking moral lesson thus taught be overlooked, but rather seek out for it,—it will add interest to your daily duties, and invest them with a high moral meaning.[1]

There is another tendency of medical study to which I must refer, however shortly. Your studies compel you to deal with the human body, both dead and living, in a way which tends to blunt that delicacy which is a cardinal virtue in the surgeon and physician. It is impossible for us to treat the dead body as mere carrion, as we are compelled to do, to cut it and saw it, to macerate it, and put bits of it into glass bottles, to distil it in retorts, and burn it in crucibles, or to medicine it as we must when alive, and retain that feeling of respect or reverence for it which is natural to us, and which we feel so strongly when we gaze on the remains of those we have loved. You must guard, gentlemen, against giving way to this perversion of one of our noblest instincts. For your own sakes, you must fight against it. Apart from all other considerations, you owe it to yourselves, to keep your hearts from contamination, and to remember "Mundis omnia munda,"—to the pure, *all* things are pure.

And in connection with this, I have to warn you against everything that tends to harden your hearts, to lessen your sympathy with suffering, or, however slightly, to brutalise your natures. There would be something terrible in a cruel surgeon; in the spectacle of a man who delighted to witness and to inflict suffering. I cannot believe that there ever was a great surgeon who had not a kind heart; and I am sure that the coolness and case-hardening which every practitioner needs, are quite compatible with an affectionate,—though not, perhaps, an expressed—sympathy for an agonising patient. John Wesley, the founder of the Methodists, a shrewd observer of human nature, reports, that he knew a surgeon who gave this account of himself,—"I would not hurt a

[1] I greatly wish that some thoughtful physiologist would take up the subject referred to in the text, and supply, for the sake of the student of medicine, the blank which occurs in most, if not all, our natural theologies on this point. It is the dark, not the bright, side of the argument from design for an all-wise and all-merciful Creator, which demands elucidation at the hands of our natural theologians, but it seldom receives it. I may refer the reader, in the meanwhile, to a paper contributed to the "British Quarterly Review," for February 1848 (p. 204), where he will find some discussion of the question touched on in the text, in a paper entitled "Chemistry and Natural Theology." There are some admirable remarks on a kindred subject, in Professor Owen's work "On the Nature of Limbs."

fly, but I could scrape all the flesh off a man's bones if it were necessary." And in the same spirit a foreign surgeon, whose life I recently read, in recounting a formidable case which called for his interference, simply says, "I commended my soul to God, and cut deep." This is the spirit required.

I should be very sorry if you imagined that I thought students of medicine had acquired a character for cruelty. I believe the very opposite. You are hardly judged by those among whom you first practise. A "young doctor," which each of you will be after his first year, is in popular estimation, at least in Scotland, an ogre who tries experiments on his patients, does not "understand the trouble," in the majority of cases he is called to, and is the cause of many deaths. But for all this, you are credited with kindliness, and have even been commended for it.

In truth, I could wish that some one would write an essay on the Cruelty of Patients towards their Surgeons. Let a sick man have but some trifling ailment, some everyday malady, nevertheless he expects that his medical attendant shall condole with him, as a man marked out by misfortune. He does not consider that his surgeon has many other patients to condole with, has formidable operations to perform, and critical cases haunting his memory. Each patient appraises his own sweet flesh still more highly than Portia did that of Antonio; and when the surgeon comes, like Shylock with his knife, demanding his pound of flesh, he is refused not only that, but grudged even "the division of the twentieth part of one poor scruple," which Portia would more than have allowed him.

This is true; but you must bear with the exactions of the sick and suffering; you must compassionate and excuse them. You must aim at preserving a spirit of benevolence and mercy. It has always characterised medicine. It has increasingly tinctured it, as the modern practice of our lunatic asylums, more, perhaps, than anything else, illustrates. You must, therefore, be watchful against its decay; and, in particular, I would warn you against allowing your craving for distinction, and your longing to be prize essayists, to tempt you unnecessarily into cruel experiments on the lower animals. They are not at our mercy to torture as we please. Their lives are assuredly of much smaller account than ours. If by their suffering, human suffering may be prevented, we are justified in torturing them. But pain should be inflicted on them thoughtfully and considerately. We should scrupulously avoid all needless infliction or prolongation of agony, and be certain before we begin that we have a reasonable prospect of bettering mankind, and of giving more emphasis to the declaration, that ours is the healing art.

I close with one remark. There is, in addition to all that has been said, a triple peculiarity about the moral character of the medical man. Ours is a very responsible profession, which requires its youngest members to take charge of most difficult cases; and, therefore, gravity becomes our calling. But that gravity must not be allowed to become gloom, for we are to console and comfort, not to terrify the suffering. Therefore, cheerfulness becomes our calling. And lastly, ours is a perilous profession—more so, perhaps, than any other in times of peace; and, therefore, courage becomes our calling.

In short, a strong, clear head—a bold, kind heart—and a dexterous pair of hands, are what each of you must do his best to procure, and wisely to employ.

Part Fourth.

PERISCOPE.

MEDICINE.

ON SPASMS AND CONVULSIONS OF CHILDREN.—BY DR AUGUST SCHŒPF MEREI, PROFESSOR OF MEDICINE AT PESTH.—(*Continued from p.* 463.)

3. *General Convulsions.—Eclampsia.*

Infants of very tender age, especially such as have not completed their first year, are far more subject to convulsions than adult females,—and these convulsions very frequently terminate in death, while in the hysterical attacks of females this occurrence is extremely rare. The reason of the comparative frequency and danger of these attacks in infancy is the extreme susceptibility of the nervous system at this early age—a susceptibility certainly exceeding that of adult females, as theirs exceeds that of the male sex. The effect of this irritability during infancy is, that the production of the slightest abnormal impressions on the organism is apt to be followed by derangement of cerebro-spinal innervation, manifesting itself by spasmodic or convulsive movements.

Symptoms.—The *nosography* of infantile convulsions is detailed with sufficient distinctness, and it may be even said has been completed in many works of ancient date. Would it were so with the pathology and treatment of the affection! The convulsions may be slight and partial, or violent and universal,—there is infinite variety in the degree of violence and in the duration of the paroxysm. The attack is often preceded by abdominal spasm, or laryngeal spasm; but it not unfrequently makes its invasion without warning, either in a slight and partial degree, or in the more formidable form as eclampsia.

The slightest and most common partial convulsion—one, moreover, which is almost always the first to appear in an attack of general convulsions—manifests itself about the muscles of the mouth, as if the child were smiling, and in the muscles which move the eyeballs; there is next a slight twitch of the whole head, which is suddenly turned, or rather thrown, for an instant to either side, or directly backwards; or, instead of the movement of the head, there may be convulsive agitation of some of the extremities. Another slight and partial form has its seat about the mouth and throat. The infant makes rapid, involuntary, and very imperfect movements of deglutition. The same is observed in cases of hysterical females, and indicates a slight degree of *globus hystericus;* this form of convulsion in infants is obviously identical, and sometimes becomes so much aggravated as to pass into the disease described at p. 457, under the name of *Asthma Koppii,* but often ceases without such transition. These, then, are slight forms of convulsion, often observed in delicate children a few days or weeks after birth, and proportionately more rarely as this tender age is passed. They seem to cause no particular annoyance to the patient, and are not necessarily followed by eclampsia. Any febrile affection, and even slight gastric derangement, is sufficient to induce these convulsive movements in very young children. I have even observed them in cases in which no appreciable cause existed; and it seems to me that, soon after birth, the imperfect consolidation of the nervous system, whose paths of innervation have as yet been little used, may sufficiently account for the production of these irregular movements.

But the course of the disease does not always rest at this stage. These muscular movements, and the smile to all appearance so mild and innocent, often

deceive the most intelligent of mothers, who, while loading with caresses the object of their affection, are struck with sudden terror on seeing the child's eyes diverted from their axis, drawn alternately upwards and to either side, till little but the white of the eyeballs is visible, the mouth twisted in various directions, the jaw in violent motion; presently head, trunk, and extremities becoming constantly and convulsively agitated,—the hands hard closed,—the face red and swollen, covered with abundant perspiration, while frothy saliva gathers about the lips.

The duration of such a paroxysm is uncertain,—it may last for hours or even days, but in the latter case is almost invariably terminated by the death of the sufferer. In the majority of cases, however, after a few minutes the convulsive movements lose somewhat of their violence, and at last cease completely; but an infant, although saved for the moment, sometimes shows, after such an attack, symptoms of feebleness more or less considerable and lasting.

Varieties.—The fit, such as we have described above, is generally termed *eclampsia*. In its maximum degree of violence it is sometimes almost identical with epilepsy. There are, however, as we have remarked, various lesser degrees; such are likewise observed in the hysterical convulsions of females. We might, then, establish a great variety of degrees and forms of the disease under consideration; but I shall only insist upon the following, which will be found practically important: Eclampsia may be *idiopathic* and essentially nervous, or it may be *symptomatic*, and in this case almost always dependent upon one of the following causes:—Irregular dentition, gastric disorder, a paroxysm of fever, meningeal irritation, or hydrocephalus. It may either commence abruptly, as has been already said, or it may be preceded by spasms of the larynx (see p. 459), or by partial convulsions of the mouth, eyes, or extremities.

Abdominal and laryngeal spasm, as well as all forms and degrees of partial convulsion, almost never terminate fatally, without passing into eclampsia (see pp. 359 and 457).

The termination of such a fit of eclampsia as has been described is very seldom suddenly fatal; in a great number of instances the first fit, after lasting from five to twenty minutes, ceases, and is not repeated; or, if a second paroxysm takes place, it is milder, and neither injurious to life nor to the integrity of the functions. But if the convulsion lasts for several hours, or if, after the first attack, a second more violent and prolonged supervenes, death is to be dreaded. I have also seen a number of cases in which infants, after violent convulsive fits of this description, remained sufferers from more or less complete paralysis of one or more limbs, generally on one side, though I have known the palsy to affect both superior extremities, and in a lesser degree one of the lower. These paralyses have been very properly termed "essential" (Rilliet et Barthez)—*i. e.*, purely nervous, produced by a simple exhaustion of nervous energy in some part of the spinal system. I am far from affirming that all partial paralyses of infants have a purely nervous origin; but, undoubtedly, such is the pathology of most instances in which the palsy follows very violent convulsion fits of the description of eclampsia. I have had occasion to make post-mortem examinations in some such cases (the infants having died of other diseases), and to ascertain anatomically the absence of any inflammatory product, and of any material derangement in the brain or spinal cord, such as the eye could appreciate. Those paralyses which succeed convulsions are almost always accompanied with some degree of spasmodic contraction of the flexor muscles of the extremities affected, especially the fingers.

The frequency of convulsions has in my practice appeared most considerable in the first month of life; from this period it becomes gradually rarer up to the fifth month, and then again more common up to the period when the incisor teeth make their appearance. After this age, the disease again becomes rare.

Pathology.—Dissections have incontestibly established that, *in the great majority* of cases of convulsions terminating fatally, there is no cerebral or

spinal inflammation, nor even evidence of active vascular congestion. This I can state, on the authority of a large number of observations furnished alike by my hospital and private practice. In general, I have found nothing on examining the bodies of infants who have been attacked with convulsions, while their health in other respects seemed good, and succumbed after one or more paroxysms. Most of these patients were aged from one day to one month, or were infants feeble from the time of birth ; there were, however, a good many, aged from five to ten months, who died after repeated fits of eclampsia, associated with symptoms of abnormal dentition, such as uneasiness, fretfulness, and diarrhœa, without other appreciable cause. Many in whom the convulsions seemed symptomatic of some abdominal affection merely presented traces of intestinal lesions, or occasionally an enlarged condition of the liver. In some instances I have obtained anatomical evidence of active cerebral congestion,—well-marked capillary injection, without exudation. In those cases in which appearances of advanced inflammation, or well-marked exudations, have been observed, and in cases of simple hydrocephalus, in the course of which convulsions have come on, these spasms have not been the earliest symptom, nor have they usually been the immediate cause of death. An important practical deduction is, that the primary convulsions of infants are in the great majority of cases unconnected with inflammation. This fact is opposed to the therapeutical indications at present much in vogue among practitioners, and which lead to the employment of depleting and debilitating remedies in this disease.

Predisposition and Causes.—The most usual predisposing cause resides in the great nervous susceptibility of infants of tender age. The more feeble the vital powers the more excessive is this susceptibility, and the more apt is the normal condition of innervation to become deranged. Hence the youngest and most weakly infants are the most subject to eclampsia ; but, nevertheless, infants of very robust constitution do occasionally suffer. There is a special predisposing constitution, of which we have already treated under the head of laryngeal spasm,—a constitution observed in infants of from five to fifteen months, and characterised as follows:—Disproportional size of the brain, retardation of cranial ossification, inflated abdomen, feeble and lean extremities, and imperfect muscular development. Such children are particularly subject to convulsions, and all the more as it is just in such subjects that dentition is usually painful and irregular. Hereditary nervous disposition on the part of the mother also exercises its influence in rendering her offspring liable to eclampsia.

The occasional causes are various. While the spasm of the larynx is referable to a more special predisposition, and to a comparatively small number of occasional causes, eclampsia is induced in virtue of a far more general predisposition, and of exciting causes both numerous and of different nature. Thus the excessive nervous sensibility of an infant may *of itself* suffice to induce general convulsions. Cerebral inflammation, anemia, a paroxysm of fever, the efforts of dentition, gastric derangement, or in short anything which can directly or indirectly exert an influence on the cerebro-spinal innervation, may cause eclampsia. The presence of worms may do so likewise ; but the effect is almost always produced by the *ascaris lumbricoides*, and as this species is very rarely developed in the intestines of infants under three years of age, the convulsions of very young children are but seldom due to this cause.

The intimate nature of eclampsia we cannot understand till we shall become acquainted with the nature of the principle which operates and circulates in the nervous system. In the present state of physiology we must content ourselves with stating, that eclampsia is the expression of deranged cerebro-spinal innervation. I say "*cerebro*-spinal," because, although the abnormal muscular movements are undoubtedly immediately connected with the spinal system, yet the cerebral system is so linked with it and with the circulation of the nervous principle, that we cannot view the innervation as re-

stricted to the former. Besides, practical observations easily convince us, that some slight cerebral symptoms almost always precede the convulsions,—symptoms of undue activity or weakness of the head, expressions of distress, of bad humour, &c. It cannot be doubted that the vitiated innervation constituting eclampsia may depend upon either an excess or diminution of nervous vitality. In many of the cases which I have witnessed, infants of strong and even robust frame became, during the period of dentition, affected with general convulsions, while their nervous system seemed in a condition very different from that of the exhausted anemic infants who are the more common subjects of the disease. In this point of view, then, laryngeal spasm is more limited in its range, being always connected with excess of nervous sensibility, together with feeble vegetative powers, but seldom, on the one hand, with vital and nervous exhaustion, and, on the other, hardly ever with a state of constitutional vigour.

Diagnosis.—The differential diagnosis is in many cases difficult; and, nevertheless, it is important, for the purposes of treatment, to determine precisely whether the eclampsia which we witness is an idiopathic nervous affection, or symptomatic of some other disease. In the latter case we should seek to ascertain whether the cause resides in some phlogistic or hydrocephalic head affection, or in gastric disorder. The attacks of idiopathic nervous eclampsia are usually very sudden, and not preceded by premonitory symptoms. If the infant present the appearances of the nervous disposition,—if the skin and whole structure of the body be delicate,—if there be a certain vivacity of deportment and movement,—if, after the paroxysm, the child promptly regain its ordinary playful condition, there can be little doubt that the eclampsia is nervous and *idiopathic*. If there be repeated fits, separated by intervals of perfectly undisturbed health, the above diagnosis acquires absolute certainty. It is by no means necessary that such infants should be feeble and ill-made; in general, however, the brain or cranium is disproportionably large when compared with the face, which, like the extremities, has a delicate appearance. Nothing can be more pernicious than to treat such patients by applying leeches.

Symptomatic eclampsia is always preceded by signs of disease very various, like the affections which they indicate. The variety most apt to be confounded with the idiopathic, is that caused by abnormal dentition. It is generally said, that the painful efforts of dentition provoke active cerebral congestion, but this has not been shown by anatomical evidence to be a constant pathological fact. I have seen infants of delicate frame who, at the period when dentition was commencing, presented symptoms such as these: uneasiness, bad humour, abundant salivation, frequent cramming of the hands into the mouth, and signs of satisfaction when pressure or friction was applied to the gums, liquid stools, frequently even diarrhœa. In the midst of such symptoms they have been seized with eclampsia, which often proved fatal. Dissection in such cases has not even furnished evidence of that insignificant degree of hyperemia so easily found during the last moments of life. But I have seen other and very different cases of abnormal dentition. These were characterised by bad humour, appearance of extreme discomfort, swollen gums, and such tenderness that, far from bearing pressure, the child would not permit them to be touched, constipation, heaviness, and heat of the head. When, in such cases, death has followed a fit of eclampsia, post-mortem examination has almost always revealed well-marked signs of active cerebral congestion. Abnormal dentition, then, may have the character of *nervous* irritation or of *congestive* irritation, and consequently eclampsia from this cause may in its nature be purely irritative or congestive. Meningitic or hydrocephalic eclampsia is always preceded by the symptoms of disease of the meninges or substance of the brain. I have never seen an instance in which these grave diseases have been ushered in by eclampsia as a first symptom.

Eclampsia is often occasioned by an acute febrile paroxysm, either rheumatismal, typhoid, or, above all, intermittent. This is most common in very young infants, when the attack of fever is sudden and violent. I have seen

very violent convulsions occur during the cold stage of ague, and only cease when sweating began. More usually, however, the convulsions only manifest themselves during the cold stage, which in infants rarely exceeds an hour in duration. I recollect many such cases, which I have seen mistaken for meningitis, and treated by leeching, and in which, on the recurrence of the paroxysm of fever and convulsions on the third day, recourse was sometimes had to a repetition of the same depletory measures. Such mistakes must always be injurious, and may even be dangerous, for the next paroxysm will, after a debilitating treatment, be more violent, and the accompanying eclampsia more formidable. I have even seen deaths thus occasioned; while if, after the first appearance of fever, recourse is had to quinine, fever and convulsions are disposed of together. It is true that every acute and violent fever is accompanied with headach; and as the headachs of infancy are with difficulty distinguished from meningitis, it is natural for a practitioner, through fear and doubt, to direct his treatment against meningitis when the diagnosis is uncertain. And it is impossible to compress within the limits of this article all the details of symptoms on which the differential diagnosis, when possible, is based. In general, however, the following points will afford some information:—The intermittent fever (rarely observed before the sixth month) is sudden in its invasion, and its first symptoms consist in lassitude and somnolence; the child becomes very pale, yawns, its hands are cold, its nails blue, the respiration much accelerated, and the pulsations very frequent. After fifteen minutes or half an hour, the whole body turns very warm, and the sweating, which supervenes in two or three hours, by and by restores the child to its former condition. Eclampsia, when associated with the paroxysm, seldom lasts more than half an hour. If, at the time of his observation, ague be prevailing epidemically, the practitioner will have acquired a valuable addition to his means of diagnosis. In general, whatever be the form of the fever accompanied with eclampsia, we ought not at once to make our diagnosis, and fulfil the supposed indication of treatment, unless the infant is plethoric, or at least very robust. When the paroxysm is at its height, whether it is rheumatismal or intermittent, the heat of the extremities and forehead is much the same; in meningitis (an affection far rarer than the idiopathic form of eclampsia or that associated with fever) the temperature of the hands and feet is almost always less than that of the head, and the pulse is not so frequent.

Eclampsia, symptomatic of gastric derangement, is always recognisable by the ordinary signs of this latter affection. An error sometimes committed, however, is the confounding of the symptoms of idiopathic gastrosis with the gastric symptoms of hydrocephalus or encephalitis, such as vomiting and diarrhœa. In the latter case the abdomen is almost always soft and even depressed, while in cases of gastric disturbance an opposite condition is observed. Eclampsia is far more commonly associated with constipation than with diarrhœa. It is seldom caused by worms before the age of three years; frequently between the sixth and twelfth year. The paroxysms, then, are in general short and not dangerous.

Prognosis.—The prognosis of eclampsia during or after the first fit is very uncertain. The younger and more delicate the child the more considerable is the danger. At the commencement of dentition our prognosis should always be given with reserve. When, after the first fit, the child is well, or when there are perfect intervals between the paroxysms, even although these should be numerous, or if the general health of the child continues good, while neither the frequency nor violence of the fits becomes augmented, the prognosis may be more favourable. As for the prognosis of symptomatic eclampsia, it must be guided chiefly by the gravity of the concomitant disease. On this head we must refer the reader to the sections which treat of the causes and nature of eclampsia—(see p. 567).

Prophylaxis.—The prophylactic treatment must be conducted according to

the principles already laid down under the heads of abdominal and laryngeal spasms. Means must be taken, by a suitable hygienic regimen, assisted, if need be, by the use of remedies, to ensure the regular development of the body and vital forces.

Treatment.—The treatment of eclampsia is very difficult, and must vary exceedingly according to the various occasional causes and primary diseases of which the convulsions are symptomatic. The practitioner meets with a similar difficulty in treating the hysterical convulsions of females. During the paroxysm medicine can do but little. In general there should be no haste in interfering, lest the interference should be hurtful. Many practitioners no sooner have a case of convulsions presented to them than they direct the application of leeches—the natural consequence of the general doctrine, that this neurosis proceeds from, or is conjoined with, congestion of the brain. My practice, which has been sufficiently extensive, has convinced me, that in the great majority of cases detraction of blood is useless and pernicious. I have explained my views on this subject under the head of *pathology*. I now oppose the indiscriminate application of the theory of congestion, and the corresponding use of leeches as a remedy in convulsions, having myself followed this practice for several years, and observed disastrous effects to result.

During the fit, if the child is feeble and anemic, I order it to be placed in an inclined position, with the head downwards; if, on the contrary, it has an appearance of strength, the position of the head and trunk should be elevated. Gentle friction over the temples with aromatised vinegar, and the application of the same fluid to the nostrils, seem to exert sometimes a wholesome influence over the duration and violence of the fits. Enemata constitute a means of treatment which I never neglect. If the child be feeble and nervous, the lavement should be a warm infusion of chamomile flowers and valerian,—if there be constipation, the warm infusion of chamomile, mixed with oil and sugar, may be employed,—if there be flatulence, and more especially if the eclampsia has been preceded by abdominal spasms, we may substitute an infusion composed of chamomile and fennel seeds, with or without oil and sugar, according to the state of the bowels. If assured that there is no plethoric or congestive state of the encephalon, we may attempt to cut short the paroxysm by circular compression of the thighs. In weak infants I have obtained evident advantage from this practice, though by no means uniformly.

Leeches are *indicated* when the child is robust, and has before the fit shown symptoms of cerebral congestion—such as heat and heaviness of the head. They may also be used in the case of a strong infant, when the fit continues, notwitstanding the use of a lavement, followed by the discharge of liquid stools. If there be signs of congestion from abnormal dentition, no time should be lost in making scarifications of sufficient depth upon the free margins of the gums.

Warm aromatised baths are decidedly useful in the idiopathic eclampsia of feeble anemic infants; but in other conditions their effect is very equivocal,—and if a robust child shows signs of the *congestive* form of difficult dentition (p. 567), or of vascular reaction before the paroxysm, it cannot be placed in a bath without danger.

When the fit is over, the treatment must be regulated by the idiopathic or symptomatic nature of the disease,—due consideration being paid to the primary and exciting cause, if any such be detected. When the child is feeble and nervous, a tonic treatment and generous regimen are applicable, as we have shown when treating of laryngeal spasm. If no congestive state follows the fit, I always order a spoonful of infusion of chamomile with one or two drops of sp. melissæ and a drop of tincture of castoreum, and this dose I cause to be repeated every three hours. If the child be feeble, pale, and of lymphatic constitution, I prescribe a mixture of sp. melissæ and tinct. ferri. æther., and direct two to four drops to be given thrice a-day till such time as there is appearance of improvement in the constitution of the patient. Other stimulant and roborant remedies have not appeared to me to act so beneficially. Cod-

liver oil may also be necessary to give an impulse to retarded nutrition. Washing or affusion with cold water is a remedy very generally applicable and efficacious. The dry atmosphere of situations of moderate elevation usually exercises a beneficial influence upon infants prone to convulsions.

On the employment of calomel I may offer a few observations, for it is, I believe, far too commonly used in the treatment of children's diseases. As an *antiphlogistic*, it is not a remedy whose action is clear and demonstrated by the comparison and severe analysis of facts; and eclampsia, we have seen, is but seldom associated with a phlogistic diathesis—as a *purgative*, its action is too slow to be indicated in convulsions—and as an *alterative* (vague expression), its effects upon the nervous system are obscure. I have often employed it, and without evident advantage, in cases of eclampsia; and now no longer expose my patients to the risk of its pernicious influences upon the blood, the digestion, and the bones.

The subjects of trismus and tetanus of newly-born infants, and the chorea Sancti Viti, I propose to discuss in future communications. My mode of treating this last neurosis, I have already explained in a letter addressed some time ago to my distinguished colleague, Professor Trousseau of Paris. This treatment, which has failed in only six out of about one hundred and forty cases of chorea (many of them very severe) in which it has been applied, consists in the long-continued use of the ammoniacal sulphate of copper in large dose. When the remedy is ordered in the form of powder, infants, especially such as are of delicate complexion, can only tolerate small doses; and even these often cause gastralgia, vomiting, colic, or diarrhœa. The subjoined formula avoids these accidents, and enables us to persist in the use of this powerful remedy in large dose, and for a considerable time:—

℞. Aq. Melissæ, ℥iij.
Cupri Ammoniat., gr. viij ad x. S.
Adde Tinct. Opii Simp. vel Sydenhami, gtt. x. ad xii.
Syrup. Capill. Veneris, ℥j. M.
Cap. æger coch. minim, j. ad iv., sexies in dies.

I commence by giving a single teaspoonful six times a-day, and increase the total amount taken by two teaspoonful each day: thus, on the *second* day, I order two doses of two teaspoonfuls, and four of one teaspoonful; on the *third* day four doses of two teaspoonfuls, and only two of one; and so on steadily increasing the dose from day to day till the highest limit is reached, which the child can tolerate without unpleasant symptoms. I continue the maximum dose till a perfect cure is obtained; and this, even in the most formidable cases, is usually accomplished in from four to six weeks. If previous relapses have been observed, I continue to give the copper every second or third day for several weeks after the cure seems perfect. I have never observed any untoward consequences.

DIFFERENTIAL DIAGNOSIS OF DIFFERENT SPECIES OF GENERAL PARALYSIS, BY MEANS OF LOCAL GALVANISATION. BY M. BRIERRE DE BOISMONT.

The general paralysis of the insane, so well described by MM. Bayle and Calmeil, seemed to have acquired a definite place in the nosological scale, when the labours of MM. Baillarger and Lunier created some doubts on the subject, by establishing that general paralysis is an affection, independent of disorders of the intellect,—that it may exist alone,—and that, in general, it makes its invasion *before* the symptoms of insanity. While I myself admitted a two-fold division of the general paralysis of the insane, the first part of which was the more common and truly characteristic, including those cases in which the paralysis is associated with ambitious delirium, or tendency to the exaggeration of self-importance, while the second included a class of cases less numerous, and in which the palsy co-existed with simple dementia, I maintained with MM. Calmeil, Foville, Parchappe, Bayle, &c., that the general paralysis of

the insane, by its symptoms, course, nature, and the age and sex of its victims, was, nevertheless, to be regarded as a special disease.[1] Desirous of elucidating this question, I undertook a series of observations in conjunction with my friend, Dr Duchenne of Boulogne, and soon became convinced that the subject was involved in singular confusion. One of the first results obtained by our experiments in the local application of galvanism was, that there are two species of general paralysis, differing completely from each other in their nature and seat.

The first species of these progressive general paralyses without mental alienation, presents as its distinctive character a feebleness, diminution, or abolition of irritability, which is the better marked the longer the disease has existed. This alteration may commence in a single muscle or limb, it usually first manifests itself in the inferior extremities, from which it spreads to all parts of the body in succession, even attacking the tongue. In several cases, autopsies conducted with the utmost care, have revealed no lesion in the brain or spinal marrow, although the disease had been of old standing.

I cite the following observation in illustration of these facts. A lady had first lost the power of the left arm, then of the left leg, and successively of the right limbs; the fingers became contracted so that she had difficulty in grasping objects; progression was embarrassed, and could only be effected with the assistance of another person's arm. The palsy had reached the tongue, and the patient could only pronounce slowly and with hesitation the words dictated by the will. Sensation was unimpaired; intelligence perfect. The disease had existed for more than a year. The digestive functions were well performed; urine and fæces could be retained. The electric apparatus occasioned no contractions of the lower limbs. The tibialis anticus, peroneal and flexor muscles, remained unmoved when the current flowed through them. The same phenomenon was rather less distinct in the muscles of the arms; the muscles of the trunk contracted but feebly.

With the above observation we may associate the case of a patient, who died while under the care of M. Andral, in hospital. This man had for more than a year all the symptoms of general paralysis, without mental alienation. In his case irritability was completely abolished, though he still could execute certain movements. Consciousness was, throughout the whole course of the disease, intact. A careful autopsy, conducted under the superintendence of M. Andral, revealed no lesion; and microscopical examination, undertaken by M. Lebert, only detected a fatty degeneration in certain muscles of the thigh. The fibre of the muscles of the leg, in which irritability was extinct, exhibited no structural change.

From these and many similar observations it may be deduced, that there are cases of progressive general paralysis without alienation, characterised by feebleness, diminution or abolition of irritability. To such results it was interesting to oppose those furnished by the examination of insane patients, the subjects of general paralysis. M. Duchenne and myself, in September 1849, repeated our experiments upon the insane paralytics in our several establishments. Three individuals were experimented on. The degree of paralysis differed in each: the first had only the intermittent stammering; the second had reached the second period of the disease, and was considerably emaciated; the third had been paralysed for several years, could with difficulty stand on his legs, and could not answer questions. In all three, the irritability existed in a marked degree. We recommenced these experiments on the 15th November, at the Bicêtre, in presence of M. Delasiauve, the physician to the hospital, and of his pupils. The subjects of experiment were chosen at random among the oldest and most advanced cases: some of the patients had been confined to bed for several months. Two of the patients had become exceedingly emaciated, and

[1] See articles in "Gazette Médicale," and an essay on progressive general paralysis, in the forthcoming "Supplément du Dictionnaire des Dictionnaires de Médecine."

their lower extremities especially even atrophied; in all six examined, irritability was observed. Almost all these patients had lost command over the sphincters. It may then be advanced as a constant fact, that in case of general paralysis with mental alienation, muscular irritability is preserved. Instances may yet be observed in which general paralysis and irritability may both exist, without signs of mental alienation; but it must not be forgotten that in this disease there exists a triple order of symptoms, and hence that sensation and voluntary motion may be alone implicated, while the intellect may not be disturbed till long afterwards. M. Delasiauve communicated to us the remarkable case of an individual who remained two years in the Bicêtre, presenting the signs of general paralysis only, but in whom the characteristic symptoms of insanity then made their appearance within four and twenty hours. Finally, in the paralysis of the insane, progressive palsies may be connected with lesion of the spinal cord.

The above observations justify the conclusion, that there exist general paralyses, both with and without mental alienation. This fact has been already stated.

The existence of the general paralysis, without the characteristic form of mental alienation described by authors, is not to be regarded as a simple alteration of the power of motion, for in all the examples which have fallen under our observation, there was evidence of dementia, or of feeble memory.

Even were the power of motion alone affected, it should be recollected that derangements of intellect, sensation, and motion, may manifest themselves after an interval more or less prolonged. The absence of the exanthema does not imply a change in the nature of eruptive fevers.

As to the seat of general paralysis, we cannot admit that it may be always localised in the nervous centres. Our observations lead us to hold, that some cases are connected with lesions of the spinal cord,—others of the great sympathetic,—that the disease is in some instances peripheric,—in many not associated with lesion of the nervous centres,—in a certain number dependent upon disease of the encephalon. We have insisted upon these distinctions, in a letter addressed to M. Verga, editor of the "Gazzetta Medica Lombarda," and published in this journal.

It may then be considered as an established fact in medical science, that there exist two great classes of general palsies, in one of which (that of the insane), all degrees of irritability are preserved; while in the other (that which is unconnected with alienation), irritability becomes altered and enfeebled in proportion to the progress made by the functional disorder.

The general palsies of the insane may be subdivided into two varieties: the first, and most numerous, being those which affect individuals in the flower of their age, and in which the characteristic mental symptom is ambitious delirium,—exaggeration of self-importance; the second, and more limited, usually attacking those already advanced in years (though sometimes its victims are adults in the vigour of life), associated with symptoms of dementia, and specially with weakness and loss of memory.

The seat of general paralysis cannot be localised as has been heretofore attempted; it is a great functional disorder which may depend upon different lesions of all parts of the nervous system.

When sufficient materials are at our disposal, M. Duchenne and myself propose to publish a *memoire* on this important subject.—*Annales Médico-Psychologiques*, October, 1850. P. 603-7.

REMARKS ON THE RELATION OF THE DUMB-BELL CRYSTALS TO URIC ACID.

BY CHARLES FRICK, M.D.

The pathology of oxalate of lime, and its relation to uric acid, are now so clearly understood, and so universally recognised, that it is needless at this time to enter into a particular explanation in regard to its formation; and we shall,

therefore, take it for granted, that, in ascribing its presence in the urine to deficient oxidation of the uric acid, we are assuming the correct explanation. Under the microscope, the most common form that this salt presents, when it exists as a urinary deposit, is that of double pyramids united at their bases, or, as they are usually termed, octahedra; and these at first were the only recognised crystals of oxalate of lime. Dr Golding Bird, however, in his work on "Urinary Deposits," first called attention to some peculiar crystalline bodies having more or less the appearance of dumb-bells, which he described as oxalate of lime, and which all observers subsequent to him have included under the same head. For many reasons, we are led to doubt the correctness of this opinion: and having had, as we think, an opportunity of satisfying ourselves in regard to their composition, we deem it a point in pathology of sufficient importance to allow of some brief remarks. It is true that the question might easily be settled by chemical analysis, but the infrequency of the occurrence of these crystals, and the minute quantity in which they are found, render this test impossible. We think, then, that these dumb-bells are not oxalate of lime, but that their presence may be accounted for by the disintegration of crystals of uric acid.

If oxalic acid and lime be added together, the crystals formed under these circumstances are always double pyramids, although at times so small as to be scarcely perceptible; and, however long the deposit be allowed to remain, neither dumb-bells nor ovals are ever found to make their appearance. Whereas, if a deposit, consisting entirely of uric acid, be carefully washed, and clear water added, we may discover, in a certain number of cases, after the lapse of a few days, that dumb-bells are present, indicating, beyond a doubt, their formation from uric acid without the addition of lime. We were led to make this experiment from the following considerations:—We had frequently observed in a specimen of urine to which a small quantity of muriatic acid had been added, that, after standing a few hours, although crystals of uric acid only, with a few epithelium scales and a small quantity of mucus were present, yet, after the lapse of some days, these same specimens would occasionally be found to contain dumb-bells, while the uric acid had either entirely disappeared or presented an amorphous appearance. We were thus shown that some relation existed between the dumb-bell crystals and those of uric acid, and that their formation was in all probability subsequent to the elimination of the uric acid by the kidneys. We then determined to take pure uric acid, or at least as pure as it could be obtained from urine by repeated washing, and observe, under the microscope, the alterations that it underwent from day to day. By setting aside several specimens, and noting the changes that took place from day to day, we observed that, in the first place, the rectangular crystals had a tendency to become irregularly rounded off at their ends, afterwards to be fissured across near their centres in an oblique direction; and, finally, these two portions still adherent became rounded off in irregular steps towards the periphery of the figure; and this change we have remarked not once only, but on several different occasions. Should the crystals originally consist of rhomboids rounded off at their oblique angles, we may often observe that these first unite at their points—the two longitudinal axes being both in the same line—and then by the extreme points falling off, and these becoming by degrees irregularly rounded, they constitute a variety of the dumb-bell crystals in which the concavity of the sides is but slightly marked. Another, and perhaps the most common, form of this transition stage, is where one end of the base is fully formed, as is also the union between the two, but the opposite head, instead of being round, is elongated to a point. More commonly, they are very irregular in shape, but still with resemblance enough to a dumb-bell to be sufficiently characteristic. It is rare for more than one or two of these crystals, while actually in the transition stage, to present themselves at the same time under the field of the microscope. But within a few weeks past we have been fortunate enough to detect, in a specimen of uric acid put aside for this purpose, not only the dumb-bells in

their formative stage, but also several fully formed, as well as some crystals of uric acid almost unchanged.

Close attention will also enable us to observe, that the ovals usually described as separate figures from the dumb-bells, are in fact identical with them, the apparent difference being in reality due to the different planes in which they are seen. Some of these are transparent dumb-bells, with a very slight concavity at the sides surrounded by an oval, the interval between the two being filled up with a black space ; while others again appear to be dark ovals with a small transparent square set in the middle. If these are made to revolve on their short axis, it will be seen that one face presents an oval, while a section made at right angles to this is a true dumb-bell. Both these forms, if the deposit is of any size, are usually seen together, but occasionally one only presents itself.

We are unable to speak with certainty as to the original shape of the uric acid crystal which determines the subsequent formation of dumb-bells ; but are of the opinion that, in a majority of instances, the rectangle, with a height nearly equal to its base, more than any other form, is most liable to this transformation. They are seldom if ever observed at the time of micturition, and are in fact very rarely seen. Of the many hundred abnormal specimens of urine presented to us during the last few years for examination, we do not remember to have seen them but on four occasions, and in each one of these the urine had been voided twenty-four honrs previously.

In his description of these figures, Dr Bird states, that "they are produced in all probability by a prolific arrangement of minute acicular crystals" (of oxalate of lime). But this, we think, cannot be the case. There can be no lime entering into their composition, for we have shown that their formation takes place when there is none of this substance present; but at the same time we must acknowledge our inability to explain the exact process that goes on. It would seem, however, from the changes exhibited under the microscope, that this result is one of simple disintegration. All crystalline bodies made up of organic elements have a tendency, by exposure to air or water, to lose their clear distinct outline and sharp edges, and to become amorphous ; and, in all probability, crystals of uric acid of a peculiar shape have, under circumstances which we cannot yet explain, a tendency to break up and agglomerate in certain forms, more or less referable to dumb-bells.

In connection with this subject, we may mention here a change somewhat analogous that occurs in the crystals of the triple phosphate. The phosphate of magnesia and ammonia, or, as it is more commonly called, the triple phosphate, occurs in the urine under two different forms, one stellar and the other prismatic; and although both of these present an endless variety of crystallization, yet both may be included under one or the other of these heads. This difference has been heretofore explained by calling the one a monobasic, and the other a deutobasic salt, under the supposition that twice the quantity of ammonia was combined in the last in proportion to the other. Our friend Dr David Steuart, of this city, however, has lately called our attention to the fact that these crystals may be formed, one from another, by simple aggregation. He succeeded in depositing them from the same specimen in both forms, and also in crystalline shapes intermediate between the two. This he effected by making an acid solution of the phosphate of ammonia and magnesia, and then adding very cautiously a drop of ammonia. This precipitates the phosphates immediately at the surface, a portion of which fall to the bottom of the glass to be again redissolved. The upper stratum is, after a time, gently shaken, when more crystals fall with the same effect. There is a point, however, if the solution is not too acid, or sufficient ammonia has been added, when some of these crystals remain undissolved, and by carefully setting aside the tube at this stage, and allowing it to remain undisturbed for some time, we may detect, by the microscope, both the prismatic and stellar crystals with their intermediate stages. This experiment we have repeated, and are able to sub-

stantiate his opinion in regard to the mode in which these formations occur. Most of the stellar crystals are of two varieties: the first being composed of four rays, and the second of six. In the first we usually find, during this process, that, by aggregation of crystalline material, two prisms are formed which intersect each other at right angles; or else two of the rays which are in the same line become elongated, and material is gradually deposited in the interstices to make up a single perfect prism, the two long arms corresponding to the length, and the two short to the breadth of the crystal. In the second variety, where the figure has six rays, four of these become elongated in a similar manner, and the same deposition occurs as in the other; or else these rays are all joined together at their extremities, and the figure then filled up constitutes one variety of the prismatic crystal which is very frequently observed. These crystals, then, are not dependent upon the difference in proportion of ammonia in the two, but merely to the rapidity with which they are formed; nor is the stellar variety, as has been heretofore stated, indicative of a severer lesion than the other. We ourselves have never seen these stellæ already existing in urine at the time of emission, nor have we ever observed them to occur spontaneously.

The relation of these two forms of phosphate to one another seems to us a very interesting point in urinary pathology, and we are of the opinion that many of the crystals found in this secretion, heretofore considered as differing from one another in their chemical composition, will be found on a more accurate observation to be formed either by the aggregation or disintegration of primitive crystals.—*American Journal of Medical Sciences*, July 1850.

[Dr Frick's observations are of interest, so far as they show that uric acid, besides the many other forms which it exhibits, may also occasionally, by disintegration of its crystals, assume the form of a dumb-bell; but further than this it does not carry us. He totally fails to establish his proposition, that his dumb-bells are identical with those described by Golding Bird, and others. We are led, from the figure which Dr F. has given, and which we do not think it necessary to reproduce, to suspect that he never has seen the so-called dumb-bell crystals of oxalate of lime; for his uric acid dumb-bells do not at all resemble them. They are, in fact, too like dumb-bells; whereas Dr Bird's are not very like a dumb-bell at all, and are much more aptly described by the other comparison which he uses,—"or rather like two kidneys with their concavities opposed." Double reniform crystals would be a much better, though slightly longer, name for them.

Dr Frick says, that it is impossible to determine their nature chemically. Here he is quite wrong. We have done so under the microscope, and found them to present all the relations to re-agents which the other forms of oxalate of lime possess. We have not been so fortunate in our attempts to repeat Dr Frick's experiments; for we have not been able to see any of his dumb-bells of uric acid, even in specimens which had been kept in fluid for some months; and the fact that it requires some days to produce them, shows that they cannot be the same as Golding Bird's crystals; for these we have several times seen in urine which had been passed only a few hours. Further, they never occur alone; they always are accompanied by the more common octahedral crystals of oxalate,—that is, they occur only when the elements of which they are believed to consist are present in the urine.

We observe, from an essay recently published in the "Medical Gazette," that Dr Golding Bird no longer believes the dumb-bell crystals to consist of oxalate of lime. He remarks that the action of the dumb-bells upon polarised light is easily observed, while the octahedral crystals (well known to be oxalates) do not manifest any such action. Besides, no octahedral crystal with which we are acquainted depolarises light, hence he argues that it is very improbable that the dumb-bells can consist of a congeries of elongated or acicular octahedra. He adduces besides, chemical reasons in support of his present opinion, that the dumb-bells are a compound of *oxaluric* acid and lime.]

SURGERY.

LONDON PRACTICE OF SURGERY.

"Even if no others had ever been recorded, the following case would be in itself sufficient to refute Mr Syme's opinion, that there is no case of stricture so impermeable that he could not pass an instrument through it; for it must be evident, that when a stricture has been laid freely open from before backwards, almost to the bladder, and when it is then found impossible to get any instrument into the latter viscus, it would be impossible to do so from the mouth of the urethra; and, if a stronger term than impossible existed in our language, we might employ it for the latter act."—*London Medical Times*, Nov. 2, p. 460.

Having read so far, we shrewdly suspected that the operator must have got into a false passage, and slit it open, instead of the urethra. This suspicion was soon confirmed, for "on attempting to pass a small instrument into the bladder, it was found that, on reaching the membranous portion of the urethra, no force could push it further forwards through the stricture, for it instantly became engaged in a sinuous and unnatural channel, which passed along for some distance;" and was proved to be correct by the account of the operation: "Having passed into the urethra a sound grooved at the sides, so as to present in the perineum, Mr Gay made an incision down upon it, and with some difficulty divided the anterior portion of the hard gristly structure; posterior to this lay a mass of cartilaginous hardness around the urethra, with one or more false passages. So much of the operation being accomplished, Mr Gay attempted to pass an instrument into the bladder; but, though he had made a full, free opening in the front part of the urethra, and he now, by the aid of a catheter, easily recognisable in the wound, was enabled to follow up the urethra for some distance, slitting it up as he advanced, till he came at last so near the bladder, that he could easily have pushed a trocar into it, yet he could get no instrument into it by the natural channel, as this could not be traced; so that the operation had to be suspended at this stage, as it had lasted a considerable time, and it was feared that further exposure to the influence of chloroform, of which the patient had now inhaled a very considerable quantity, might, in a man of his shattered constitution, produce very serious effects. Besides, it was hoped, that, after the excitement had subsided, and the bleeding had been completely checked, an instrument might be passed through the whole of the urethra into the bladder; and this hope was realised, for the next morning Mr Robertson was able to introduce a small catheter, the orifice of the urethra being with some difficulty made out.

"For some days the case presented a fair prospect of success; the urethra was undergoing gradual dilatation, the most urgent symptoms were passing off, and he made water freely. But all at once, about the fifth day, without any appreciable cause, he began to sink; the pulse flagged, and with it all the phenomena of life. Active stimulation was resorted to, but without any good effect; he gradually sank, and died in about ten days after the operation."

We have here another instance of the disastrous effects which attend the old operation for stricture, and cannot refrain from expressing our surprise at the obstinacy with which our brethren in London adhere to it, more especially now when they have had recommended to them another which is perfectly safe and certain. Instead of accepting this proposal as a fair subject of trial, they have ridiculed and reviled it in every possible way, not declining the worthless testimony even of persons who, by their systems of advertising, and other disreputable proceedings, have placed themselves beyond the pale of professional respect. They have professed to practise it, with utter disregard of the principles upon which it is founded, and have not scorned to publish the results of this spurious experience as confirmatory of their objections. In short, they have

spurned no means which promised to aid them in the vain attempt to resist an improvement, merely because it did not emanate from one of their own fraternity.

But, while publishing from week to week the sad results of their *own* method, they must not be allowed to forget that, instead of resorting to a mode of treatment so discreditable to surgery, so painful—we are bound to suppose—to their own feelings, and so dangerous, or rather certainly destructive, to the patient, they have it in their power to send him, free of expense, to Edinburgh, with the certainty of either obtaining relief, or protecting themselves for the future from the repetition of an offer apparently not very agreeable.

The London reporter says—"However, notwithstanding what Mr Syme says, we believe it is pretty generally admitted that strictures are *constantly* met with through which no surgeon, however experienced, can pass an instrument, even though the water comes off regularly by this channel." And in illustration of this statement, we may quote from another report of the same surgeon's practice, which lately appeared :—"Two or three weeks ago a patient was admitted into the Royal Free Hospital, labouring under retention of urine. He was suffering extremely, and described himself as having laboured under stricture for years. He had drunk very freely on the preceding evening, and had passed no water since. Here, then, as in very many of these cases, a spasmodic stricture, induced by a sudden fit of debauchery, was superadded to a permanent stricture of long standing. After a long, continued, and careful attempt to pass the smallest catheter, Mr Jackson, the house-surgeon, was obliged to send for Mr Gay, who, after consultation with Mr T. H. Wakley, pronounced the stricture to be impermeable, and proceeded to puncture the bladder from the rectum. This was done with little difficulty, and with relief to the immediate symptoms.

"The patient was ordered doses of sulphate of magnesia, with tartar emetic and opium, every three hours, and hot fomentations over the bladder and to the perineum. He immediately fell asleep, and remained so until the evening ; the bowels were freely relieved, and on the first recurrence of desire to micturate, the patient passed his water in a tolerably good stream through the urethra. In a day or two he became an out-patient."—*Med. Times*, June 1850.

The grounds afforded by such statements for estimating the present state of London surgery, are deserving of attention from the Council of the College, who maintain, that any surgeon practising beyond the distance of five miles from the General Post-office is unworthy of representing his profession in England. As friends and well-wishers, looking on dispassionately, we beg to warn the Council, that before long, instead of persisting in restrictions so exclusive, so old-fashioned, and so unsuited to the present state of things, they will be glad to invite the co-operation of men who, in regard to extent of practice, professional emolument, and social position, are greatly superior to a large proportion of the metropolitan councillors.

Retention of urine from stricture is relieved so much more safely and satisfactorily by the catheter than by puncture of the bladder, that the latter measure has hitherto been regarded as a last resource, of which the employment was warranted only by absolute necessity. Mr Liston had such a horror of this operation that he would hardly condescend to teach it, and has thus expressed himself in regard to it :—

"Patients have occasionally lived after these operations ; but the chances in their favour are few, compared to those afforded by withdrawal of the fluid, properly and without lesion, through the natural passages. The operation of puncturing the bladder in any way was not once performed, in a series of years during which I filled the offices of assistant-surgeon and surgeon to the Royal Infirmary of Edinburgh ; and it has been performed but once—and that before I joined it—at the University College Hospital, since it was opened for the reception of patients. The cases of bad urinary disease at those institutions during these periods have by no means been few."

But it must not be forgotten, that all practitioners have not the requisite opportunities for acquiring and retaining the power of managing instruments so as to overcome the various kinds and degrees of resistance opposed to their introduction by contractions of the urethra; and that for those not so qualified, puncture of the bladder may afford the only chance of saving their patient's life. In his "Principles of Surgery," Mr Syme has, therefore, treated the operation with more respect:—"If the surgeon possesses the requisite tact for introducing instruments into the bladder through the urethra, and has the treatment of the case from its commencement, he will very rarely, perhaps never, be under the necessity of resorting to this puncture. But should he not be able to draw off the water by the catheter, either from his own want of dexterity, or from the existence of obstacles arising from mismanagement or previous organic alteration of the passage—as stricture, or enlargement of the prostate, complicated with a lacerated, softened, swelled, and bleeding state of the lining membrane, caused by forcible attempts to pass an instrument,—there can be no hesitation in having recourse to the operation. Puncture of the bladder, however performed, is always attended with more or less danger of urinous infiltration; but a doubtful remedy is better than none; and there are few states of disease more hopeless than complete retention of urine, permitted to follow its own course." Mr Syme has been engaged in hospital practice for more than twenty years, and has never required to puncture the bladder for stricture; but he does not deny the possibility of a case in which it would be necessary ever occurring. All that we contend for is, that the less frequently the bladder is punctured the better, and that hospital surgeons, instead of being satisfied with descending to the level of routine practice, should take advantage of their opportunities for raising the standard of surgical efficiency.

CASE OF POPLITEAL ANEURISM: TREATMENT BY COMPRESSION AND LIGATURE.

BY D. BERGIN, M.D., CORNWALL, C.W.

Israel Brien dit Du Rocher, æt. 27, consulted me on 11th March 1850,—a stout athletic man, a hewer. No hereditary predisposition to any disease; always enjoyed good health until about two years ago, at which time, after a fast walk of eighteen miles, he experienced a sharp pain in the right ham, extending up the thigh and down the leg and foot. On examining, he discovered a small swelling in the ham, very hard, and extremely painful when touched. He suffered dreadfully during four days, not being able to procure any relief. On the fifth day, notwithstanding his sufferings, he walked six miles to see a physician, who prescribed a stimulating liniment, which he applied three times a-day, and with some benefit, during seven or eight weeks, at the end of which time the pain had nearly ceased.

He continued in this way, without any apparent swelling in the ham, and with but occasional pain, until the month of January last, when he was again attacked with pain in the same situation, and which was so severe as to oblige him to give up hewing for a time. This relieved him for a short period, until he recommenced hewing, when the pain immediately returned, and with greater force than ever. He was of course obliged to relinquish work at once; in fact, his comrades were obliged to carry him back to his shanty, as he was unable, from the severity of the pain, to stand upon his limb. Two days after this he walked seven miles on his way homeward, but his sufferings became so great, as to oblige him to hire a horse to enable him to perform the remainder of his journey.

After his return home he employed frictions, lotions, &c., without any benefit, during three weeks. At the end of this time, the swelling and pain increased so rapidly within a few hours, that he sent for a physician, who, on his arrival, was obliged to slit open his pantaloons, in order to examine the swelling—to so great an extent had it proceeded within that short period. The physician prescribed a stimulating liniment; this he applied for a few days without any benefit. He then called in my friend Dr William Waggoner, of

Osnabruck, who immediately diagnosed his disease, aneurism, and desired him to consult some one of the medical men in Cornwall. Accordingly, a day or two afterwards (on 11th March), he visited me, and gave me the above history of his case.

Present state.—The tumour has been very rapidly increasing since January last, the time at which he returned home from the shanty. There is now a tremendous swelling, or rather pulsating tumour, occupying the whole of the lower third and a greater portion of the middle third of the right thigh ; the tumour is very hard and firm, more particularly on its inner and inferior aspect ; there is no discolouration ; there is great pulsation, which is quite evident not only to the touch, on pressing with the fingers, or on laying the hand over it, but also to the eye. Pulsation exists in every part of the tumour, and can be seen and felt in every direction of it. He says that he has always experienced pulsation in it since January. Pulsation now so great as almost to throw stethoscope off the tumour when applied ; it raises the ear very perceptibly. Aneurismal bruit quite distinct. I at once pronounced his case popliteal aneurism, and pointed out to him the necessity for immediate treatment. I explained to him that there were two modes of cure I might adopt,—viz., by compression first, and, in case of its failure, by the ligature. He expressed his entire willingness to submit to any plan of treatment I might direct. As he resided eighteen miles from this, he was obliged to return home to make some few preparations, before placing himself under my care here. On the following Friday he sent me word that he had returned to town.

Having read in your Journal Dr Bellingham's paper on "Aneurisms and their Cure by Compression," I determined to try its effect in this case, and in the event of its failure, to resort to the ligature. The size of the tumour, however, was such as not to render me very sanguine as to the success of pressure, Dr Bellingham's assertions to the contrary notwithstanding.

April 16, 8 P.M.—Accompanied by my friend, Dr Roderick M'Donald, who saw the case on Monday last, and by Dr Dickenson, I visited my patient, and after bandaging him from the toes upwards, applied two tourniquets over the femoral artery, one immediately above the tumour, and the other a short distance above the first, so as to alternate the pressure, and also to follow as closely as possible the method described by Dr Bellingham. I had forgotten to mention, that before applying the instruments, pressure upon the artery by the finger merely moderated the circulation in the tumour, but was not sufficient to arrest it completely.

On application of the tourniquet, the circulation through the tumour immediately ceased ; but the pain caused was so intense, as to oblige him, although a man of great courage and patience, to change the pressure from one instrument to the other every eight or ten minutes.

He says that he has slept very little for some time past ; his appetite is bad ; his tongue is furred and dark, though moist ; his bowels are costive ; his urine is natural in quantity and colour ; pulse 90, full and strong; pulsation in tumour synchronous with pulse at the wrist.

April 17th.—Passed a very bad night last night; pain from tourniquets almost intolerable ; did not sleep any ; tongue dried and furred ; cheeks flushed ; skin dry and hot ; bowels not yet opened. Ordered a black draught, and to remain perfectly quiet. Diet to consist entirely of fluids, or rather slops. Leg and thigh are cold and œdematous from the pressure. No bruit or pulsation discernible in tumour ; did not, however, loosen the tourniquet. 9 P.M.—Seems better than in the morning ; tongue cleaner ; skin not so harsh ; bowels not yet open. Venesection to about twenty ounces, and to relax tourniquets slightly, as they produce great pain, in order that he may procure sleep if possible.

April 18th.—Feels better this morning ; bowels were well opened during the night ; and he slept some this morning. Pain from tourniquet as great as ever; no bruit or pulsation in tumour ; swelling evidently increasing.

19th.—As yesterday; slept none since; to take acet. of morphia ¼ grain at bed time. Bowels were open again to-day. Complains very much of pains caused by tourniquet. On relaxing instruments, bruit and pulsation returned; on tightening them, they again ceased. Says that he cannot possibly suffer the pressure more than two or three days longer.

20th.—Slept well after taking the morphia; passed a comfortable night; feels much easier this morning. As yesterday, when I relieved the tourniquet, bruit and pulsation returned in as great force as ever. Compression does not seem to have been productive of the slightest benefit; on the contrary, the tumour is increasing in size. To repeat acet. of morphia again to-night.

21st.—Morphia had no effect; slept none; suffers terrible pain; cannot, evidently, suffer application of instruments much longer. To take 1 grain of morphia to-night. Tumour manifestly larger.

22d.—Slept about one hour last night; otherwise experienced but little relief from morphia; says that he cannot suffer compression much longer; begs for the operation; and says that death were preferable to his present sufferings.

23d.—Bowels not open since the 19th; to take a purgative of calomel, scammony, and colocynth; to repeat the morphia at bed time. Tumour still increasing. On relaxing tourniquet, bruit and pulsation as strong as ever; it is quite evident that the only effect produced by the compression has been to increase the man's sufferings; it has not meliorated his disease in the slightest; arrest of circulation through the limb is merely temporary,—the moment the instruments are relaxed the bruit returns. It is quite evident that compression must now be discontinued, else gangrene of the limb, or more probably suppuration and bursting of the sac, will soon take place. I now relaxed the tourniquets, being perfectly satisfied not only of their inutility, but of their absolute hurtfulness. He wishes me to operate immediately, but I have decided not to do so until Monday, the 25th, in order to give my confrères an opportunity of being present, and also to secure their assistance.

24th.—Fell asleep about an hour after I relaxed the tourniquets yesterday; slept well during the whole night; wishes me to operate at once, and not delay until to-morrow; seems excited and nervous. Purgative produced one evacuation only; to repeat it to-day, and if necessary to take 1¼ grains acet. morphia in the evening, if the pain continues very severe.

25th, 8 A.M.—Took the morphine, but without much benefit, last night; purgative acted well. Seems a little feverish this morning; to lose ℥xx. of blood; is very anxious for the operation. 10 A.M.—Assisted by my friends, Drs Roderick and Æneas M'Donald and Drs Dickinson and Allan, I took up the superficial femoral artery in the upper third of its course, immediately above where the sartorius and abductor muscles meet, and a short distance below the origin of the profunda. I applied two ligatures: the first, being of doubled and twisted silk, could not be tightened sufficiently, so that I was obliged to apply a second of fine single silk, which answered the purpose admirably. In order to avoid disturbance of the vessel in its sheath to any extent, and which I considered of very great moment, I placed it round the vessel immediately beside the first. To this too great disturbance of the vessel in the sheath is to be attributed, in a great measure, in my opinion, the very frequent failures of the older operations. I left both ends of the ligature hanging from the wound, and brought its edges together with adhesive plaster. Immediately upon the application of the second ligature the pulsation ceased in the tumour. He was in great pain after the operation, and I administered 1½ gr. morphine. 9 P.M.—Feels quite easy; morphine gave him instant relief from pain, though it failed to produce sleep; says he "feels too happy to sleep."

26th.—Was asleep when I paid my visit; his wife says that he has been free from pain during the night, but has slept none until this morning. Limb of the natural heat; no pulsation in the tumour; pulse 85, soft and full; had a slight bleeding from the nose, which lasted, however, only for a few minutes.

27th.—Feels perfectly well, he says; not the slightest pain in the limb;

bowels confined, however; to take a purgative of calomel and scammony. From this time he continued to do well; had no pain in the limb since the operation, and slept well at night. On the 9th day from the operation, or 2d April, the second or fine ligature came away, and without any hemorrhage; the greater part of the wound had healed, except immediately around the ligatures. Has had a slight epistaxis every day since the operation; thinks he is now well enough to return home; in fact, I have great difficulty in persuading him to remain until the remaining ligature shall come away, and the wound heal.

April 5th.—Was called suddenly about 2 P.M. to see him; the messenger said that he was bleeding to death. On my arrival I found that the blood had ceased; from the appearance of the bedding, &c., he must have lost very nearly two pints of blood. He appeared very much alarmed. On inquiry as to the cause of the hemorrhage, he told me, "that there had been, during the twenty-four hours previous, a small oozing of pus from around the ligature, and this drying had fastened his shirt and the ligature together; and that on attempting to get out of bed quickly, his shirt had dragged or jerked the ligature, and thus produced the hemorrhage. I left the tourniquet on the limb, so that if the hemorrhage should recur, he could arrest it by tightening the tourniquet, and thus allow time to send for assistance. I at the same time placed a small compress of lint over the wound, and brought the edges around the ligature more closely together. 10 P.M.—Has been no hemorrhage since my last visit. He is very pale, and seems now thoroughly frightened at the effects of his rashness; he is well persuaded of the necessity that exists for the most absolute rest and quiet being observed. I do not anticipate any further difficulty in this way.

6th.—Hemorrhage returned during the night, but on tightening the tourniquet it ceased at once; there was a slight oozing, however, through the edges of the wound. If the hemorrhage continue in any quantity, will be obliged to take up the external iliac. From the immense number of large glands in the groin, it would be almost impossible to take up the common femoral without wounding some of them, and which might occasion terrible trouble. To reopen the wound and apply another ligature would be out of the question. I intend, however, trying the effects of the tourniquet some time longer, and unless the pressure produce bad effects, will avoid taking up the iliac as long as possible.

7th.—There was a very slight hemorrhage during the day, not more than 1 oz., and which ceased of itself.

8th.—No hemorrhage since.

9th.—Continues to do well.

11th.—Feels slight pain in edges of the wound; suppuration evidently about to take place; wound emits a slight odour.

16th.—Removed dressing, as the wound smells badly, and suppuration has evidently taken place. The ligature came away with the dressings, and entire; the noose was quite distinct. From this time forward he improved rapidly; by the 30th the wound had entirely healed.

The tumour has now (May 4th) almost entirely disappeared; you can still, however, discover a slight hardness between the hamstrings, and I have this day given him permission to return home. He can now straighten his leg without any difficulty, and can walk well and firmly with the aid of a stick.—*British American Med. and Phys. Journal*, September 1850.

[The "Dublin Medical Press" has noticed this case, and labours to convince its readers that it furnishes no argument against the use of pressure "*more Hibernorum.*"]

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

CONGENITAL CRANIOMALACIA OR CRANIOLABIS. BY DR GŒDECHENS, OF HAMBURG.

Elsæsser, who was the first to draw attention to this disease of infancy, did not believe that it came on earlier than three or four months after birth. Dr

Gœdechens, however, relates a case in which an infant actually affected with this disease, was born of a sickly mother. It was the first of twins. The second born was strong and healthy.

The symptoms of the affection are profuse perspirations from the head, nocturnal restlessness, feebleness of the digestive apparatus, rachitic habit, slowness of development, and especially softening of the bones of the head.

In Dr Gœdechens's case, the parietal and occipital bones adjoining the lambdoidal suture were as soft as cartilage at the time of birth. The child died when six months old, and then the softening had spread over the whole of the parietal and occipital bones, except the very centre of the parietal, and the tuberosity of the occipital.

Subsequently the surviving twin was seized with the disease, but gradually improved under the use of iron internally, and astringent baths. — *Gazette Médicale*, 31st August 1850.

A CASE OF RUPTURE OF VARICOSE VEINS OF THE LABIUM PUDENDI. BY DR THOMSON.

This patient was in the ninth month of her pregnancy, and, while engaged in household duties on the 5th January, felt a large tumour, which she thought must be the child's head, bearing down. Placing her hand below, she felt a tearing sensation, and she became deluged in blood, so that the reporter believes she lost considerably more than a gallon. Upon examination, he found a laceration on the left of the median line, extending from within an inch of the anterior commissure of the labia to the upper part of the *mons veneris*, about three inches in length ; the soft parts on that side being greatly distended by the effusion of blood into the cellular tissue. The patient being in the recumbent posture, the hemorrhage ceased, the varicose veins of the extremity having become completely emptied, and the heart scarcely pulsating. As the bleeding had stopped, the case was left to nature. After the first week, camphor liniment was employed to disperse the large swelling produced by the extravasated blood, which entirely disappeared before the end of the second, so that the edges of the wound, which were at first three inches apart, were now in close contact. On the 20th January the fœtus was suddenly and easily expelled, and the case did well.—*New York Journ. of Med.*, vol. iv., p. 317 ; and *Brit. and For. Med. Chir. Rev.*, October 1850.

ON FUNGOUS TUMOUR OF THE RECTUM IN CHILDREN, ATTENDED WITH BLOODY DISCHARGES. BY M. LECLAYSE.

M. Martin has already directed attention to the affection as it occurs in the adult, producing discharges which are mistaken for those from hemorrhoids. The first case occurred in a child, æt. 5, about whom the author was consulted, in consequence of hemorrhages which occurred during a prolapsus ani, and which arose from an excrescence that he at first mistook for hemorrhoids. Examining it more closely, he found it was a spongy vegetation, not unlike a portion of the placenta, which protruded from beyond the sphincter when the child went to stool, and was quite insensible to the touch. As the hemorrhage had been considerable, the fungus was touched with the nitrate of silver, whenever it protruded ; and owing to its softness, four or five applications, at intervals of several hours, sufficed for its destruction. In a second case, a girl, æt. 8, had become much reduced by the quantity of blood she had lost during several weeks ; and a fungous tumour, about the size of an almond, was easily removed in the same way. A third case occurred in an infant six months old, in whom efforts at stool protruded a tumour the size of a pea, which bled. The author believing it to be the germ of the fungous tumour, also treated it with caustic.

M. Leclayse believes that this affection is often mistaken for hemorrhoids ; and especially when the bleedings are said to be due to internal piles. The caustic could not be applied very high up, but as the bleeding has only occurred on the protrusion of the tumour, this has been easily reached, the application being successful even when the base of the tumour could not be

attained.—*Rev. Medico-Chirurgicale*, tom. vii., p. 346; and *Brit. and For. Med. Chir. Rev.*, October 1850.

[This disease is fully described in Mr Syme's treatise on the Rectum, under the title of "Polypus of the Rectum."]

THE BIRTH OF ACRANIAL MONSTERS. BY DR PURPLE.

That labour should be more or less modified by acrania or pseudencephalous fœtuses, we should, *à priori*, naturally infer. Such, in fact, is the case. In those cases where the tumour on the head presents, the young practitioner might very easily be deceived; and should there take place, by any accident (which certainly is possible) rupture of the vessels of the vascular tumour, the hemorrhage might be easily confounded with a modified case of placenta previa. Should such an accident complicate the delivery of a pseudencephalous fœtus, we should not have such an amount of hemorrhage, as in the case of placenta previa; neither would it occur previous to the commencement of labour, as is the case in the latter.

There is another point which has been noticed by writers, and which should not escape our notice in this place, viz.,—the difficulty which the considerable volume of these fœtuses gives rise to. The bodies of almost all acrania or pseudencephalous fœtuses, which arrive at the full period of gestation, are more fully developed than the bodies of those which present no anomalies. The shoulders are broader, and their weight is greater, as a general rule. This fact must render labour much more protracted, as is illustrated in two of the cases reported. Hiliare, on this point, says:—"The considerable volume of the greater portion of pseudencephalous fœtuses renders labour very protracted; this fact will appear, if reference be made to the accurately made and recorded observations on this family of monsters, and such, also, is clearly seen in those cases in which I have been able to collect precise information, and in nearly all of which they were born a long time after the invasion of the first pains." Should circumstances arise in the course of labour which would indicate the application of forceps, it is reasonable to suppose, from the anatomical figure of the head, that more or less difficulty would be experienced in the extraction of the head, if not in the application. In the case which I have recorded, the breadth of the shoulders did not exceed those of a normal fœtus; this fact may account for the ease and rapidity of the labour.

The frequent occurrence of this family of monsters is mentioned by writers. Mr Wilde, in his work on Austria, published some six or eight years since, gives the statistics of monstrosities occurring in the Imperial Lying-in Hospital in Vienna (the Geberanstalt), for the eight years ending 31st of December 1840. Out of 23,222 births, there were 68 cases of monstrosities,—or 1 in 341·5 births; of the 68 cases, there was only 1 "wanting the superior part of the vertex,"—1 other is reported as "hemicephalus." From these tables, it would appear that they are by no means of frequent occurrence, as some authors have supposed.—*New York Journal of Medicine*, July 1850.

SECONDARY UTERINE HEMORRHAGE ON THE THIRTEENTH DAY AFTER DELIVERY. BY DR FERGUSSON.

The patient was twenty-four years of age, and had never born children. Her labour was comparatively an easy one of fifteen hours' duration, and no unusual symptoms presented themselves during her convalescence till the day above mentioned. For three or four days previous she had left her bed during a good portion of the day, and as the presence of the binder was unpleasant to her, she had removed it without my direction or knowledge. On the morning of the thirteenth day I was suddenly called to see her, and found her very much prostrated from loss of blood. I learned from her that during three hours blood had been escaping from the vagina, but she had avoided communicating the fact to any one, in the hope that the hemorrhage would cease spontaneously. The exact quantity of blood could not be accurately ascertained. All the clothing in the immediate neighbourhood of the nates was saturated

with blood, and large clots were lying below the vulva, from which I judged that the quantity lost was not far from two pints and a-half. The discharge had continued to increase since its commencement, and when I saw her it was still escaping quite rapidly. Her countenance was pale and anxious, and her pulse at the wrist exceedingly weak and frequent, it being one hundred and ten per minute. Upon examination, over the hypogastric region, the uterus was distinctly felt considerably larger and softer than is usual at this period, a circumstance which I attributed partially to the early removal of the binder, and partially to the probable existence of an internal hemorrhage previous to the discharge of blood from the vagina. Upon introducing a finger into the vagina, I found a clot lying in its cavity, and the os uteri sufficiently dilated to admit its extremity for a short distance. I immediately ordered a cold douche, followed by the application of a bladder of ice, over the lower part of the abdomen; administered ʒi. of the saturated tinct. of ergot, and enjoined absolute quiet in the recumbent position, and cold drinks. The effect of these agents was to produce a moderate contraction of the uterus, though the discharge of blood was not very materially diminished. I now made a re-application of the binder, with a thick compress over the region of the uterus, as firmly as could be borne by the patient, and ordered the administration every second hour of the following pill:—

℞. Pulv. Secal. Cornut., gr. iij.
Plumbi Acetatis, gr. ij.
Pulv. Opii, gr. ¼.
Muc. Gum. Acac. q. s. Fiat pilula.

Under this treatment she gradually improved, but as the hemorrhage still continued, though in smaller quantities, on the morning of the second day, I asked Dr Thos. F. Cock, one of the attending physicians of the asylum, to see the case with me, who recommended a continuation of the treatment which I adopted. In the evening, the bowels having been constipated for three days, and being unwilling to administer a cathartic in her present condition, I ordered an injection of cold water into the rectum, in order to effect the double object of an evacuation of the contents of the bowels, and, probably, aiding the uterus in its contraction. From this time, the intervals between the administration of the pills being gradually lengthened, the discharge steadily diminished, and on the evening of the third day entirely ceased. Nourishing diet was allowed the patient, her canvalescence was speedy, and without an untoward symptom, and she was discharged from the asylum in good health.

The occurrence of hemorrhage from the uterus several days after delivery is so unusual, and so little has been written concerning it, that I have considered this case worthy of particular description. I have examined several statistical tables with reference to this subject, and I have not been able to find any case in which hemorrhage occurred at so late a period. Among 16,654 cases of delivery in Dublin Lying-in Hospital, reported by Dr Collins, there were but 43 cases in which uterine hemorrhage occurred subsequent to the expulsion of the placenta. In 40, the discharge took place within twelve hours; in 1, on the fourth day; in 1, on the fifth; and in another, on the tenth day. Drs M'Clintock and Hardy report 6634 cases of delivery in the same hospital. 25 cases of secondary hemorrhage were recorded, 1 of which occurred on the seventh day. There was also reported from this asylum, by Dr Stimson, in N. Y. Annalist, of October 1st 1847, the case of a patient in whom hemorrhage took place on the tenth day subsequent to parturition.

Various causes have been assigned for the occurrence of this accident, but in this instance I think the hemorrhage is fairly attributable to the early removal of the binder, accompanied by premature exertion on the part of the patient, causing great excitement of the circulation.—*New York Journal of Medicine*, September 1850.

SUCCESS OF TAPPING IN OVARIAN DROPSY. BY DR ATLEE.

Dr Atlee denies the accuracy of Mr Safford Lee's statement of the danger of *tapping*, and especially (according to Dr Meigs) the first tapping. According to Mr Lee's own tables, of 123 cases treated in the ordinary way, 63 died in two years, and 90 within four years—leaving only 33, or not quite 1 in 4. Thus 51 per cent. died in two years, 73 per cent. in four years, and only 27 per cent. survived this period. If we examine the table of the mortality after a first tapping, in the 40 cases in which the whole duration of the disease is stated, it is found that 15 (or 39 per cent.) died in two years, 11 within four years (65 per cent.), while 14 (35 per cent.) lived beyond that time,—that is, nearly 36 per cent. in favour of tapping. Both Mr Lee and Dr Meigs employ the loose phraseology, that the women died *after* the tapping, as must necessarily be the case ; but the true question is, how far has the tapping been the cause of death ? The experience that Dr Atlee and his brother have had of this operation since 1823, and the numerous inquiries he has made of surgeons in large practice, convince him that death or even serious symptoms, never result from tapping, that life is usually prolonged, not curtailed, by resorting to it, while in several cases even permanent recovery has resulted.—*American Journal of Medical Science*, No. 38, *and Brit. and For. Med. Chir. Rev.*, October 1850.

REPORT ON ERGOT OF RYE. BY M. DANYAU.

On the 24th September, M. Danyau communicated to the Parisian Académie de Médecine the report of a large committee appointed to answer the following question proposed to the Académie by the Prefet de la Seine :—

" What are the possible effects of ergot of rye upon the life of the child and the health of the mother ?"

The following report of the commission was adopted :—

" 1. Ergot, whatever may be the advantages which belong to it as a medicine, may, when imprudently administered, occasion the death of the child, and lesions more or less grave to the mother.

" 2. In the present state of our legislation, it is impossible to withdraw from midwives their present legal right to administer ergot; and such a prohibition might, in certain cases, occasion serious inconvenience.

" 3. It is to be wished, that new laws, anxiously expected, may, while elevating the standard of instruction applicable to midwives, fix their duties in a more precise manner, and, if possible, assign due limits to the nature of the prescriptions which they may be authorised to employ."—*Revue Médico-Chirurgicale de Paris*, October 1850, p. 253.

[In the course of the discussion on the report, it was stated by M. Villermé, that the question of the Prefet was suggested, by the almost annual increase in the proportion of infants still-born in Paris.]

SPONTANEOUS EXPULSION OF A LARGE URINARY CALCULUS IN A FEMALE. BY M. BRIAULT.

A woman, æt. 50, for a long time affected with ovarian dropsy, complained to her physician of difficulty in making water, which he naturally referred to the presence of the hypogastric tumour. In consequence of the continuance of the complaint, M. Briault was led to make an examination of the parts. The catheter was introduced with difficulty, and caused some pain, but no suspicion was roused of the existence of a calculus in the bladder. On examining per vaginam, a tumour was felt through its anterior wall, resembling in some respects the enlarged prostate felt in the same situation in the male.

M. Briault returned on the following day to repeat his examination, when the patient informed him that she had passed the greater part of the night in acute pain, accompanied with expulsive efforts, like labour pains. These exertions ended in the expulsion of a large calculus. On again examining, M. Briault discovered the urethra in a state so distended, as easily to admit his finger, and allow of the examination of the vesical walls. The urine was

bloody, and passed involuntarily; but these symptoms disappeared in a few days. The calculus weighed somewhat more than an ounce; it was two inches long, and nearly three inches in circumference.—*L'Union Médicale*, September 28, 1850.

Part Fifth.

MEDICAL NEWS.

POPULAR PREJUDICE AGAINST VACCINATION.

We extract from the Registrar-General's second Quarterly Return for 1850, the following remarks upon this important subject :—

" Although vaccination is actively promoted by the Poor Law Board, is now performed at the public expense, and affords almost complete immunity from small-pox, it is still neglected to a great extent by the ignorant classes of society. According to an ancient and popular theory, the disease is the elaboration of some morbid principle from the system; which can only be delivered in this way from its influence. The objections to vaccination, grounded upon the theory are, however, now deprived of all their force by the able and satisfactory investigations of Mr Ceeley of Aylesbury. Mr Ceeley has proved beyond doubt that small-pox and cow-pox are essentially the same disease. He introduced the matter of small-pox taken from man, into the cow, and produced pustules, which supplied lymph, and in children raised pustules possessing the properties of cow-pox, and which have served since for all the purposes of vaccination. Cow-pox then is a modified small-pox; and removes any morbid matter from the system as effectually as the more severe, deforming, fatal type of malady. Some of the objections are excuses for negligence; others are based on a sort of fatalism; but such cases as the registrar of Nottingham records are criminal, and it is to be feared spring from the same cruelty as leads to the sacrifice of children's lives in other circumstances. A woman there who had lost a child by the disease, assured the registrar that 'she would rather lose half-a-dozen children by it than fly in the face of Providence, in having one vaccinated!' "

In the return from *St Ann's*, Nottingham, we find the following :—" In one house a family of eleven children were ill of small-pox at the same time; three died within the space of twenty-four hours. None of them had been vaccinated except two infants (twins), but even these had taken the complaint before vaccination or immediately after. It is perhaps right to add that, from observation and inquiry among medical men, it appears the cases of small-pox without previous vaccination have occurred exclusively amongst the lowest orders, and not amongst the middle or the educated portion of the working classes."

In the return from *Walcot*, Bath, out of eighty-five deaths fourteen are ascribed to small-pox :—" Small-pox made its appearance at the commencement of the present year, at the western extremity of Bath; it gradually extended itself over the city, and in the beginning of the present quarter reached this district. It is now subsiding; no death has occurred since the 14th June, and the district may now be considered in a very healthy condition. In my opinion the prevalence of this disease may be attributed in great part to the negligence of the lower classes in not having their children vaccinated, which arises entirely, I believe, from their being strongly prejudiced against the cow-pox. As one of the surgeons to the Western Dispensary of Bath (which provides gratuitous medical advice at their own houses to the poor of a certain limited district) many of the earliest cases fell under my own observation, but I only met with one case where the child had been previously vaccinated, and this was of a very slight description."

In *Lansdown*, Bath :—"Small-pox caused five deaths, which occurred in April. The mortality of small-pox is owing to the prejudice and ignorance of persons in not having their children vaccinated. Positive refusals are given to the public vaccinator. It is much to be regretted on the score of public good that vaccination is not compulsory, for small-pox will naturally prevail at different periods, from accumulation of unvaccinated children."

In *Batheaston*, Bath :—"Four deaths, all of children, occurred from small-pox, without previous vaccination—many persons refusing to have their children vaccinated. A man was committed to prison for inoculating his children with small-pox."

But in *East Budligh*, St Thomas, Devonshire, it seems, that of twenty-six deaths, eleven were caused by small-pox, "which has been propagated throughout the district (except Satterton) by persons, most violently opposed to vaccination, having undertaken to inoculate their own families and others, or expose them to the influence of the disease ; otherwise the deaths would have been much below the average of previous quarters."

[Where the force of reason fails in overcoming mischievous prejudices, it would be well that statutory penalties were established and enforced. The experience of some of our continental neighbours, who have wisely made vaccination compulsory upon all citizens, may be appealed to, in vindication of the policy, as well as the humanity, of affording legal protection against such popular delusions as still disgrace the lower orders in certain localities of this kingdom.]

OBSERVATIONS ON THE FREEZING OF THE ALBUMEN OF EGGS.

BY JAMES PAGET, ESQ.

The object of this paper is to illustrate a peculiar property of the albumen of the eggs of birds, a property which seems to have its purpose in preserving them from the injurious effects of very low temperatures.

Mr Hunter observed that a fresh egg will resist freezing longer than one which has been previously frozen and thawed ; and he referred this fact to the "vital power" of the egg in the first case, and the destruction of that power by freezing in the second. The author's experiments confirm those of Mr Hunter, and prove, also, that when fresh eggs are exposed to very low temperatures, and also in the case of eggs which are decayed, or putrid, or the contents of which have been much altered by mechanical force or by electricity, a shorter time is sufficient for the freezing of such eggs than is necessary for the freezing of those which are uninjured.

An examination of the rates at which heat was lost by the several eggs exposed to temperatures varying from zero to 10° Fahr., showed that fresh eggs, though they resist freezing longer than any others, yet lose heat more quickly, and that their resistance to freezing is due to the peculiar property of their albumen, the temperature of which may be reduced to 16° Fahr., or much lower, without freezing, although its proper freezing-point is at or just below 32°. Other than fresh eggs lose heat comparatively slowly, but freeze as soon as their temperature is reduced to 32° ; fresh eggs lose heat more quickly, but may be reduced to 16° or lower ; then, at the instant of beginning to freeze, their temperature rises to 32°.

That this peculiarity of fresh eggs is not due to vital properties is proved by experiments, which show that certain injuries, such as mechanical violence, addition of water, and others, which spoil their powers of resisting freezing, do not prevent eggs from being developed in incubation. By the same and other experiments, which are related, it is made probable that the peculiarity depends on the mechanical properties of the albumen ; for whatever makes the albumen more liquid than it is naturally in the fresh egg, destroys the power of resisting freezing.

The author could find no other substance possessing this property ; and in evidence of its adaptation to the purpose of preserving eggs from the loss of their capacity of development, which they would suffer in being frozen, he

relates experiments in which eggs were kept for a considerable time at temperatures ranging from zero to 10° Fahr., yet were afterwards developed in incubation. By the same series of experiments, it was shown, that although freezing renders the effectual development of the germ impossible, yet the intensest cold, if freezing does not take place, has no similar result.—*Pharmaceutical Journal*, from *Medical Gazette*.

MEDICAL GRADUATION.

The following resolution of the Medical Faculty of the University of Edinburgh was alluded to at page 435 of our November number. It has since been communicated to us for publication :—

"The Medical Faculty have resolved that, in future, candidates for graduation shall be allowed to take their Latin examination at the beginning of the winter session, in any year of their study ; and that they shall also be allowed to take their examination on Botany and Natural History at the same time, if properly qualified to do so by attendance on these classes.

"Students who wish to avail themselves of this permission during the present session, are requested to send their names and addresses to the Dean of the Medical Faculty before the 15th November.

"*College, Edinburgh, 23d Oct.* 1850. J. H. Balfour, *Dean.*"

VARIETIES.

Tribute to the Memory of Mr Liston !—The lately appointed assistant-surgeon of University College Hospital, Mr Cadge, in order to vindicate "the memory of his friend and teacher from the imputation of vain-glory and presumption," tells us that in the "*latter years*" of his practice (namely, when he was labouring under a fatal disease, which, though not detected until revealed by examination after death, must have exerted an influence sufficient to paralyse the exertions of most men), he occasionally failed in passing instruments through strictures of the urethra, and then resorted to incision of the perineum. This information will be satisfactory to those who, despairing of excellence, are always glad to get an apology for mediocrity. But we much fear that the fortunes of University College will hardly be retrieved through such principles.

Exile to Erin.—The "Dublin Medical Press" complains that no students are *sent* to their school from Edinburgh. It may console our testy cotemporary to know, that, if the teachers here had ever so much desire to comply with his wishes, the students would not go. The *vis-a-tergo* may be an excellent argument in elementary education, but it is on the power of *attraction* that a professional school must depend for its prosperity.

Hydrophobia.—The official bulletin of the chief council of health at Turin, is full of cases of hydrophobia,—among others, some produced by the bites of foxes and cats. Unfortunately, the importance of a prophylactic treatment by means of caustic is little understood, even among the clergy. The curé of Perouse dissuaded persons who were bit from cauterising their wounds, and advised a pilgrimage to the Madonna di Cantagno. One of the unfortunate persons who followed this advice died of the disease.—*L'Union Médicale*, July 25, 1850. [We regret to announce that another fatal case of hydrophobia has recently occurred in our own neighbourhood, in the practice of Dr James Struthers. The case was communicated to the Medico-Chirurgical Society, and will appear in this Journal.]

Poisoning.—A return has been printed by order of the House of Commons, of the number of persons, male and female, tried in the United Kingdom for murder, and attempts to murder, by the administration of poison, from the year 1839 to the year 1849, both inclusive. The number of persons so tried in England and Wales, during the ten years, was 154—namely, 69 males and 85 females ; the number of convictions on either charge was 66. In Scotland

the trials for murder by poison, since 1839, have been only 9—2 males and 7 females; the convictions were 3. The trials for attempts to murder were 6—3 males and 3 females. Total trials in Scotland, 15; total convictions, 7. In Ireland the trials amounted to 56—25 males and 31 females; and the convictions were 13. In 1839 there occurred only one conviction in Ireland for murder by poisoning; in 1841 there were 5 convictions found against 10 persons accused. In 1849 the number of indictments was 13—7 males and 6 females; and the convictions 3.

A New Metal—Aridium.—A report read before the Académie des Sciences of Stockholm, announces that M. Ulgren has discovered a new metal, which has received the name of *aridium*. This substance has been found in the chromate of iron of Reoras. Its oxides are analogous to those of iron, but may be distinguished. Thus, with prussiate of potash, a solution of the protoxide of the new substance gives, like iron, a *dark blue precipitate;* but it passes into a *dirty green* on the addition of an excess of the prussiate. Aridium has not yet been obtained in the metallic state.—*L'Union Médicale.*

Glanders in the Human Subject.—A well marked case of this disease was recently treated in one of the clinical wards of the Infirmary. The patient died, and a careful *post-mortem* examination of the body was made. The details of the case will probably be published in our January Number.

Royal Infirmary.—Recent vacancies in the staff of the Surgical Hospital, caused by the retirement of Dr Douglas Maclagan from the post of surgeon, have been filled up by the appointments of Dr Richard Mackenzie as surgeon, and of Dr J. D. Gillespie as an assistant-surgeon, to the institution.

PUBLICATIONS RECEIVED.

A Course of Lectures on General Pathology. By John Simon, F.R.S., &c. London, 1850.

On the Identity or Non-Identity of the Specific Cause of Typhoid, Typhus, and Relapsing Fever. By Wm. Jenner, M.D., &c. London, 1850.

On the Identity or Non-Identity of the Specific Cause of Typhoid, Typhus, and Relapsing Fever. By William Jenner, M.D. —(From vol. xxxiii. of the Medico-Chirurgical Transactions.) London, 1850.

Report of the Proceedings of the Pathological Society of London. Fourth Session, 1849-50. London, 1850.

Trial of Alexander Hain before the Sheriff of Fife.—Case of Alleged Insanity. Cupar-Fife, 1850.

Chambers's Educational Courses. — Chemistry. By Dr George Wilson. Edinburgh, 1850.

Brighton and its Sanative Resources. By Edwin Lee, author of the Baths of Germany, &c. London, 1850.

On the Containing Texture of the Blood. By William Addison, M.D., F.R.S., &c.—(From London Medical Gazette.) 1850.

Der Kriegs und Friedens-Typhus in den Arméen. Ein Bietrag zu einer künftigen Gesundheitspflege in den Kriegsheeren. Von Dr C. F. Riecke. Regimentsarzt im Königl. Preuss. Cadetten Corps. Nordhausen, 1850. Erster Theil.

Die Asiatische Cholera und die Gesundheitspflege. Von Dr C. F. Riecke. Nordhausen, 1850. Zweiter Thiel.

The Edinburgh University Calendar, and Student's Guide. Edinburgh: Maclachlan and Stewart. 1850.

Conspectus Medicinæ Theoreticæ. Auctore Jacobo Gregory, M.D. Editio Undecima, Omni Parte Perfecta, Prioribus Emendatior. Edinburgi: Apud Maclachlan and Stewart. 1850.

Compte Rendu des Travaux de la Commission de Souscription pour le Monument de Larrey. A Paris, 1850.

Discourse on the Method of Rightly Conducting the Reason and Seeking Truth in the Sciences. By Descartes. Translated from the French, with an Introduction. Edinburgh, 1850.

Influence of Physical Agents on the Development of the Tadpole, of the Triton, and the Frog. By John Higginbottom, Hon. Fellow of the Royal College of Surgeons of England. — (From the Philosophical Transactions.) London, 1850.

Dr Warren's Address before the American Medical Association at Cincinnati. Boston, 1850.

On the Therapeutic Application of Electro-Magnetism. By Dr Froriep. (Translated from the German by Richard Moore Lawrance, M.D.) London, 1850.

Report on the Medical Literature of the United States. Presented to the American Med. Association, by the Standing Committee on Med. Literature. Philadelphia, 1850.

Guy's Hospital Reports. Second Series, vol. vii., part I. London, 1850.

Pathology of the Human Eye. By John Dalrymple, F.R.C.S. Fasciculi 5 and 6. London, 1850.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Fasciculi 6 and 7. London, 1850.

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